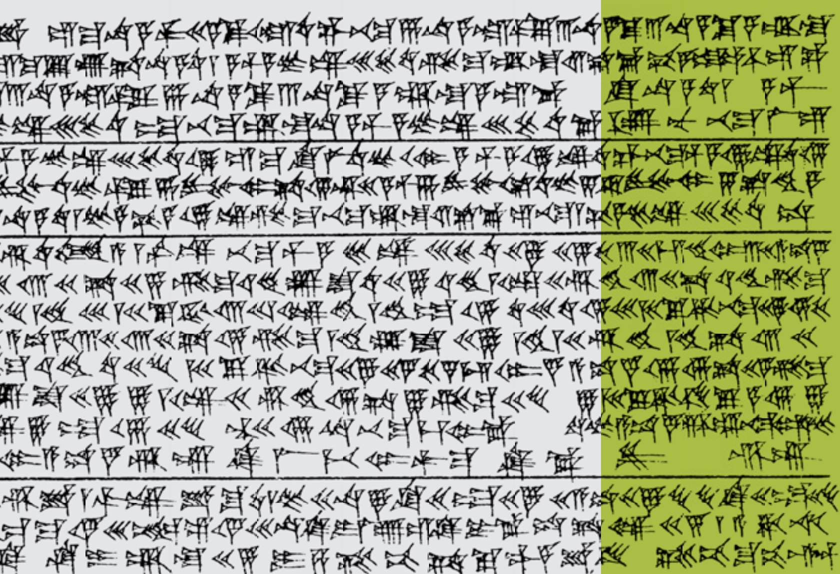
# Studies on the Ancient Exact Sciences in Honor of Lis Brack-Bernsen

John M. Steele
Mathieu Ossendrijver
(eds.)



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THE ANCIENT EXACT SCIENCES are the main subject in this collection of papers, offered in honor of Lis Brack-Bernsen by her colleagues and friends. The topics of the articles are linked by the themes that have been at the center of much of Lis's own work: the Babylonian observational record, and the relationship between observation and theory; the gnomon, sundials, and time measurement; and the relationship between different scientific activities in the ancient world, especially the connections between mathematics and astronomy. Lis Brack-Bernsen has been a key figure in transforming the study of Babylonian astronomy from an almost exclusive focus on the mathematical astronomy of the late period to embracing a much broader consideration of all aspects of the subject, both early and late, mathematical and observational, astronomical and astrological, and their relationships between one another. The papers demonstrate the wide variety of questions asked and approaches used by historians of ancient science.

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John M. Steele

# Introduction

The field of Babylonian astronomy has been transformed over the past three decades, changing from an almost exclusive focus on the mathematical astronomy of the late period (generally known as 'ACT' astronomy after the acronym of Otto Neugebauer's classic *Astronomical Cuneiform Texts* published in 1955) to embracing a much broader consideration of all aspects of the subject, both early and late, mathematical and observational, astronomical and astrological, and their relationships between one another. Lis Brack-Bernsen has been a key figure in turning the study of Babylonian astronomy into what it is today.

Among Lis' many contributions to the study of Babylonian astronomy, two have been particularly significant in shaping the way that research in the field has progressed. The first is by drawing attention to the so-called 'lunar six' time intervals – measurements of the time between the moon and sun crossing the horizon on six specific occasions during a month – which were regularly observed in Babylon from at least as early as the middle of the seventh century BC. In a series of papers, Lis has explored the role of lunar six observations and the development of the ACT lunar systems, the first attempt to answer the question of the relation of observation and theory in Babylonian astronomy, and, even more significantly, uncovered a Babylonian method of using past lunar six observations to predict future lunar six intervals which was completely unknown to modern scholars and whose discovery has opened up a whole new area of research. Almost as important as her own work in these areas has been Lis's second transformative contribution to the study of Babylonian astronomy: the foundation of the so-called 'Regensburg' series workshops on Babylonian astronomy which have brought together specialists for intensive, detailed, and collegial discussion of different aspects of Babylonian astronomy. The first Regensburg workshop, held in Regensburg in 2002, was so successful that is has been followed by 'Regensburg' workshops in Amsterdam (2004),

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Durham (2008), and Berlin (2014), with discussions already underway for the next in the series. It is no exaggeration to say that these workshops have significantly influenced the direction of research on Babylonian astronomy over the past decade.

In addition to her work on the lunar six, Lis has made significant contributions to many other aspects of the study of Babylonian astronomy including deepening our understanding of the early astronomical compendium MUL.APIN, and research on the gnomon and the origin of the zodiac, the operation of the Babylonian calendar, and the mathematical astrological schemes known as the *Kalendertexte* and *dodecatemoria* schemes. Outside of Babylonian astronomy, she has also published important works on Babylonian mathematics and, earlier in her career, on Mayan astronomy.

The papers in this collection are offered in honor of Lis Brack-Bernsen by her colleagues and friends, including many of the participants in the Regensburg workshops. The topics of the articles are linked by the themes that have been at the center of much of Lis's own work: the Babylonian observational record, and the relationship between observation and theory; the gnomon, sundials, and time measurement; and the relationship between different scientific activities in the ancient world, especially the connections between mathematics and astronomy.

A tradition of regular and precise observation lies at the heart of Babylonian astronomy. One of the most common types of observation recorded by the Babylonian astronomers is of the position of the moon and the planets relative to a group of reference starts. In their paper Gerd Graßhoff and Erich Wenger analyze Babylonian observations of this kind in order to understand the coordinate system underlying these measurements, demonstrating that positions were measured parallel and perpendicular to the ecliptic. The earliest systematic records of Babylonian observations date to the eighth and seventh centuries BC. John Steele discusses one of the earliest records of planetary observations, a compilation of reports of Mars's synodic phenomena dating to the reign of Nebuchadnezzar II. Using Lis Brack-Bernsen's work on the lunar six as a starting point, Teije de Jong discusses the development of Babylonian lunar theories from observations of the lunar six and eclipses. Peter Huber analyzes records of the length of the month and their application to the problem of Old Babylonian chronology. Mathieu Ossendrijver edits and discusses a text which describes methods of predicting planetary conjunctions from past observations, using the same principle of 'goal-year' astronomy as Lis Brack-Bernsen uncovered for the lunar six.

The gnomon and the sundial were used across the ancient world to measure time and to study the daily and yearly motion of the sun. Elisabeth Rinner analyzes the mathematics of conical sundials, one of the most common types of Greco-Roman sundial, and its connection with theories of conic sections. Alexander Jones's paper complements Rinner's by providing an analysis of spherical sundials and the geometry of curves. Also related to time measurement, Hermann Hunger reedits (with the addition of a substantial new fragment to the tablet) and analyzes a Neo-Assyrian text which has previously been thought to concern seasonal hours, demonstrating that this is not the case.

The relationship between different types of ancient science is explored in the final three papers. Jens Høyrup's paper examines the various forms and places of mathematics and mathematicians in the ancient world. Wayne Horowitz and John Steele examine a peculiar cuneiform tablet which combines numbers with star names. Finally, Francesca Rochberg studies the relationship between astronomy and divination, and in particular the idea of norms and deviations from those norms in Babylonian divinatory traditions.

Together, the papers in this volume present a snapshot of research into the ancient exact sciences. They demonstrate the wide variety of questions asked and approaches used by historians of ancient science, and comprise, as we hope, a fitting tribute to Lis Brack-Bernsen's groundbreaking work in this field.

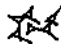
The editors wish to express their thanks for Alex Schwinger for his assistance in the production of this volume, and the Excellence Cluster Topoi for publishing this volume in the series *Berlin Studies of the Ancient World*. We also remember Norbert A. Roughton, who contributed the foreword to this volume, but sadly passed away before its publication. Roughton had participated in all four of the 'Regensburg' workshops and will be sorely missed at future workshops.

Norbert A. Roughton

# Foreword: An Essay in Story Form, Honoring Babylonian Astronomers and Connecting Them with Scholars Who Study the Tablets Today

Once, long ago, in a land where the night sky blazed with stories, a young boy struggled to learn the secrets of the stars. Oh, he knew Leo the lion and Pisces the fish and that he had been born under the sign of Taurus the bull, but he wanted to know all the signs and the stars and their travels.

His father and grandfather before him had written and studied the royal tablets to find the rhythms in the movement of the skies. While other boys practiced archery with their bows or played the game of stones, he worked – sometimes in the sand, sometimes on discarded bits of clay, sometimes on his practice tablet – drawing the lines that made the names of the zodiac.

His own, Taurus,  he already knew. His mother was Aquarius  and his father  Leo. His brother's Scorpio was going to take more practice.

"You will learn", reassured his mother as she handed him a ball of dough to flatten and mark his name before she baked it. "You need to be patient like the bull who knows how to wait."

"You're too young to stay up late", teased his older brother as he gathered his stylus and clay to take to scribal school where he was learning to copy the stories sung by the priests at the ritual feasts.

Later, when Father came home, he smiled and tousled the boy's hair. At the meal table he looked at the boy, saying, "Tonight you will go with me. If your grandfather's figures are correct, we will observe and record again the great wandering star's travel across the sky. It will be a good omen for the king."

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The boy returned his father's smile with a grin and a smug look to his brother. "Bring your cloak", said his father, "along with your practice tablet and a keen eye. The night will be cool and we may have to wait and watch until dawn".

Thus it was that the boy began his studies beside his father, learning to copy what others before had predicted, to record the positions of the moving stars in relation to the stationary normal stars, and to figure when the patterns would occur again. Sometimes he would start and have to smooth the clay to start again. His father checked his work until the day his planet names were correct:

Jupiter Venus Mercury Saturn Mars

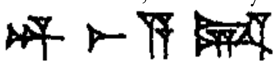
He was even able to accompany his father and others on a journey to Uruk to consult with the astronomers there about a particularly puzzling star formation. They were greeted with courtesy, offered cool refreshments, and escorted to the chambers where the observers had laid out their tablets for consultation. For two days they pored over the figures in question. On the next three nights they studied the sky: arguing about possibilities, clarity of terms, and corrections in calculations.

When they had finally come to an agreement, the two groups exchanged copies of the tablets that contained the work they had agreed upon. Father told him the information would be transferred to almanacs – those collections of findings recorded on the observational tablets which were accumulated and stored for reference to predict and verify the positions of the planets during the months of the year.

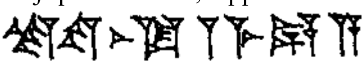
Under the watchful eye of his father, the boy became skilled in using his stylus to write and record the positions of the planets as they crossed the territories of the zodiac.

Venus in Pisces, First Visibility

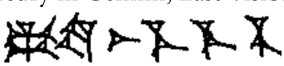
Mars in Leo, Stationary



Jupiter in Aries, Opposition



Mercury in Gemini, Last Visibility



Mars Leo Reaches

With time, study, practice, and encouragement, as the boy grew he was able to join the ranks of the scribes with the solemn tasks included in their mission as astronomers in the king's court. Their clay tablets, carefully recorded and stored, lay hidden in rubble and dust for centuries waiting to reveal their findings to curious minds.

Today we wonder, observe, study, and wonder again – finding our own way back to the early astronomers who also wondered and wrote their observations of the mysteries of the sky. As we study the tablets, we reach across the years and meet the challenges faced by our ancestors, marveling with them at the elegance of the universe.

Many thanks to Lis Brack-Bernsen for organizing the first Regensburg meeting of researchers of Babylonian astronomical tablets. Lis led the way for us modern astronomers to converse, comment, collaborate and continue to share our works with the ancient cuneiform tablets.

The following tablet, LBAT 1591, is a brilliant example of a school tablet for early scribes learning to record astronomical events (Fig. 1). A practice tablet for modern scholars beginning to translate ancient texts. The cuneiform lines used in this essay were taken from that tablet.

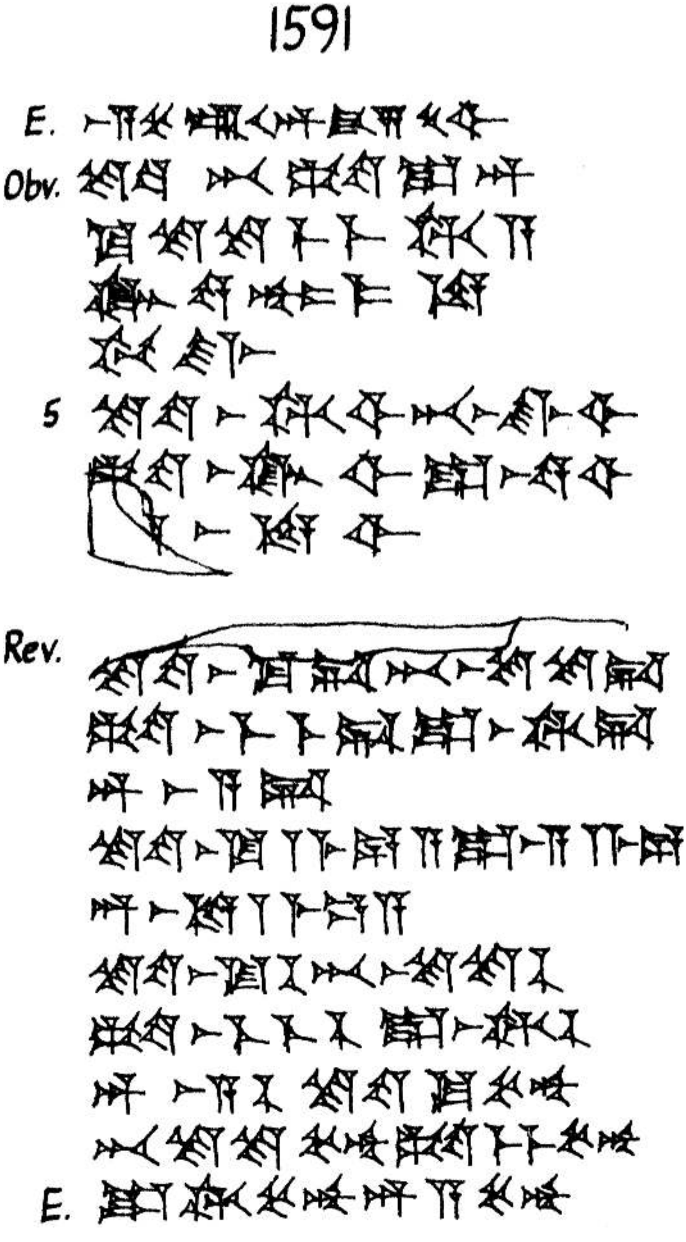



Fig. 1 LBAT 1591.

## Illustration credits

**1** Copy no. 1591, in: *Late Babylonian Astronomical and Related Texts*. [LBAT.] Copied by T. G. Pinches and J. N. Strassmaier, prepared for publication by A. J. Sachs, with J. Schaumberger. Brown University Studies 18. Providence, RI: Brown University Press, 1955, p. 256.

### NORBERT A. ROUGHTON

passed away on Saturday, January 14, 2017, at the age of 79.
Roughton (B.S., M.S., John Carroll University; Ph.D., Saint Louis University) was Professor Emeritus of Physics and former Chairman of the Physics Department at Regis University in Denver, CO. At Regis University, his teaching covered Physics, Astronomy, History of Science and Computer Science. While his early scientific interests were located in the area of Experimental Nuclear Astrophysics, his research since the 1980s focused on Babylonian Astronomical Texts, and he was a regular participant in the Regensburg workshops on Babylonian astronomy.

# BABYLONIAN OBSERVATIONAL AND THEORETICAL ASTRONOMY

Peter J. Huber

# Dating by Month-Lengths Revisited


## Summary

The chronological implications of the month-length evidence are re-examined on the basis of additional data, and newer astronomical theories and insights about the clock-time correction. The month-length evidence available by 2013 is internally consistent, and it confirms the former conclusions of 1982, although with slightly lowered confidence. It favors the High and disfavors the Middle chronologies with confidence levels between 95% and 99%. A Bayesian argument intimates that the High chronology (Ammiṣaduqa year 1 = 1702 BC) is roughly 25 times more probable than each of the other three main chronologies (1646, 1638, or 1582 BC). Independently, also the Ur III evidence points toward a High chronology (Amar-Sin year 1 = 2094 BC).


Keywords: Near Eastern Chronology; month-length dating; Venus Tablet; Ammiṣaduqa intercalations; clock-time correction.

Die chronologischen Implikationen der Belege für Monatslängen werden in diesem Beitrag anhand von zusätzlichem Material sowie von neueren astronomischen Theorien und Erkenntnissen über die Zeitkorrektur nachgeprüft. Die 2013 zur Verfügung stehenden Belege für Monatslängen sind in sich stimmig und bestätigen die Schlussfolgerungen von 1982, wenn auch mit etwas niedrigerer Konfidenz. Bei einem Konfidenzniveau zwischen 95 % und 99 % wird die lange Chronologie zu Ungunsten der mittleren Chronologien favorisiert. Ein Bayessches Argument verdeutlicht, dass die lange Chronologie (Ammiṣaduqa Jahr 1 = 1702 v. Chr.) ungefähr 25-mal wahrscheinlicher ist als jede der anderen drei Chronologien (1646, 1638 oder 1582 v. Chr.). Davon unabhängig deuten auch die Ur-III-zeitlichen Belege auf die lange Chronologie hin (Amar-Sin Jahr 1 = 2094 v. Chr.)

Keywords: Chronologie Altvorderasiens; Monatslängen; Venus-Tafel; Ammiṣaduqa-Interkalation; Zeitkorrektur.

I appreciate the help obtained, directly or indirectly, from several colleagues through discussions and constructive criticism. Foremost among them I should mention Michael Roaf.

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The suggestion to explore small prior probabilities for the additional intercalation (Section 5.2.3) is due to Jane Galbraith. Of course, all opinions and errors are my own.

## 1 Introduction

Month-length dating forms part of a tangled tale concerned with fixing the absolute chronology of the ancient Near East. This tale is based on evidence from history (back-reckoning using king lists, eponym lists, synchronisms, ...), archaeology (stratigraphy, pottery, ...), and natural science (C14-dating, dendro-chronology, volcanic activity, ...), including astronomy (Venus Tablet, solar and lunar eclipses, month-lengths). The internal relative chronology of the period in question, which ranges from the late third to the mid-second millennium, that is from the beginning of the Third Dynasty of Ur to the end of the First Dynasty of Babylon, is now agreed upon to within very few years, but its absolute position still is in doubt, and the disputes shift it forth and back over roughly 150 years. While the present paper concentrates on month-length dating, by necessity it must touch on some of the other parts also.[1]

The last comprehensive treatment of the month-length evidence has been that by Huber et al. in *Astronomical Dating of Babylon I and Ur III* (published in 1982),[2] followed by Huber's somewhat cursory re-takes and updates, spreading from 1987 to 2012.[3] These papers had reached the conclusion that the month-length evidence overwhelmingly favored the High Venus chronology (HC, Ammiṣaduqa year 1 = 1702 BC).

The current re-examination has been triggered by the recent flurry of activity concerning the Old Assyrian eponym lists and the dendro-chronological dating of the Kültepe site. This activity has collected strong, and as it seems, equally overwhelming evidence in favor of the so-called Middle Venus chronologies. Barjamovic, Hertel, and Larsen, in a comprehensive monograph published in 2012, have settled on the traditional Middle Chronology (MC, Ammiṣaduqa year 1 = 1646 BC).[4] De Jong (in a paper published in 2013) and Nahm (in a paper published in 2014) argue in favor of the Low Middle Chronology (LMC, Ammiṣaduqa year 1 = 1638 BC).[5] Roaf (in a paper published in 2012) favors the Middle chronologies but advises caution.[6]

1 Note: The paper was written in early 2014 and is based on materials available by 2013.

2 Huber, Sachs, et al. 1982.

3 Huber 1987; Huber 1999/2000; Huber 2000; Huber 2012.

4 Barjamovic, Hertel, and Larsen 2012.

5 De Jong 2013; Nahm 2014.

6 Roaf 2012.

In view of this seemingly irreconcilable conflict it is worthwhile – and perhaps even mandatory – to re-examine the month-length evidence with the help of the currently available material: (i) a moderately increased data base, (ii) more modern astronomical theories, and (iii) a better insight into the clock-time correction. I shall concentrate on the methodological aspects, in order to check and possibly identify weak spots of the arguments.

New OB material has been supplied by Seth Richardson, new Drehem material by Robert Whiting, and I am offering heartfelt thanks to both. With regard to methodological aspects it is relevant to note that (i) it does not suffice to scan the electronic text catalogs for intercalations and day-30 dates – it is absolutely necessary to examine the cuneiform sources in detail and to rely on the judgment of specialists, and (ii) that more data do not necessarily imply improved chronological discrimination.

The Babylonian months are based on a lunar calendar, and their length alternates irregularly between 29 and 30 days. The Babylonian day began at sunset, and the Babylonian month began with the first visibility of the lunar crescent in the evening. According to Babylonian custom, immediately after sunset of day 29, day 30 would begin in any case. But if the moon became visible shortly thereafter, that is some 20–30 minutes after sunset, the day would be denoted 'returned' (Akkadian *turru*) to become day 1 of the following month (that is, the date would be changed *retroactively*, with the retroaction spanning some 30 minutes). The preceding month thus would become hollow (29 days). Otherwise the day would be 'confirmed' (*kunnu*) or 'rendered complete' (*šullumu*); see the Akkadian dictionaries for these verbs, and in particular Neugebauer's translation and commentary of ACT No. 200 Sect. 15 for the technical use of the terms in mathematical astronomy,[7] and the letter BM 61719 (CT 22, No. 167), where the writer asks for speedy information whether the day is *kunnu* or *turru*.

There are no intervals of 28 days between two calculated crescent sightings, and only rare intervals of 31 days (about once in a century, and therefore statistically irrelevant). It seems that a Babylonian day 30 always was followed by day 1, whether or not the crescent was sighted. Since the synodic month has 29.53 days, one expects that 53% of the months have 30 days and 47% 29 days. For randomly selected (wrong) chronologies we therefore expect an agreement rate of 53% between calculated and observed 30-day months, and 47% for 29-day months. These rates for wrong chronologies are based solely on astronomical theory. For a correct chronology the evidence from Neo-Babylonian administrative texts (mostly texts dated on day 30) gives an agreement rate with modern calculation of 67% (103 of 153 attestations). Actually, I find it more convenient to work with expected miss rates; for 30-day months these are 47% for a wrong, 33% for a correct chronology. If the data set contains also attestations of a few 29-day months, the miss rate for random wrong chronologies must be minimally adjusted upward.

7 O. E. Neugebauer 1955, 206.

The figure of 33% applicable to a correct chronology is an empirical estimate and as such is affected by a standard estimation error of about 3.8 percentage points. Apart from this estimation error we do not know for certain whether the NB miss rate is applicable also to OB and Ur III times. The data sets are not exactly comparable; NB and OB evidence mostly is from texts dated on day 30, while a substantial fraction of the Ur III evidence also derives from other, and possibly more reliable data (e.g. from regular deliveries: one sheep per day for the dogs of Gula).

The principal criticism voiced against month-length dating seems to be that it has not been proved that the Neo-Babylonian 33% rate for correct chronologies is applicable to Old-Babylonian and Ur III data. This criticism is beside the point. The central argument showing that a certain chronology is right (thereby simultaneously establishing that its competitors are wrong) consists in showing that the miss count of that chronology is significantly below that to be expected from a wrong chronology. This argument relies only on the theoretically secure rate of 47%. If the miss rate is not significantly below 47%, we simply shall be unable to reach a conclusion. The 33% rate is used only in an ancillary fashion, namely to add evidence that a certain chronology is wrong. I hope to clarify these issues in the discussion of the Ammiṣaduqa-Ammiditana data.

The Venus Tablet remains a central part of the evidence.[8] The paper by Nahm (published in 2014) contains a most recent, comprehensive discussion.[9] In view of the agreement of the pattern of intercalations with that of contemporary Old Babylonian texts we now know for sure that the first 17 years of the Venus Tablet correspond to the first 17 years of the Old Babylonian king Ammiṣaduqa (see Section 10.1 in the Appendix of this article). Moreover, we now know that we have the complete pattern of intercalations for those 17 years (more precisely: we know all intercalations contained in the interval from year 1 month VII to year 18 month VI), and in particular we know the exact distances between the months of that interval. Note that we do not know for sure whether year 1 is normal or whether it contains a second Ulūlu ($VI_2$). This uncertainty is of some relevance in connection with the Ammiditana date (see Section 5.2).

I believe that only the four main Venus chronologies (Ammiṣaduqa year 1 = –1701, –1645, –1637, –1581) have a realistic chance of being correct.[10] We distinguish them as High (HC), (High) Middle (MC), Low Middle (LMC), and Low Chronology (LC). This assertion in part is based on the Venus Tablet evidence and in part on the historical time window now considered to be feasible. Among the other chronologies that have been entered into the discussion in recent years, the Gasche-Gurzadyan chronology (year 1 = –1549) is incompatible with the lunar calendar, and the Mebert chronology (year 1 =

8 Reiner and Pingree 1975.

9 Nahm 2014.

10 I am using the astronomical year count, which differs by one year from the historical count – in the latter, the year 1 BC is followed by AD 1.

–1573) relies on some demonstrably wrong assumptions about the arcus visionis values and on some questionable textual emendations.[11]

Current evidence centered on dendro-chronology and Assyrian eponym lists points toward the Low Middle chronology (year 1 = –1637). On the other hand, for the middle two chronologies the Venus phenomena show statistically significant deviations of ±2 days against calculation, on a 1% significance level. In particular, for the Low Middle chronology and all four events the observations on average are about 2 days later than calculated, while for the High Middle chronology they are correspondingly earlier.[12] This holds if the Old Babylonian observing and recording practices were basically the same as the Late Babylonian ones. We do not know for sure how the Babylonian astronomers dealt with adverse weather conditions. Ordinarily, the LB observers inserted educated guesses for absent observations, possibly based on observations made one Venus period (8 years) earlier, with the remark 'not observed' (NU PAP). The OB observers might have used a more naïve approach, possibly causing a systematic shift. Werner Nahm hypothetically suggests that they might have written down the first date on which they could confirm that Venus had entered a new phase, either visibility or invisibility.[13] If so, bad weather would delay the observed phenomena, but his suggestion does not convince me. An even more simple-minded approach based on *actually observed* first and last visibilities seems to me at least as plausible. With this approach, bad weather would have symmetric effects, on average mutually canceling each other: it would not only delay first visibility, but also lead to an earlier begin of invisibility. I shall keep all four main chronologies as possibilities, since – as always with delicate data analytic arguments – there is a non-negligible residual risk of error. But in my opinion it is small enough to cast serious doubts on the middle chronologies.

## 2 Calculation of crescents

Theoretical crescent visibility shall be determined according to a recipe described by P. V. Neugebauer.[14] The position of the moon is calculated at the time of sunset or sunrise (more precisely: when the center of the sun is in the mathematical horizon), ignoring parallax and refraction. For these calculations I used the programs by Chapront-Touzé and Chapront (published in 1991),[15] but with improved values for the clock-time correction ΔT and the lunar orbital acceleration.

11 Gasche et al. 1998; Mebert 2010; Huber 2000; Huber 2011.

12 See the row with the medians in Tab. 2.2 of Huber 2000.

13 Nahm 2014.

14 P. V. Neugebauer 1929.

15 Chapront-Touzé and Chapront 1991.

The lunar crescent then is supposed to be visible shortly after sunset if the altitude $h_{\text{moon}}$ of the moon at sunset exceeds a certain value $h$ (the thin lunar crescent is not visible at the moment of sunset or sunrise itself). The critical value of $h$ depends on the difference $\Delta$ in azimuth between sun and moon and $h$ has been determined empirically. Thus the crescent is assumed to become visible on the first evening for which the altitude difference

$$\Delta h = h_{\text{moon}} - h$$

is greater or equal zero (and to become invisible on the first morning for which this difference is less or equal zero). The tables for the critical value $h$ given by P. V. Neugebauer,[16] and shortly before by Langdon, Fotheringham, and Schoch,[17] differ slightly. Both tables go back to Carl Schoch. Identifying the tables by the initials of the authors, I am following PVN, while Parker and Dubberstein[18] followed the earlier LFS version. See Tab. 1 and Fig. 1.

This method for calculating crescent visibility admittedly is dated. Its advantage is that it has been extensively tested against antique data (see the next section). There are more modern approaches by Schaefer and others, but in the absence of testing it is not known how well they perform with regard to observations made before the industrial revolution. Of course, the critical altitude $h$ is not meant as a sharp limit, and the following section gives empirical evidence for the size of its uncertainty range.

In 1982 I calculated all 33 000 lunar crescents for Babylon (44.5 E and 32.5 N) between the years –2456 and +212. The following statistics may be of some interest. I am quoting the results of 1982; more modern programs and different choices of the clock-time correction $\Delta T$ cause only negligible minor variations. There were no 28-day months at all, but there were 20 months with 31 days. We believe that the Babylonian months never exceeded 30 days (even if the crescent did not appear), and therefore, a 31st day should be carried over to the next month. After carrying over the additional days of the 31-day months, there were 15 491 29-day months (46.9%) and 17 509 30-day months (53.1%).

Note that the difference in longitude between sun and moon on average changes by 12° in 24 hours. However, first visibility of the crescent depends not only on the difference in longitude, but also on lunar latitude. On the day before theoretical first visibility, $\Delta h$ can be as low as $-14.3°$, and on the day of first visibility, it can be as high as 14.4°. Between these two days, the value of $\Delta h$ increases by at least 6.2° and by at most 14.5°.

16 P. V. Neugebauer 1929, Tab. E 21.

17 Langdon, Fotheringham, and Schoch 1928, Tab. K.

18 Parker and Dubberstein 1956.

The 29- and 30-day months follow each other in a quite irregular and not easily predictable sequence, but which is not really random (i.e. there are discernible differences between this sequence and one obtained by tossing a biased coin). I checked it for

| $\|\Delta\|$ | $h$ | |
|---|---|---|
| | PVN | LFS |
| 0 | 10.4° | 10.7° |
| 1 | 10.4 | 10.7 |
| 2 | 10.3 | 10.6 |
| 3 | 10.2 | 10.5 |
| 4 | 10.1 | 10.4 |
| 5 | 10.0 | 10.3 |
| 6 | 9.8 | 10.1 |
| 7 | 9.7 | 10.0 |
| 8 | 9.5 | 9.8 |
| 9 | 9.4 | 9.6 |
| 10 | 9.3 | 9.4 |
| 11 | 9.1 | 9.1 |
| 12 | 8.9 | 8.8 |
| 13 | 8.6 | 8.4 |
| 14 | 8.3 | 8.0 |
| 15 | 8.0 | 7.6 |
| 16 | 7.7 | 7.3 |
| 17 | 7.4 | 7.0 |
| 18 | 7.0 | 6.7 |
| 19 | 6.6 | 6.3 |
| 20 | 6.2 | |
| 21 | 5.7 | |
| 22 | 5.2 | |
| 23 | 4.8 | |

**Tab. 1** Critical altitudes $h$ for crescent visibility, in dependence of the azimuth difference $|\Delta|$. The values are those of P. V. Neugebauer (PVN), and of Langdon, Fotheringham, and Schoch (LFS), respectively.

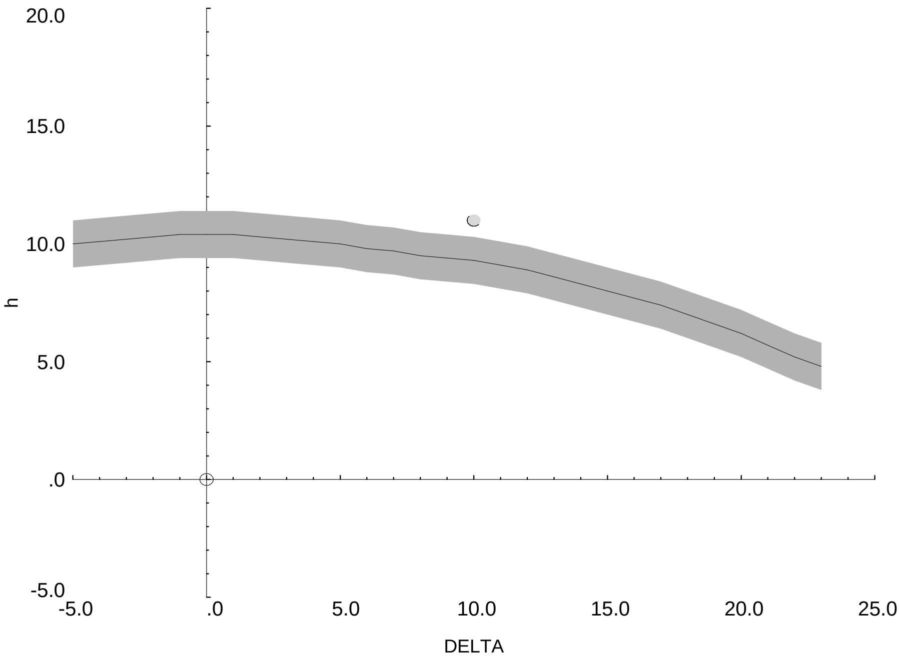


**Fig. 1** Crescent visibility. Shown are the theoretical visibility curve of Tab. 1, PVN (solid), the gray zone (±1°), the sun (at the coordinate origin), and the thin lunar crescent (with the earth light). The figure is to scale.

periodicities by comparing month-lengths spaced up to 3000 months apart. The most pronounced period is 669 months or 54 years: month-lengths spaced 669 months apart agree in 81% of the cases. Note that 669 synodic months, or 54 years, is a well-known eclipse period (the so-called exeligmos). In particular, there are fewer and shorter runs (sequences of consecutive months of equal length) than in a truly random sequence. I found 410 runs of three consecutive 29-day months, and 100 runs of five consecutive 30-day months; longer runs did not occur.

## 3 The Late Babylonian evidence: astronomical texts

In preparation to the publication of *Astronomical Dating of Babylon I and Ur III* in 1982,[19] I had collected 602 lunar crescents in Late Babylonian observational astronomical texts. These are observations of the crescent and as a rule are accompanied by a measured time interval between sunset and moonset. Most of the texts are dated between 500 and

19 Huber, Sachs, et al. 1982.

150 BC. I had excluded calculated crescent data, that is crescents explicitly designated as 'not observed' (NU PAP), therefore the agreement of that data base with modern calculation may be better than the agreement to be expected from genuine observations. On the other hand, one should be aware that the ancient astronomers occasionally may have substituted educated guesses (based on observations shortly before or after the critical evening) or predictions when observational conditions were poor, without always stating the fact.

In the time when those astronomical texts were written the Babylonians had fairly accurate prediction methods. Between 641 and 591 BC they had developed methods for predicting the so-called Lunar Six (time differences between the rising and setting of sun and moon, near new and full moons).[20] Their methods for predicting the Lunar Six and the beginning of the month were based on observations made one Saros cycle, or 18 years, earlier; they have been elucidated by Brack-Bernsen.[21]

This observational material then was compared with modern calculations based on the PVN values of Tab. 1; it was not deemed extensive enough to model seasonal dependencies. Among the 602 crescents, there are 34, or 5.6%, discrepancies between observation and calculation. Of those, 30 correspond to marginal visibility conditions with $|\Delta h| < 1.0°$, that is to cases where the altitude of the moon was within $\pm 1°$ of the theoretical curve deciding visibility, see Fig. 1. Among the remaining four observations, one is a clear gross error, and two come from the same, poorly preserved tablet. This residual error rate is remarkably small. Note that according to modern experience, when data are recorded by hand, in the absence of proof reading gross error rates in the range between 1% and 10% are quite common.[22] I therefore assume that there was careful proof reading. Given the low residual error rate, observations with $|\Delta h| \geqslant 1.0°$, rather than being genuine observations that are less accurate than usual, just as likely either are gross scribal errors, or evidence for wrong modern dating of the tablet.

If we disregard gross errors, we thus have 598 observations, among which 30, or 5.0%, disagree with modern calculation. This disagreement rate is a statistical estimate and as such, assuming an underlying binomial distribution, is affected by a standard error of 0.9% percentage points. It is advisable to keep this statistical uncertainty in mind – with a similar but independent data set we might just as well have obtained a miss rate near 4% or 6% – but for the subsequent order-of-magnitude calculations we shall operate with 5%. Since a month-length depends on two crescents, the 5% miss rate for crescents translates into an approximate miss rate of 10% for month-lengths.

The following Tab. 2 gives the empirical distribution of sighted and not sighted crescents with calculated $|\Delta h| < 1.0$.

20 Huber and Steele 2007.

21 Brack-Bernsen 2011.

22 See Hampel et al. 1986, 25–28.

| | not sighted | Δh | sighted | |
|---|---|---|---|---|
| | ××××××××× | –0.9 | | |
| | ××××× | –0.8 | | |
| | ××× | –0.7 | ××× | |
| | ××× | –0.6 | ×× | |
| | ××××××× | –0.5 | × | |
| 44 | ×××××× | –0.4 | ××× | 15 |
| | ×××× | –0.3 | | |
| | ××× | –0.2 | × | |
| | ×××× | –0.1 | ×××× | |
| | | –0.0 | × | |
| | | 0.0 | ×× | |
| | × | 0.1 | ××××××× | |
| | × | 0.2 | × | |
| | × | 0.3 | ××× | |
| | ×× | 0.4 | ×××××× | |
| 15 | ××× | 0.5 | ××××× | 38 |
| | ××× | 0.6 | ×× | |
| | × | 0.7 | ××× | |
| | ×× | 0.8 | ×× | |
| | × | 0.9 | ××××××× | |

**Tab. 2** Sighted and not sighted crescents in the Late Babylonian observational texts with the calculated value of $|\Delta h| < 1.0$ (from Huber, Sachs, et al. 1982, 27).

Based on this table I had tentatively proposed a probability model that disregarded gross errors but otherwise represented the observational astronomical data fairly well, namely:

- if $\Delta h < -1$, the crescent is never seen;
- if $-1 \leqslant \Delta h \leqslant 1$, the crescent is seen with probability $\frac{1+\Delta h}{2}$;
- if $\Delta h > 1$, the crescent is always seen.

Thus, near $\Delta h = 0$ the chance of seeing the crescent is roughly 50%, and for $\Delta h = -0.8$ the probability of sighting the crescent drops to 10%. By averaging over the intervals we obtain that for $-1 \leqslant \Delta h \leqslant 0$ the crescent is not sighted with probability 0.75 and sighted with probability 0.25, while for $0 \leqslant \Delta h \leqslant 1$ it is not sighted with probability 0.25 and sighted with probability 0.75. These theoretical 3 : 1 ratios are close to the empirical ratios 44 : 15 and 15 : 38 of Tab. 2.

Note that for genuine observations the situation is not symmetric: if a text claims that the crescent had been seen, but calculation gives a negative $\Delta h \leqslant -1.0°$, we are practically guaranteed to have a gross scribal error or a wrong date. But if the crescent had not been seen with $\Delta h \geqslant 1.0°$, it is possible that the sighting had failed because of poor atmospheric conditions. However, this asymmetry does not manifest itself in the Late Babylonian data and therefore was not modeled.

On the basis of a long sequence of 33 000 calculated crescents (with $\Delta h$ values rounded to the nearest multiple of 0.1°), the above probability model yields that the crescents would be observed one day early or late in 2.3% of the cases, respectively, resulting in a calculated miss rate of 4.6%. This is well within the statistical uncertainty of the observed miss rate, but for the model calculations of Sections 7 and 8, I preferred to increase the width $\pm d$ of the gray zone from $\pm 1.0°$ to $\pm 1.1°$, in order to obtain a miss rate of 5.1%, closer to the observed value.

## 4 The Neo-Babylonian evidence: administrative texts

Non-astronomical texts – mostly administrative texts from between 650 and 450 BC, dated on day 30, where such a date would appear to imply that the month had 30 days – have a substantially higher disagreement rate against calculation. In 1982 we found 153 suitable texts, with a disagreement rate of $50/153 = 32.7\%$ for month-lengths.[23] This translates into about 17% with regard to crescents, and there are about 8% cases with $|\Delta h| \geqslant 1.0°$.[24] It is difficult to separate the causes of these discrepancies into careless dating, less reliable observations made by non-astronomers, and gross scribal errors. I now repeated the calculations with newer programs and the best currently available ΔT-values for the Neo- and Late-Babylonian period.[25] The results were practically identical.

The correct chronology with 50 misses does not give the best possible fit. Among 20 000 alignments of the 153 observed month-lengths along a calculated sequence there were 16, or 0.08% alignments with 50 or fewer misses. The best fit had 46 misses, and fits with 50 and 49 misses were found 669 months, or 54 years, before and after the true date, respectively (remember the 669 months lunar period!). This means that a randomly chosen (wrong) alignment has a chance of 0.08% of hitting an equally good or better agreement than the true one. And there are good chances to find an equally good fit exactly 669 months or 54 years before or after the true one.

23 Huber, Sachs, et al. 1982, 28–29.

24 Huber, Sachs, et al. 1982, 28–29.

25 The ST82f formula for ΔT of Huber and De Meis 2004, 25.

Reassuringly, we can conclude that the agreement of the recorded 30-day months with the calculated 30-day months is significantly better for the true chronology than for a wrong chronology. However, even with 153 recorded month-lengths the agreement ordinarily is not good enough to permit independent dating in the absence of other evidence, that is, unless we can narrow down the candidate chronologies to a few precise years. A detailed quantitative discussion is required.

Theoretical arguments involving the miss rates of *wrong* alignments are based on astronomical theory, namely on the 29.53 days length of the synodic month and the resulting miss rate of 47% for randomly aligned 30-day months. The binomial distribution gives a good approximation to the distribution of empirically observed miss rates, see Fig. 2.

Arguments involving the miss rate of the *correct* chronology are more delicate, quite apart from the question whether the miss rates for NB and OB times were the same. For true alignments this miss rate also follows a binomial distribution, but with a lower value of $p$, see Fig. 7 of Section 8. For arguments relying on the miss rates for true alignments one should keep in mind that the disagreement rate of $p = 50/153 = 32.7\%$ between observed and calculated month-lengths is a mere estimate, and as such is affected by a standard error of $\sqrt{p(1-p)/n}$, or about 3.8 percentage points.

Since the observed miss count is a random quantity, some luck is involved. Let us fix the idea by arbitrarily assuming that the true disagreement rate is 32.7%, and that we are trying to find a date on the basis of an independent new sample of 153 month-lengths. Note that this is a much larger sample than we can hope for in the case of Ammiṣaduqa + Ammiditana. Then for a correct alignment the chances are 27% that the observed miss count is less or equal 46, and also 27% that it is greater or equal 54. With a correct alignment and good luck, we perhaps might have obtained 46 misses and a miss rate of 30.0%, with bad luck perhaps 54 misses and a miss rate of 35.3%. With a wrong chronology the expected number of misses is $0.47 \times 153 = 72$, and the binomial probability of obtaining 46 or fewer misses is 0.0000145, and of obtaining 54 or fewer misses is 0.00224.

Assume now that we desire to fix the true chronology with an error probability of 1%. Then the probability that the best of 690 random trials with wrong alignments achieves 46 or fewer misses is approximately 1% ($\approx 690 \times 0.0000145$). In such a lucky case, picking the correct date based solely on month-lengths is eminently feasible – in a line-up of 690 candidates we pick the true one with 99% chance. On the other hand, if we are unlucky, the probability that the best of 4 random trials with wrong alignments achieves 54 or fewer misses is about 1% ($\approx 4 \times 0.00224$), which would be just sufficient to pick the correct chronology in an Ammiṣaduqa-like case with 99% chance in a line-up of four precisely fixed candidate chronologies.

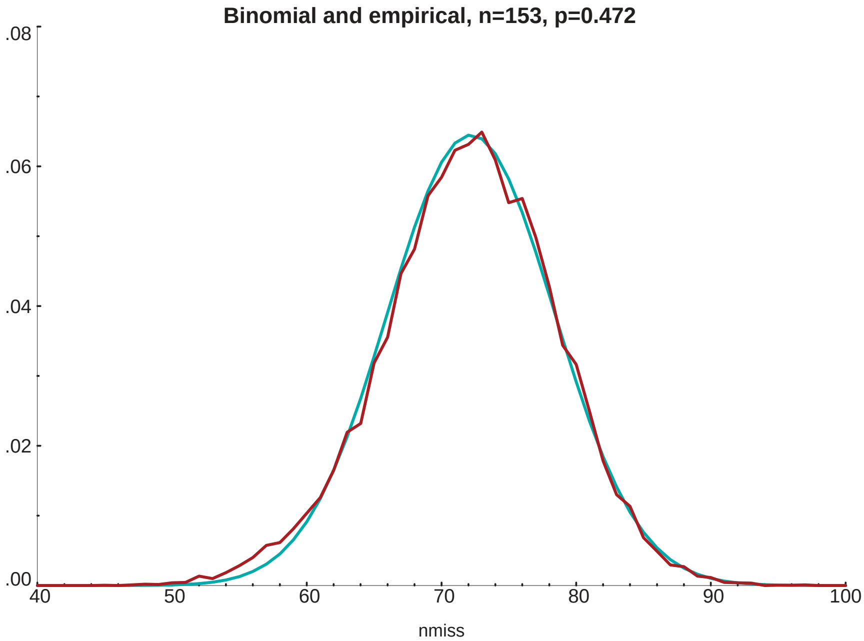


**Fig. 2** Comparison between the binomial distribution ($n = 153$, $p = 0.472$, blue) and the empirical frequencies obtained from 20 000 alignments of the Late Babylonian data (red). (The LB data contain 149 30-day months and 4 29-day months, and the $p$ of the binomial distribution was adjusted accordingly from 0.47 to 0.472.)

A detailed discussion of the NB material follows. The 153 texts contain 4 attested 29-day months, 3 of which agree with calculation, and one calculates as 30 days, possibly shortened by marginal calculated visibility ($\Delta h = 0.2°$) at the beginning of the month. Tab. 3 lists the results of a comparison of the remaining 49 months that calculate as having 29 days but where the texts have a day 30. This is an extract from *Astronomical Dating of Babylon I and Ur III*,[26] but re-calculated with newer programs.

We note that of those 49 months 14 have marginal visibilities $(-1.1 \leqslant \Delta h \leqslant 0)$ at the beginning of the month, and 14 have marginal visibilities at the end of the month $(0 \leqslant \Delta h \leqslant 1.1)$. The former may lengthen the observed month at the beginning, the latter at the end. One month has marginal conditions both at the beginning and the end. For the remaining 22 months the mismatch to calculation cannot be explained by marginal visibility; for them, we have $1.6 \leqslant \Delta h \leqslant 6.8$ at the end of the month. Incidentally, the big list of 33 000 calculated crescents shows that at the end of calculated 29-day months $\Delta h$ ranges from 0 to 10.9.

26 Huber, Sachs, et al. 1982, 51–55.

| Syzygy Number | begin of month | | end of month | |
|---|---|---|---|---|
| | Δ$h$ before 1st visibility | Δ$h$ at 1st visibility | Δ$h$ before 1st visibility | Δ$h$ at 1st visibility |
| 5537 | –6.2 | 3.6 | –9.1 | .0 |
| 5925 | –4.0 | 6.8 | –10.9 | .1 |
| 5827 | –4.2 | 6.7 | –11.2 | .1 |
| 5600 | –.8 | 7.7 | –7.4 | .2 |
| 5610 | –5.6 | 4.7 | –7.8 | .2 |
| 5917 | –7.0 | 5.7 | –10.6 | .3 |
| 5687 | –3.9 | 6.0 | –9.1 | .3 |
| 5315 | –6.1 | 3.6 | –8.6 | .4 |
| 5722 | –6.5 | 6.1 | –10.4 | .7 |
| 5854 | –3.8 | 7.2 | –9.6 | .8 |
| 11235 | –1.4 | 9.4 | –8.6 | .9 |
| 9035 | –3.8 | 6.3 | –8.1 | .9 |
| 5597 | –7.0 | 4.9 | –8.4 | .9 |
| 6219 | –3.4 | 5.5 | –7.8 | 1.0 |
| 5683 | –.6 | 10.3 | –7.3 | 1.5 |
| 4726 | –3.1 | 8.1 | –10.0 | **1.6** |
| 5781 | –2.9 | 9.5 | –9.1 | **1.7** |
| 5363 | –1.2 | 8.2 | –5.4 | **2.0** |
| 5766 | –5.1 | 8.1 | –10.6 | **2.0** |
| 5045 | –1.0 | 8.3 | –7.0 | 2.2 |
| 5446 | –4.3 | 7.9 | –8.4 | **2.2** |
| 5748 | –5.3 | 5.9 | –8.1 | **2.7** |
| 5570 | –5.2 | 8.5 | –10.0 | **2.8** |
| 5774 | –1.8 | 7.6 | –6.6 | **3.1** |
| 5802 | –4.3 | 8.1 | –9.9 | **3.4** |
| 5530 | –1.4 | 10.7 | –8.2 | **3.5** |
| 5526 | –4.3 | 7.0 | –7.1 | **3.6** |

*(continued on next page)*

*(continued from previous page)*

| Syzygy Number | begin of month | | end of month | |
|---|---|---|---|---|
| | Δ$h$ before 1st visibility | Δ$h$ at 1st visibility | Δ$h$ before 1st visibility | Δ$h$ at 1st visibility |
| 5877 | –4.7 | 9.0 | –9.5 | **3.6** |
| 5678 | –4.8 | 8.7 | –9.5 | **4.3** |
| 5327 | –.2 | 9.3 | –4.3 | 4.4 |
| 6001 | –4.9 | 9.3 | –9.6 | **4.4** |
| 4996 | –5.4 | 7.8 | –9.1 | **4.7** |
| 5850 | –2.2 | 10.5 | –7.2 | **5.0** |
| 5334 | –1.5 | 11.6 | –7.3 | **5.1** |
| 5890 | –4.4 | 9.6 | –8.1 | **5.3** |
| 6377 | –.0 | 12.7 | –6.3 | 5.3 |
| 5405 | –.6 | 11.6 | –6.1 | 5.4 |
| 5329 | –.0 | 10.3 | –5.4 | 5.5 |
| 5499 | –2.0 | 11.7 | –6.7 | **5.5** |
| 5903 | –4.0 | 9.9 | –7.0 | **6.1** |
| 6194 | –2.5 | 9.6 | –5.3 | **6.4** |
| 5050 | –1.3 | 11.8 | –6.4 | **6.8** |
| 5654 | –.2 | 13.3 | –5.5 | 7.4 |
| 5863 | –.8 | 13.0 | –5.5 | 7.7 |
| 5899 | –.2 | 12.1 | –4.2 | 8.5 |
| 5008 | –1.1 | 11.6 | –4.5 | 8.7 |
| 5677 | –.3 | 12.5 | –4.8 | 8.7 |
| 6392 | –.1 | 13.7 | –3.5 | 9.0 |
| 5442 | –.8 | 13.2 | –4.8 | 9.5 |

**Tab. 3** Neo-Babylonian data: 49 texts dated on day 30, whereas calculation indicates a 29-day month. The table is sorted according to the calculated Δ$h$ at 1st visibility at the end of the month. Where a mismatch cannot be explained as a gray-zone effect, the Δ$h$ value is shaded.

I am not sure how Tab. 3 is to be interpreted. Clearly, there is a crowding of values in the marginal visibility zones, both at the beginning and the end of the month. For the remaining 22 months, or 45% of the total, at the end of the month Δ$h$ is fairly evenly distributed over the range between 1.6 and 6.8, and thus the mismatch cannot simply be explained by expanding the marginal visibility zones. I believe the most plausible suggestion is that between 40% and 50% of the day-30 dates are 'overhang' dates, on which a scribe wrote day 30 instead of day 1 of the following month. In these cases the following day would be day 2 of the new month. I shall elaborate on this idea in Section 7.

## 5 On the discriminatory power of month-lengths

I shall concentrate on methodological aspects, but shall illustrate them by discussing in detail two concrete data collections that involve crucial aspects and difficulties: month-lengths (1) from the reign of Ammiṣaduqa, and (2) from the reign of Ammiditana. An early draft had contained also a detailed discussion of (3) the Hammurabi-Samsuiluna and (4) the Ur III evidence, both being less conclusive, but for reasons of space I now give only brief summaries. To avoid over-burdening the discussion, I shall relegate most technical details to Sections 7, 8, and 9 below, and to Section 10, the Appendix listing the data collections.

A perennial methodological problem is that our pool of month-length data may be too small to guarantee a decision. Even in the absence of grosser errors, such as erroneous intercalations, the unavoidable problem is the randomness of the miss counts. With some luck, the correct chronology may give a lower than expected miss count and force a decision. But if it accidentally gives a high miss count, the situation may remain undecided. With the miss counts of wrong chronologies opposite problems apply. More new data will not necessarily sharpen the decision – extreme counts will tend to regress toward the average (Galton's law of 'regression to mediocrity'). For example, in the case of Ammiṣaduqa to be discussed in Section 5.1, addition of 5 more month-lengths resulted in a poorer separation between the putative right and wrong alignments. In order to illustrate the intrinsic variability of small sample statistics – and to raise a warning signal against the temptation of over-interpretation – I shall present the analysis of the Ammiṣaduqa data both in terms of the smaller earlier and the increased later sets. Note that in critical cases elimination of a single mismatch by a minute change of $\Delta T$ can dramatically lower the P-values (minimum rejection levels), see Section 5.2.1.

Repeatedly, doubts have been raised whether the Old Babylonian month-lengths, i.e. month-lengths derived from texts dated on day 30, obey laws comparable to those of the Neo-Babylonian ones, in particular whether the NB miss rate of 33% is applicable. If it comes to the worst, the miss counts for the correct OB chronology might be no better than for wrong ones, and then the month-lengths would be useless for dating purposes. But at least in principle – that is, if the sample size is large enough – the month-length data can be used to settle this question in a methodological clean fashion, namely by testing whether one of the four main Venus chronologies gives a better fit than the best of four random wrong chronologies. Technically, this means that we should show by a statistical test that the best of the four miss counts is significantly better than the best of four random draws from a binomial distribution corresponding to wrong chronologies. If successful, this test (which is based only on the theoretically secure rate $p = 0.47$) would not only establish that one of the four chronologies is correct. It would also imply correctness of the singled-out best chronology and wrongness of the other three, and that the miss rate for a correct chronology – while not necessarily equal to the NB value – is well below that for a wrong chronology also for OB data.

But what sample size would we need? The Ammiṣaduqa sample sizes of 27 or 32 are not good enough. Assuming that the miss rate for correct chronologies is close to the NB value of 33%, I have estimated (with the help of some rough order-of-magnitude calculations with the binomial distribution) that one would need 60 or more month-lengths for such a test to have a fair chance of being successful. Actually, with some luck we shall squeeze by with a total of 49 data by combining the Ammiṣaduqa and Ammiditana samples in Section 5.2.1.

### 5.1 Case 1: Ammiṣaduqa

This subsection is concerned with the question whether and when the best fitting chronology can be declared being the correct chronology. It also illustrates that more data do not necessarily improve the discriminatory power.

In the case of the reign of Ammiṣaduqa we have four distinct, precisely fixed main chronological possibilities: Ammiṣaduqa year 1 = –1701, –1645, –1637, or –1581. For each of these four possibilities the syzygy numbers of the months from year 1 month VII to year 18 month VI are astronomically fixed by the Venus data. If Ammiṣaduqa year 1 is a normal year, they imply that month I of that year has the syzygy numbers –8666, –7974, –7875, or –7183, respectively.[27]

I first shall discuss the set of 27 day-30 dates available by 2010. Fig. 3 plots the binomial distributions corresponding to $p = 0.33$ on the left hand side (the miss rate corresponding to the Neo-Babylonian control material for a correct chronology) and to $p = 0.47$ on the right hand side (the theoretical rate for random wrong chronologies). There is considerable overlap between the two distributions. For a correct chronology we expect a miss count of 8.9, with a standard deviation of $\sqrt{n\,p\,(1-p)} = 2.4$. For a wrong chronology the expected count is 12.7, and for the best of four wrong chronologies the expected count is 10.0.

27 These numbers continue the syzygy count of Goldstine 1973 backward to the 2nd and 3rd millennium.

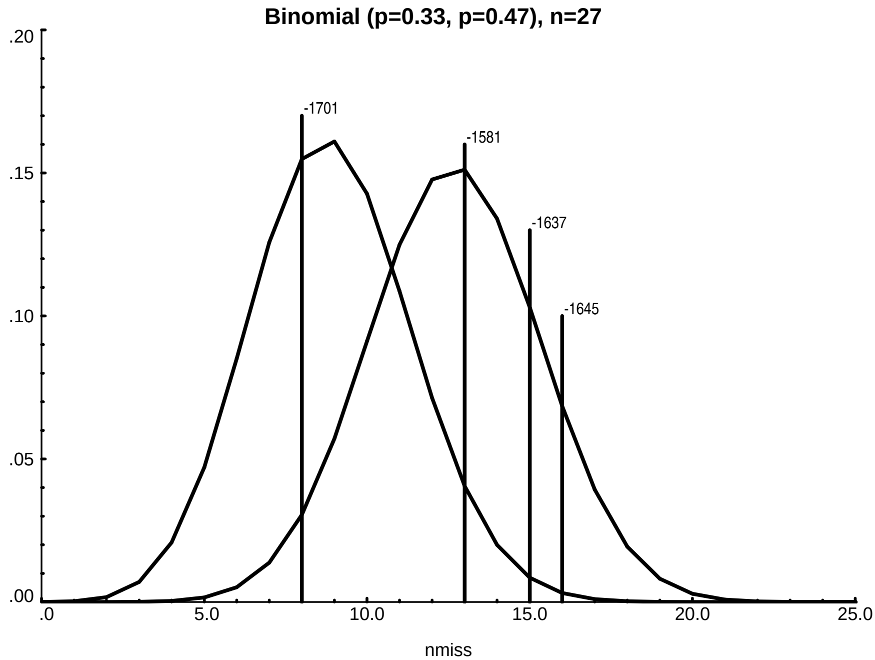


**Fig. 3** Ammiṣaduqa data (set available in 2010). Binomial distributions for $p = 0.33$ and $p = 0.47$, $n = 27$. The vertical lines indicate the number of misses obtained for the 4 main chronologies with the Ammiṣaduqa data: –1701: 8, –1645: 16, –1637: 15, –1581: 13.

For the four main chronologies we obtain miss counts of 8, 16, 15, and 13 respectively, see Fig. 3. We expect that one of the four main chronologies is correct and is drawn from the left-hand distribution, while the other three are wrong and are drawn from the right-hand distribution. The figure clearly is consonant with this assumption; it suggests that –1701 is correct, and that the other three are unlikely in different degrees. Actually, the miss count for –1701 is below the expected value for a correct chronology by one unit, and the other three counts are all above the expected value for a wrong chronology.

While this data set clearly favors the –1701 chronology, the sample size is not large enough to force a decision in its favor. The probability that a random wrong chronology yields 8 or fewer misses is 0.052. But since we have picked the –1701 chronology not for extraneous reasons, but rather because it was the best of four, we should consider the probability that *the best of four random wrong chronologies* yields 8 or fewer misses; this probability is $1 - (1 - 0.052)^4 = 0.19$.

Through a Bayesian approach we can quantify the intuitive impression that –1701 is best and –1645 worst by assigning equal prior probabilities to the four chronologies. Then, their posterior probabilities are proportional to

$$\left(\frac{p\,(1-q)}{(1-p)\;q}\right)^k$$

where $p = 0.33,\; q = 0.47$, and $k$ is the number of misses. For the 2010 data set they calculate as:

–1701: 0.927, –1645: 0.008, –1637: 0.015, –1581: 0.049.

Note that if all $k$ are increased by the same constant, the posterior probabilities stay the same – this means that the Bayesian approach ignores the absolute quality of the four fits and is in particular unable to tell you whether or not all four are wrong. A disadvantage of all Bayesian approaches is that they have to rely on the Neo-Babylonian value of $p$.

By 2013, 5 more day-30 dates had become available. The result is depicted in Fig. 4. The –1701 chronology still is ahead, but its miss count of 12 now exceeds the expected value of 10.6 for a correct chronology by one unit, while the other three are at or above the expected value of 15.0 for a wrong chronology.

The 2013 Ammiṣaduqa data set is less able to assert correctness of the –1701 chronology than the 2010 set. While with the earlier set the lowest miss count was one unit below the expected value for a correct chronology, with the later set it is one unit above the expected value, and the probability that a random wrong chronology yields 12 or fewer misses is 0.18. The miss count of 12 of the High Chronology lies between the expected miss count for a correct chronology (10.6) and the count expected for the best of four wrong chronologies (12.2). All these number lie well within statistical variability; note that the standard deviation $\sqrt{n\,p\;(1-p)} = 2.7$ of the miss count for the correct chronology exceeds the difference between the last two numbers. (By the way, the standard deviation of the miss count of the best of four wrong chronologies is 2.0.)

The evidence does not suffice to establish correctness of the High chronology, but if the miss rate of 0.33 of the NB data is even approximately applicable, we can confidently (with better than 99% confidence) reject correctness of the traditional –1645 Middle chronology: the P-values are 0.45% for the 2010 set and 0.19% for the 2013 set.

For the 2013 set the posterior probabilities calculate as

–1701: 0.736, –1645: 0.012, –1637: 0.126, –1581: 0.126.

We summarize: the Ammiṣaduqa month-length evidence points in favor of the High chronology and disfavors the –1645 chronology.

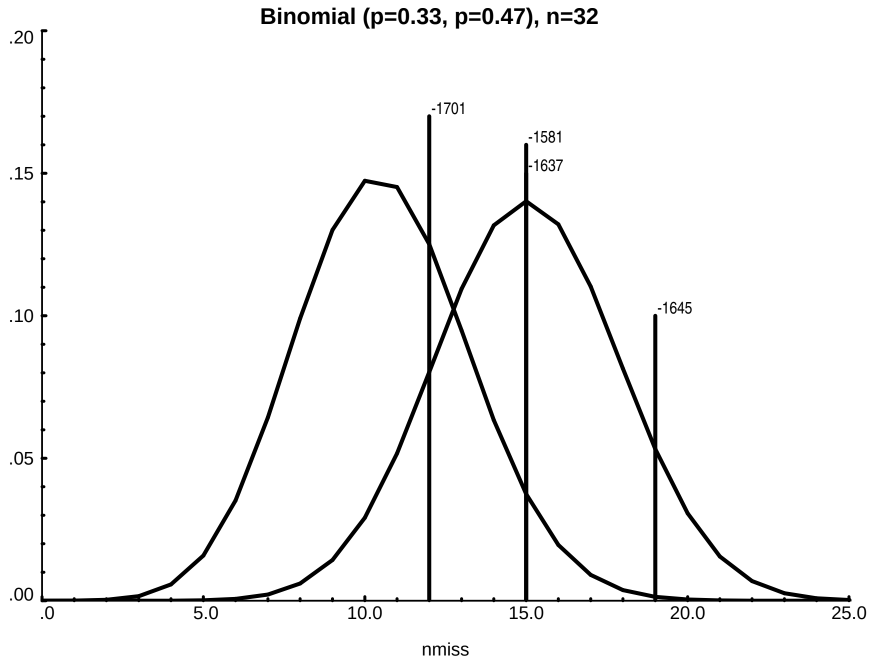


**Fig. 4** Ammiṣaduqa data (set available in 2013). Binomial distributions for $p = 0.33$ and $p = 0.47$, $n = 32$. The vertical lines indicate the number of misses obtained for the 4 main chronologies with the Ammiṣaduqa data: −1701: 12, −1645: 19, −1637: 15, −1581: 15.

### 5.2 Case 2: Ammiditana

In the case of Ammiditana, the king of Babylon immediately before Ammiṣaduqa, we have 13 (set available since 1982), or 17 (set available in 2013) usable attestations of 30-day dates (from Ammiditana years 24 to 36). The problem here is that the positions of the intercalary months are not fixed by Venus observations as in the case of Ammiṣaduqa. Specifically, the question is whether the Ammiditana segment joins snugly in front of Ammiṣaduqa. Note that for the 7 years between Ammiditana 34 and Ammiṣaduqa 3 only a single intercalation is attested (see Section 10.2 in the Appendix of this article). So we should consider the possibility that there is an unattested intercalation near the boundary (for example a $XII_2$ in Ammiditana year 36, or a $VI_2$ in Ammiṣaduqa year 1; these two choices shift all currently attested Ammiditana month-lengths by one month, but do not interfere with their relative distances). I think it is advisable, if not mandatory, to take the uncertainty into account and to consider the possibility of an additional intercalation. In Tabs. 4–5, the results are identified by '+0' without, by '+1' with such

| | Ammiṣaduqa year 1 | Syz.no. of year 1 | Aṣ | +0 Ad | +0 Ad+Aṣ | +1 Ad | +1 Ad+Aṣ | min Ad | min Ad+Aṣ |
|---|---|---|---|---|---|---|---|---|---|
| No. of months | | | 27 | 13 | 40 | 13 | 40 | 13 | 40 |
| High | −1701 | −8666 | 8 | 8 | 16 | 2 | 10 | 2 | 10 |
| High Middle | −1645 | −7974 | 16 | 5 | 21 | 8 | 24 | 5 | 21 |
| Low Middle | −1637 | −7875 | 15 | 8 | 23 | 11 | 26 | 8 | 23 |
| Low | −1581 | −7183 | 13 | 9 | 22 | 5 | 18 | 5 | 18 |

**Tab. 4** Counts of misses for Ammiṣaduqa and Ammiditana (sets available in 2010).

an additional intercalation, and 'min' gives the lower of the two counts. With '+1' the Ammiditana block as a whole is shifted one month.

When considered by themselves, the Ammiditana data lead to similar conclusions as the Ammiṣaduqa data: both favor the High chronology, see Tabs. 4–5, and compare Figs. 3–4 with Fig. 5. We may treat the two data sets as two independent witnesses. They are concordant, but not quite conclusive when taken separately. There are more promising approaches, namely by combining the two sets. I shall discuss three possible approaches.

Firstly, we may form a working hypothesis on the basis of one set and then test it on the basis of the other. Or secondly, we can pool the data, forget about the evidence of the components and proceed on the basis of the joined set. A third possibility is to combine the evidence from the different sets by Bayesian methods. To some extent the choice of method is a matter of taste. Personally, I think that in our case the first approach, testing a working hypothesis, is the cleanest (and clearest). Others might better like the third, Bayesian approach.

The approaches are complementary. Statistical tests can assess absolute quality, but have difficulties measuring relative merits, while with Bayesian approaches the opposite applies. In our particular case the first approach is suitable for establishing correctness of a chronology, the second for establishing wrongness of selected chronologies, and the third for assigning relative probabilities.

From now on we shall concentrate on the 2013 data set. The joined Ad + Aṣ data of Tab. 5 suggest that in the +0 column all four alignments are 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 own standard

| | Ammiṣaduqa year 1 | Syz.no. of year 1 | Aṣ | +0 Ad | +0 Ad+Aṣ | +1 Ad | +1 Ad+Aṣ | min Ad | min Ad+Aṣ |
|---|---|---|---|---|---|---|---|---|---|
| No. of months | | | 32 | 17 | 49 | 17 | 49 | 17 | 49 |
| High | –1701 | –8666 | 12 | 11 | 23 | 3 | 15 | 3 | 15 |
| High Middle | –1645 | –7974 | 19 | 6 | 25 | 10 | 29 | 6 | 25 |
| Low Middle | –1637 | –7875 | 15 | 9 | 24 | 13 | 28 | 9 | 24 |
| Low | –1581 | –7183 | 15 | 10 | 25 | 7 | 22 | 7 | 22 |

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Expected number of misses (± standard deviation) | correct | | 10.6 ±2.7 | 5.6 ±1.9 | 16.2 ±3.3 | 5.6 ±1.9 | 16.2 ±3.3 | | |
| | wrong | | 15.0 ±2.8 | 8.0 ±2.1 | 23.0 ±3.5 | 8.0 ±2.1 | 23.0 ±3.5 | | |
| | best of 4 wrong | | 12.2 ±2.0 | 5.9 ±1.4 | 19.4 ±2.4 | 5.9 ±1.4 | 19.4 ±2.4 | | |

Tab. 5 Counts of misses for Ammiṣaduqa and Ammiditana (sets available in 2013).

deviation of 3.3. Thus, either a snug joining of the two data sets (+0) is wrong, or all four chronologies are wrong, or OB month-lengths are worthless for dating purposes.

On the other hand, in the +1 column the miss-counts match the assumption that we have one correct and three wrong chronologies. The count (15) for the HC turns out even better than what we would expect (16.2) for the correct chronology, and the counts for the Middle chronologies are devastatingly poor.

If indeed one of the four chronologies is correct, as is generally assumed, and if OB month-length data can provide valid evidence, a comparison between the +0 and +1 columns thus furnishes strong arguments in favor of an additional intercalation, and in favor of the High chronology, as well as against the Middle chronologies.

Fig. 5 is analogous to Figs. 3–4. It plots the binomial distributions corresponding to $p = 0.33$ (the miss rate corresponding to the Neo-Babylonian control material for a correct chronology) and to $p = 0.47$ (the theoretical rate for random wrong chronologies), and in addition it also shows the distribution of the counts for the better of two random draws from the right hand distribution. Correspondingly, the vertical lines indicate the better of the two Ammiditana counts (without and with the additional intercalation).

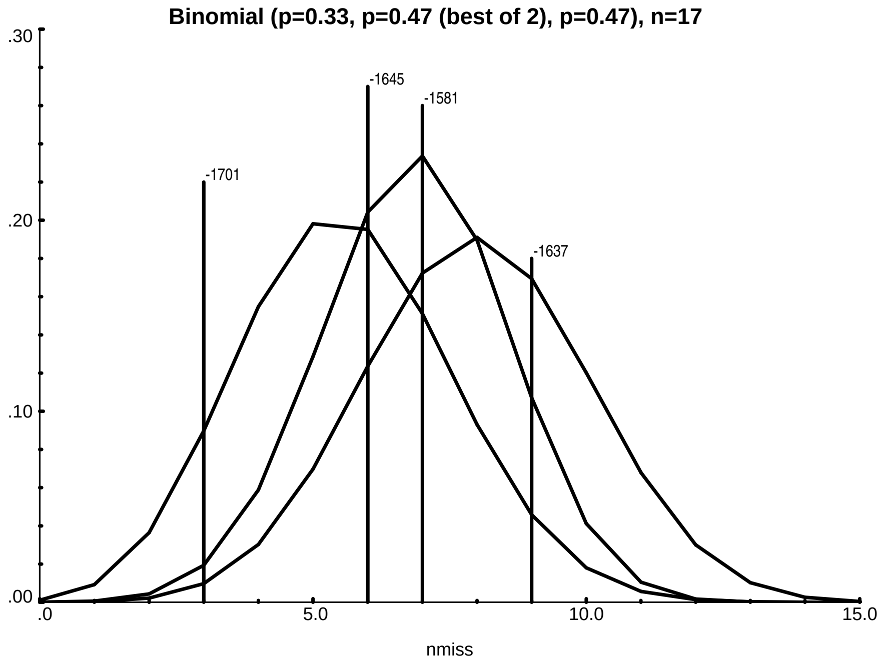


Fig. 5 Ammiditana data (set available in 2013), $n = 17$. Leftmost: binomial distribution for $p = 0.33$ (correct chronology); rightmost: for $p = 0.47$ (wrong chronology). In between the two, also the distribution of the best of two wrong chronologies is shown. The vertical lines indicate the number of misses obtained for the 4 main chronologies with the Ammiditana data. The lines correspond to the numbers obtained for the best of two fits (without or with the additional intercalation): −1701: 3, −1645: 6, −1637: 9, −1581: 7.

Back in 1982, for the −1701 chronology and the 13 month-lengths then available I had obtained a single miss when assuming an additional intercalation. For the same data the newer programs gave two misses. It turns out that the uncertainty of ΔT is such that for one of the Ammiditana month-lengths the decision between 29 and 30 days is ambiguous. The newer programs, which allow to vary ΔT, show not only that with the default ΔT the miss counts correspond to a local maximum for both Ammiṣaduqa and Ammiditana, but also that if ΔT is lowered by merely 3 minutes (that is, if $c$ is changed from 32.50 to 32.35), the Ammiditana miss counts become 1 for the 1982/2010 set, 2 for the 2013 set. See Section 9 and Figs. 8, 9, 10, 11, and 12.

### *5.2.1 Ammiṣaduqa results used as a working hypothesis*

The Ammiṣaduqa data – both the Figs. 3–4, and the Bayesian posterior probabilities – suggested that the HC is the correct chronology, but they did not suffice to establish it on the 5% level. We now use the HC as our working hypothesis, and we have to test the hypothesis that HC is wrong with the help of the Ammiditana data.

We take the 2013 month-length data and the miss counts of Tab. 5. If the HC is wrong, the Ammiditana miss count is distributed like a draw from a binomial distribution with $n = 17$ and the parameter $p = 0.47$, whether or not we assume the presence of an additional intercalation. The smaller of the two miss counts (without and with the additional intercalation) then is distributed like the smaller of two draws from this binomial. With the default ΔT, the smaller of the observed miss counts is 3 (see Tab. 5), and the probability of achieving $\leqslant 3$ misses is 2.4%. If we decrease ΔT by 3 minutes, the Ammiditana miss count for –1701 is decreased by 1 unit, and the minimum rejection level is reduced from 2.4% to 0.50%. Calculations with any of the other chronologies no longer are relevant. In other words, we reject wrongness of the HC on a level below 3%, possibly below 1%.

A possible criticism that might be raised against these calculations is that we draw pairs of chronologies spaced by one month, and so the draws are not quite random. But an empirical test (comparing pair-wise random draws with single draws from the calculated sequence of month-lengths) shows that the approximation nevertheless is excellent.

We emphasize that this test relies only on the secure rate of $p = 0.47$. In addition to confirming that one of the four chronologies is correct, namely the HC, it simultaneously implies that the Old-Babylonian miss rate for a correct chronology is substantially below 47%, and that the other three chronologies are wrong.

*We summarize that we can confirm 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.*

### 5.2.2 *Joining the Ammiṣaduqa and Ammiditana data*

Alternatively, we may join the two data sets. The counts are shown in Tabs. 4–5, and the situation is depicted in Fig. 6. We again expect that one of the four main chronologies is correct and is drawn from the left-hand distribution ($p = 0.33$), while the other three are wrong and are drawn from the rightmost distribution ($p = 0.47$). Since for each chronology we again are considering the better matching of two possibilities, the vertical lines indicate the lower value of the two counts, and the distribution of the lesser of two independent draws from the right-hand binomial ($p = 0.47$) is depicted in the middle. There is considerable overlap between the distributions, but the figure clearly suggests that –1701 is the correct chronology and that both middle two chronologies are wrong. This can be quantified by calculating minimum rejection levels.

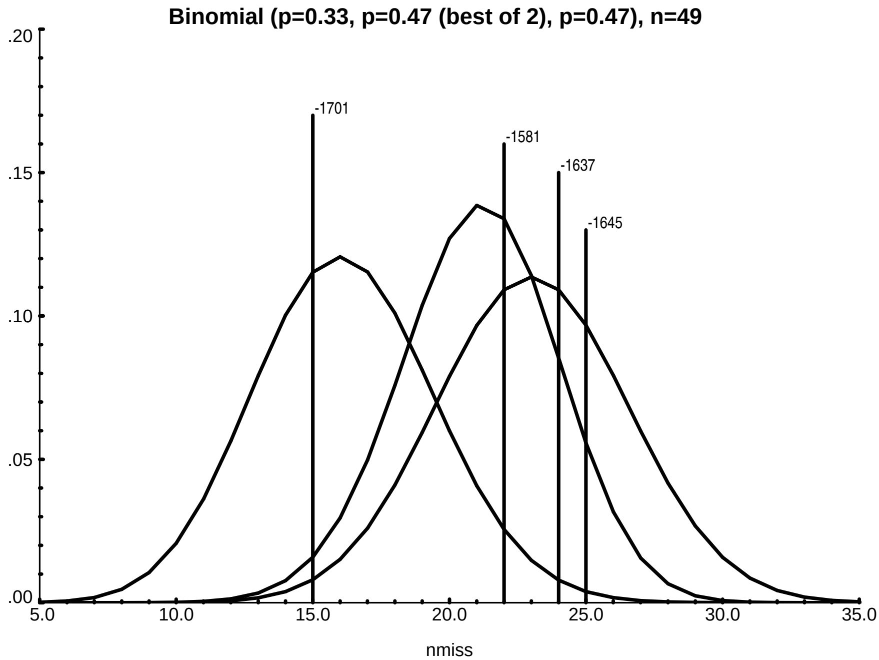


**Fig. 6** Ammiditana + Ammiṣaduqa data (set available in 2013). Binomial distributions for $p = 0.33$ (correct chronology) and $p = 0.47$ (wrong chronology); $n = 49$. In between the two, also the distribution of the best of two wrong chronologies is shown. The vertical lines indicate the number of misses obtained for the 4 main chronologies with the combined Ammiṣaduqa-Ammiditana data. The lines correspond to the numbers obtained for the best of two fits (that is without or with the additional intercalation): −1701: 15, −1645: 25, −1637: 24, −1581: 22.

We perform statistical tests between the hypothesis H (chronology correct, $p = 0.33$) and the alternative A (chronology wrong, $p = 0.47$). Let $x$ be the number of misses and assume that the binomial distributions $B(n, p)$, with the above values of $p$, and $n = 49$, give adequate approximations for the probability distribution of the miss counts.

*(1) Test A against H*

In this case, we have to test A (wrongness of the best fitting chronology). That is, we ought to check whether the best alignment we had found (among 4 chronologies and 2 intercalation patterns for each) fits significantly better than what can be expected from the best of 8 randomly chosen wrong alignments. We run into similar sample size problems as above with the Ammiṣaduqa data, but the rejection level (the probability that *the best of four random wrong chronologies* yields 8 or fewer misses) is somewhat reduced, namely to 11.1% for the 2013 set. This number is conservative: the (somewhat arbitrary default) formula for $\Delta T$ yields a local maximum of the counts (see Figs. 11–12 of Section 9), and decreasing $\Delta T$ by merely 3 minutes would lower the counts by 1 unit and lower the rejection level from 11.1% to 5.2%. These tests rely only on the theoretically secure rate $p = 0.47$.

*(2) Test H against A*

This test is included here to illustrate the ancillary use of the 33% rate to add evidence that a particular chronology is wrong. Assume that you reject H (correctness of the chronology) if $x \geqslant k$, and thereby accept A (wrongness of the chronology). Then, the probability of falsely rejecting H can be calculated from the binomial distribution appropriate for H ($p = 0.33$). For example, with the 2013 data set for the Low Middle chronology we have 24 misses for +0 and 28 for +1, and we obtain for the probability of falsely rejecting correctness of that chronology 1.47% or 0.04%, respectively. We stay on the conservative side if we pick the lower miss count and the higher $P$-value (if we had assigned equal probabilities to +0 and +1, we would have taken the average of the two $P$-values). Thus, we obtain the results seen in Tab. 6.

| Chronology | 2013; $n = 49$ | $P\ (x \geqslant k)$ |
|---|---|---|
| –1645 | $k = 25$ | 0.68% |
| –1637 | $k = 24$ | 1.47% |
| –1581 | $k = 22$ | 5.51% |

**Tab. 6** Ammiditana + Ammiṣaduqa. Error probability when rejecting correctness of a chronology.

*Thus, for each of the two middle chronologies we can assert, with error probabilities below 2%, that it is wrong.* This result is dependent on the reliability of the estimated value $p = 0.33$.

With this test, the case of the Low chronology (–1581) is inconclusive. While in Fig. 6 it sits where we expect a wrong chronology to sit, Tab. 6 shows that the fit is not sufficiently poor that the chronology can be rejected on its own merit with the conventional 5% significance level.

### 5.2.3 *Combining the Ammiṣaduqa and Ammiditana data by Bayesian methods*

Probabilities from different sources are easiest to combine by Bayesian methods, but it is difficult to agree on the choice of prior probabilities. We must consider eight possibilities: four chronologies, and for each of them absence or presence of an additional intercalation. For the following I give equal probabilities 0.25 to the four main chronologies, and probability $\alpha$ to the presence of an additional intercalation. As in Section 5.1

these priors are multiplied by

$$\left(\frac{p\,(1-q)}{(1-p)\;q}\right)^k$$

where $p = 0.33$, $q = 0.47$, and $k$ is the number of misses. The posterior probabilities of the eight possibilities are obtained by scaling the resulting values so that they sum to 1. The sum over the four components with additional intercalation then gives the posterior probability $\beta$ of having such an intercalation, and for each particular chronology the posterior probability is the sum of the two values without and with 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 $\alpha = 0.1$. The results of the calculation for the 2013 set (i.e. with the miss counts of Tab. 5) are listed in Tab. 7.

*We note first that $\alpha = 0.1$ suffices to boost the posterior probability of an intercalation to $\beta = 0.85$ and the posterior probability of the High Chronology to 0.91, while the other three chronologies are limited to posterior probabilities below 0.04. Second, confirmation of an additional intercalation ($\alpha = 1$) would render the Middle Chronologies utterly implausible.*

The results are quite dependent on the observed miss counts. With the 2010 data set of Tab. 4, the Ammiṣaduqa count is more strongly in favor of the High Chronology, and correspondingly the posterior probability of the latter is as high as 0.91 already for $\alpha = 0$, and the posterior probabilities of the other three chronologies are all below 0.05.

**Prior and posterior probabilities of an additional intercalation:**

| prior α | 0.000 | 0.050 | 0.100 | 0.500 | 1.000 |
|---|---|---|---|---|---|
| posterior β | 0.000 | 0.731 | 0.852 | 0.981 | 1.000 |

**Posterior probabilities of chronologies:**

| prior α | | 0.000 | 0.050 | 0.100 | 0.500 | 1.000 |
|---|---|---|---|---|---|---|
| HC | −1701 | 0.460 | 0.843 | 0.906 | 0.973 | 0.983 |
| MC | −1645 | 0.142 | 0.038 | 0.021 | 0.003 | 0.000 |
| LMC | −1637 | 0.256 | 0.069 | 0.038 | 0.005 | 0.000 |
| LC | −1581 | 0.142 | 0.050 | 0.035 | 0.018 | 0.016 |

**Tab. 7** Ammiṣaduqa and Ammiditana data, 2013 miss counts of Tab. 5. Posterior probabilities of the four main chronologies.

### 5.3 Case 3: Hammurabi-Samsuiluna

For the Hammurabi-Samsuiluna segment I repeated the analysis of 1982,[28] comprising 54 month-lengths, but used newer astronomical programs. The intercalations are highly irregular: from Hammurabi year 32 to 36 the New Year longitude increases by 72°. The consequence is that for each candidate chronology we must consider at least three different seasonal alignments. Some misgivings about possibly misplaced intercalations remain.[29] If we treat the data as independent evidence, the High chronology (–1701) again comes ahead. Its miss rate of $20/54 = 37\%$ is compatible with that of a correct chronology, but unpleasantly high and therefore offers only weak supportive evidence.

### 5.4 Case 4: Ur III

For the Ur III period the situation is more complex, and I shall discuss some of the problems. If these problems can be resolved, the Ur III month-length data might attain decisive chronological relevance.

By 1986 we had a total of 228 month-lengths. In 2013 this number was modestly increased to 240. What I shall discuss here is my more comprehensive analysis of the smaller earlier set, but using newer programs. Among the different parts of the data, the Drehem segment from Amar-Sin to Ibbi-Sin ($n = 126$) probably is to be trusted most. With the Umma segment from Amar-Sin to Ibbi-Sin ($n = 60$) there are doubts about the intercalations,[30] and with the Šulgi segment from year 39 to 48 ($n = 42$) there are serious doubts about the calendar.

The relative chronology from the beginning of the Ur III dynasty to the end of the Hammurabi dynasty is well established. By reckoning back from the four main Venus chronologies one obtains for Amar-Sin year 1 = –2099, –2043, –2035, –1979.[31] We stay on the safe side by assuming that the true date is within ±10 years of the back-reckoned dates, and that the New Year syzygy longitude is between 310° and 50°. Then we obtain about 75 feasible alignments for each of the four chronologies: a range of 21 years for the chronology and a little more than 3 months for the season. The four chronologies together give a total of 252 feasible alignments (there is an overlap for the middle chronologies).

A simple calculation with the binomial distribution shows that if we are considering the best of 75 alignments, we need over 70 month-lengths such that the correct chronology has an even chance of sticking out, and if we want it to stick out with 90% probability, we need about 180 correctly distanced month-lengths. In any case, the Umma and

28 Huber, Sachs, et al. 1982.

29 See Huber, Sachs, et al. 1982, 36.

30 See Huber, Sachs, et al. 1982, 38.

31 See Sallaberger 2013, who points out that independent reconstructions suggested uncertainties in the range of ±1 year.

Šulgi segments are too small to be used for independent dating on their own – that is, unless we encounter extraordinarily lucky low miss counts. See also the discussion of the NB data in Section 4.

Among 8000 alignments between –2213 and –1567, the absolutely best fit of the 228 month-lengths gave 84 misses (with the earlier programs 83 misses)[32] and was obtained for three chronologies (Amar-Sin year 1 = –2093, –2020, or –1775). The first is inside a feasible window, corresponding to the High chronology. Among the 252 feasible alignments, by chance the 7 best of them happened to contain representatives from all 4 main chronologies. The 6 alignments matching Middle or Low chronologies had miss counts between 88 and 90.

The best obtained miss number is unpleasantly high ($84/228 = 36.8\%$). However, the Drehem subset from Amar-Sin year 1 to Ibbi-Sin year 2 for the same alignment (Amar-Sin Year 1 = –2093, syzygy number of month I $= -13\,516$) gives a miss rate close to the NB value ($43/126 = 34.1\%$), and the Umma subset even a lower one ($18/60 = 30\%$). On the other hand the Šulgi segment contributed an extraordinarily high number of misses to the total, namely 23 out of 42 months. Note that this rate, $23/42 = 54.8\%$, lies even above the rate expected for a wrong chronology, and the probability that a correct alignment produces 23 or more misses is merely 0.3%. But by aligning the Šulgi segment 5 months earlier, the number of misses was reduced from 23 to 14. Through this hypothetical modification the miss rates were reduced to the NB value: namely for the Šulgi segment to $14/42 = 33.3\%$ and for the full set to $75/228 = 32.9\%$. With this modification, the fit of the –2093 chronology, giving 75 misses, was far superior to the best of the other feasible alignments (86 misses for the –2037 and –1979 chronologies). In any case, the original Šulgi segment appeared to be the odd man out, and I wondered whether Šulgi's years began in fall.

The calendars of Drehem and Umma were not synchronized, and several intercalations differ.[33] The intercalary months usually, but not always, were inserted at the end of the year. Sometimes a 13th month was used by the scribes as a placeholder for the first month of the next year, if the name of the new year was not yet known to them.

Wu Yuhong distinguishes between two different calendars used in Ur III times.[34] The month-names of what he calls the Mašda calendar were *i:* iti-maš-dà-gu$_7$, *ii:* iti-šeš-da-gu$_7$, *iii:* iti-u$_5$-bí-gu$_7$, *iv:* iti-ki-siki-$^{d}$Nin-a-zu, *v:* iti-ezem-$^{d}$Nin-a-zu, *vi:* iti-á-ki-ti, *vii:* iti-ezem-$^{d}$Šul-gi, *viii:* iti-šu-eš-ša, *ix:* iti-ezem-maḫ, *x:* iti-ezem-an-na, *xi:* iti-ezem-Me-ki-gál, *xii:* iti-še-kin-kud. However, at least in Šulgi years 44–48, an alternative so-called Akiti calendar was in use, where the year began in fall, with month *vi:* iti-á-ki-ti of the Mašda calendar. This would seem to give a posterior justification to the experi-

32 See Huber 1987, 14–15.

33 This was noted already by Huber, Sachs, et al. 1982, 38. See now Wu Yuhong 2002 for a detailed investigation.

34 Wu Yuhong 2000.

mental 5-month shift I had applied to the Šulgi data (the years 44–48, where the Akiti calendar had been in use, contain about three quarters of the Šulgi month-lengths available to us). From Amar-Sin on, the Mašda calendar was in use.

Now, what should we do: keep the original Šulgi data, shift them by 5 months, or ignore them? Either way, the Ur III data provide additional, admittedly not quantifiable support for the High chronology. The Middle chronologies give poorer fits. But the Ur III calendars and their synchronization clearly need more investigation before they can be fully trusted for the purposes of dating.

## 6 Summary: internal consistency and coherence of the results

The Ammiṣaduqa month-length data show the pattern to be expected from one correct and three wrong chronologies, see Figs. 3–4, and they point toward correctness of the High chronology (–1701). The Ammiditana data show the same behavior, see Fig. 5. The discussion of Tab. 5 in Section 5.2 provides strong evidence in favor of the High chronology and against the Middle chronologies. Clean quantitative results are obtained by forming a working hypothesis on the basis of the Ammiṣaduqa data and then testing it with the Ammiditana data. This approach allows to affirm the High chronology on at least the 5% level, and if we are willing to lower ΔT by 3 minutes against the arbitrarily assumed default formula, it is confirmed even on the 1% level. By a Bayesian argument it can be shown that the High chronology is roughly 25 times more probable than each of the other three main chronologies (Tab. 7).

The Hammurabi-Samsuiluna and the Ur III data support these results, even if their reliability might be questioned. In addition, the Simānu eclipse of EAE 20, commonly thought to refer to the death of Šulgi, can be identified with the lunar eclipse of –2094 July 25, just one year before the date –2093 of Amar-Sin year 1 suggested by the month-lengths. None of the other possible identifications of that eclipse fall within one of the time windows implied by the Venus chronologies.[35]

The test performed in Section 5.2.1 implies that also for Old-Babylonian times the miss rate for a correct chronology is substantially below that for a wrong chronology. The miss counts corresponding to the High chronology, as shown in Figs. 3, 4, 5, and 6, all approximately correspond to the 33% Neo-Babylonian rate. This does not prove that the OB rates for correct chronologies are equal to the NB rates, but at least they do not contradict such an assumption.

35 See Huber 1999/2000, 77, for a list of alternative identifications (the next eclipses matching the description in the omen are in –2018, –2007, –2001, –1936).

In my opinion, internal coherence of the results is an even stronger indication of their trustworthiness than any statistical significance assertions. And anyone desiring to defend one of the Middle chronologies, rather than ignore the opposing month-length evidence, or simply discount it as being the only witness in favor of the High chronology, should better find plausible arguments discrediting the evidence of Figs. 3, 4, 5, and 6. In order to be convincing, such arguments would seem to require new data. They might be based on a large, reliable set of contradictory new month-length data, or on new eponym lists bridging the interval between Old Assyrian and Neo-Assyrian times.

## 7 Modeling and simulation of crescents and month-lengths

In statistics, the principal purpose of modeling and stochastic simulation quite generally is to obtain crude estimates of the statistical variability of various empirical measurements. The models are designed to give a satisfactory phenomenological description of the situation. Whether they can give a causal explanation is a more difficult and possibly unanswerable question. Here are the facts and assumptions on which we shall base the models.

For randomly chosen wrong chronologies the agreement/disagreement rates between observed and calculated month-lengths are fixed by astronomical theory, that is by the length of the synodic month (29.5306 days): 53% of the months have 30 days, 47% have 29 days. It follows that a collection of 30-day months, when aligned at random along a calculated sequence, has an expected miss rate of 47%.

An approximate estimate of the variability of empirical miss rates then can be obtained from the binomial distribution for which the miss counts have the standard deviation $\sqrt{n\,p\,(1-p)}$. An alternative, perhaps more reliable version can be found by aligning a batch of observed 30-day months at many positions along a calculated sequence of such months. See Fig. 2 for a comparison between the two approaches.

For a correct chronology the LB astronomical texts give a rock bottom lower limit of about 10% for the rate of discrepancies between observed and calculated month-lengths. I used the calculated sequence of 33 000 months to check the effects of the pure gray-zone model (with $d = 1.1^\circ$). The probability of seeing the crescent 1 day early was 2.56%, and that of seeing it 1 day late was 2.51%. Months never were shortened to 28 days by gray-zone effects, but occasionally they were lengthened to 31 days. With calculated 29-day months, lengthening to 31 days happened in 0.06% of the cases, with 30-day months in 0.23% of the cases (that is, about once in a human life-time). Calculated 29-day months were lengthened to 30 days in 10.4% of the cases, and 30-day months were shortened to 29 days in 9.4% of the cases. This corresponds to the 10% miss rates of the LB astronomical texts.

The NB administrative texts give a higher discrepancy rate. As mentioned in Section 4 there are 149 NB texts dated to day 30, and 49 of them occur in a month that according to calculation has 29 days. The discrepancy rate thus is approximately $29/149 =$ 33%. Up to 27 of these 49 discrepancies might be explainable by 'gray-zone' effects of early or late sightings, namely those for which $|\Delta h| \leqslant 1.1°$, but at least 22 discrepancies must have a different cause. Note that at the begin of a month lengthening can occur only because of a fortuitous early sighting in the range $-1.1 \leqslant \Delta h \leqslant 0$, while at the end poor weather or sheer lack of care might cause a delay with values of $\Delta h$ larger than $+1.1$.

I propose a simple two-component model. One component corresponds to the 'gray-zone' model of the astronomical texts, and the other to a practice of 'overhang dating' or 'double dating' (a term preferred by Michael Roaf) by the ancient scribes: when dating a text they would occasionally write day 30 in cases where they more properly should have written day 1 of the next month. The consequences of such a model shall be developed in Section 8 (following next). Evidence for the presence of overhang dating is contained in Sallaberger's remark,[36] according to which Amar-Sin year 5 contained 9 day-30 dates, instead of the expected 6. In other words, we can have more day-30 dates than are astronomically possible. Note that variability caused by 'gray-zone' effects would stay on in the calendar, while 'overhang' effects would not. If the officials responsible for the calendar should decide that the preceding month had had only 29 days, the scribe simply would skip a day and let day 30 be followed by day 2.

A letter to an Assyrian king (presumably Assurbanipal) has the remarkable passage:

> I observed the (crescent of the) moon on the 30th day, but it was high, too high to be (the crescent) of the 30th. Its position was like that of the 2nd day. If it is acceptable to the king, my lord, let the king wait for the report of the Inner City before fixing the date.[37]

This letter is interesting because it shows that the beginning of the month could be fixed retroactively, and possibly the length of the preceding month even could be shortened to 28 days.

A possible argument against this simple overhang model is that a (preliminary) investigation of Ur-III-time month-lengths based on regular deliveries did not seem to give a substantially better agreement with calculation than those based on day-30 dates.

We do not know when and why the scribes would use overhang dating, but we can crudely estimate how often it may have occurred in the NB material. In Section 4, I had

36 Sallaberger 1993, 12.

37 Parpola 1993, Letter 225.

estimated that overhang might occur between 40% and 50% of the cases. The theoretical model of the next section gives the best fit when assuming an overhang probability $p_{ov} = 0.46$.

Overhang dating according to the model just described raises a problem: if many scribes independently use it, then every true hollow month ultimately will acquire some overhang dates. But true full months will obtain day-30 dates more often than true hollow months. In this case the proper solution is to count dates with their observed multiplicity. On the other hand, whenever multiple dates originate in the same scribal office, they are strongly dependent and one should count them only once. It is difficult to separate these cases. Here, I acted as if the second case applied and counted multiple occurrences only once (with the presently available material they are relative rare anyway).

## 8 Theoretical miss rates

Independently of the cause of the discrepancies between calculated and observed month-lengths, the calculation of the miss rates of day-30 dates is, essentially, a straight exercise with conditional probabilities.

The miss rate in question is the conditional probability, given a recorded day-30 date (D30), that the underlying month calculates as a 29-day month (C29):

$$p_{\text{miss}} = \text{P}(\text{C29}|\text{D30}) = \frac{\text{P}(\text{C29 \& D30})}{\text{P}(\text{D30})}.$$

We have

$$\text{P}(\text{C29 \& D30}) = \text{P}(\text{D30}|\text{C29})\,\text{P}(\text{C29}),$$
$$\text{P}(\text{D30}) = \text{P}(\text{D30}|\text{C29})\,\text{P}(\text{C29}) + \text{P}(\text{D30}|\text{C30})\,\text{P}(\text{C30}).$$

Here

$$\text{P}(\text{C29}) = 0.47,$$
$$\text{P}(\text{C30}) = 0.53.$$

If we assume zero width for the gray zone, and that overhang occurs at random with probability $p_{ov}$, and if we assume that dates higher than day 30 are not permitted, then

$$\text{P}(\text{D30}|\text{C29}) = p_{ov},$$
$$\text{P}(\text{D30}|\text{C30}) = 1,$$

and we can substitute these values into the above formula for $p_{\text{miss}}$.

Now assume a gray zone with finite width, such that in the absence of overhang the miss rate for month-lengths is $\mu = 0.1$.

Then, the last two probabilities are changed to

$$P(D30|C29) \;=\; p_{ov} + (1 - p_{ov})\,\mu,$$
$$P(D30|C30) \;=\; 1 - (1 - p_{ov})\,\mu.$$

The justification for these formulas is as follows.

- Given that there is overhang in that particular month, then $P(D30|C29) = 1$, and given that there is none, then $P(D30|C29) = \mu$.
  If overhang occurs with probability $p_{ov}$, then we obtain the first of the above formulas.
- Given that there is overhang in that particular month, then $P(D30|C30) = 1$, and given that there is none, $P(D30|C30) = 1 - \mu$.
  If overhang occurs with probability $p_{ov}$, then we obtain the second of the above formulas: $P(D30|C30) = p_{ov} + (1 - p_{ov})(1 - \mu) = 1 - (1 - p_{ov})\,\mu$.

I believe the most questionable assumption in the above arguments is that overhang occurs at random (i.e. independent of $\Delta h$). It appears at least plausible that overhang is more likely to occur for small values of $\Delta h$ than for large ones. But the NB material does not really support such a conjecture, see Tab. 3. It shows a clear cluster of values in the gray zone ($\Delta h \leqslant 1.1$). In the range between 1.6 and 6.8 the number of $\Delta h$ values shows a moderate decrease, but this decrease seems to go in parallel with a decrease in the number of $\Delta h$ values calculated for 29-day months.

The following probabilities were calculated with the above model, on the basis of 33 000 calculated month-lengths, not on the binomial distribution, by applying the model to a large number of randomly chosen subsets of the calculated sequence. The overhang probability was empirically adjusted to $p_{ov} = 0.46$, so that the combined model approximately reproduced the miss rate of $50/153 = 0.327$ of the Neo-Babylonian material.

For the purpose of these modeling calculations I assumed for the gray zone model:

- if $\frac{\Delta h}{d} < -1$, the crescent is never seen;
- if $-1 \leqslant \frac{\Delta h}{d} \leqslant 1$, the crescent is seen with probability $(1 + \frac{\Delta h}{d})/2$;
- if $\frac{\Delta h}{d} > 1$, the crescent is always seen,

with $d = 1.1°$ (in 1982 I had used $d = 1.0°$).

Pure overhang model (overhang probability $p_{ov} = 0.46$):

Miss rate $P(C29|D30) = 0.290$

Pure gray zone model (zone width $d = 1.1°$):

$P(D30|C29) = 0.104$
$P(D30|C30) = 0.906$
Miss rate $P(C29|D30) = 0.092$

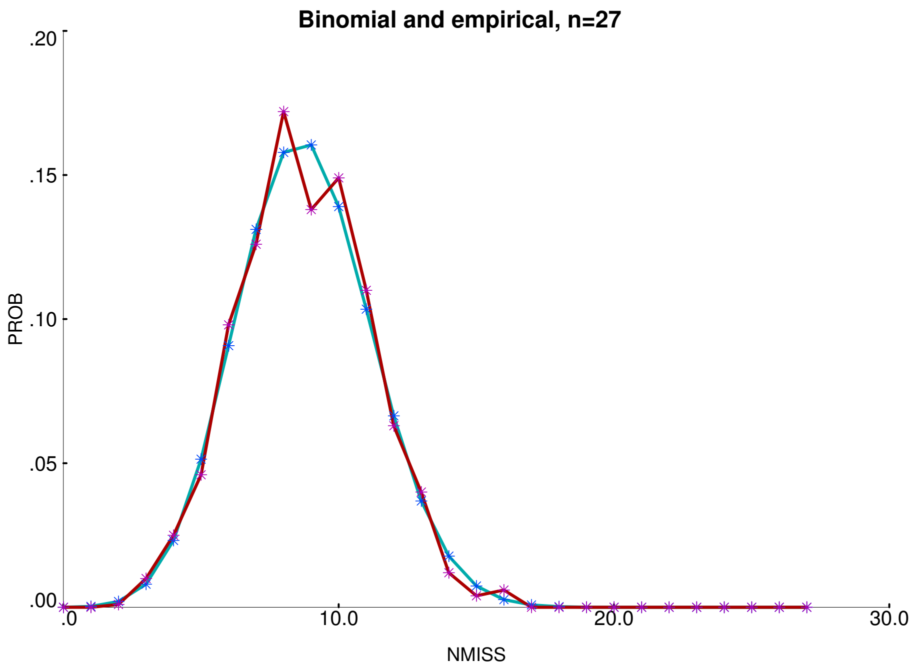


**Fig. 7** Comparison between the binomial distribution ($n=27,\ p=0.325$, blue) and the empirical frequencies obtained from 1000 samples based on the overhang model ($d=1.1°,\ p_{ov}=0.46$, red).

Combined model ($p_{ov} = 0.46,\ d = 1.1°$):

$P(D30|C29) = 0.516$

$P(D30|C30) = 0.949$

Miss rate $P(C29|D30) = 0.325$

The sequence of calculated month-lengths is not quite random, and therefore the distribution of the miss counts does not necessarily follow a binomial distribution. In the case of the wrong chronologies, it had been possible to compare the binomial distribution with the results of a large number of alignments (Fig. 2). The case of the correct chronology is less straightforward, but we can compare the binomial distribution with the results of the simulated error model. Also here the binomial distribution gives a very good approximation. Fig. 7 shows a comparison between the empirical frequencies based on the above overhang model ($d=1.1°,\ p_{ov}=0.46$) and the binomial distribution ($n=27,\ p=0.325$). The empirical frequencies were obtained by drawing 1000 random samples of size 27 from the calculated sequence of months and then applying random gray zone and overhang effects.

## 9 Sensitivity to the clock-time correction ΔT

A central problem of historical astronomy is our insufficient knowledge of the clock-time correction $\Delta T = ET - UT$ (the difference between the uniform time scale ET underlying the astronomical calculations and civil time UT depending on the irregular rotation of the earth). Because of tidal friction ΔT increases quadratically with time, but it is subject to sizable random fluctuations. By now, it is reliably known back to 700 BC within a standard error of approximately 5 minutes, but its extrapolation from there to 2000 BC is affected by a standard error of about 1 hour.[38]

For the present paper I have assumed a formula proposed by Morrison and Stephenson (in a paper published in 1982) as my default:[39] $\Delta T = c\,t^2$ sec, with $c = 32.5$ and $t$ measured in centuries since AD 1800, together with lunar orbital acceleration $\dot{n} = -26''/cy^2$. I made this choice for three reasons: first, because of its simplicity, second, because calculations based on it agree very closely with the traditional tables by P. V. Neugebauer and Tuckerman,[40] and third, last but not least, if the solar eclipse of Sargon of Akkad has been correctly identified, it implies that ΔT in the mid-24th century was between −20 and +7 minutes of that default.[41] For that time this corresponds to a range of $c$ between 31.7 and 32.8. Moreover, this solar eclipse now would seem to determine the clock-time correction ΔT with a standard error of the order of 10–15 minutes back to the 24th century BC. If the Ugarit eclipse of –1222 has been correctly identified, perhaps 10 minutes higher values of ΔT, corresponding to values of $c$ that are about 0.5 higher, may hold for the Ur III and OB periods.[42]

I found it convenient to vary ΔT by modifying the parameter $c$; note that a change of 1 unit in $c$ by –1700 amounts to a change of 20 minutes in ΔT. I believe that the most probable range of $c$ is between 31.5 and 33.5, but for the sensitivity study depicted in Figs. 8, 9, 10, 11, and 12, I have applied a range of $c$ between 27 and 38, that is of approximately ±2 hours for the Ur III and OB periods. These figures are based on the data available in 2013.

One of the questions to be addressed by this sensitivity study was whether perhaps the sensitivity of the miss counts to ΔT was such that that ultimately they might be used to improve our estimates of ΔT.

38 Huber 2006.

39 Morrison and Stephenson 1982.

40 P. V. Neugebauer 1929; Tuckerman 1962; Tuckerman 1964.

41 Huber 2012; Morrison and Stephenson 1982. The formula more recently proposed by the latter authors in 2004 for extrapolation beyond –700, namely $\Delta T = -20 + 32t^2$ sec, with $t$ in centuries since AD 1820, differs only negligibly: the difference in ΔT increases from 0 in –600 to 3 minutes in –1700 and 6 minutes in –2400 (Morrison and Stephenson 2004).

42 Huber 2012, Fig. 3, showing the deviations from the default ΔT and their variability.

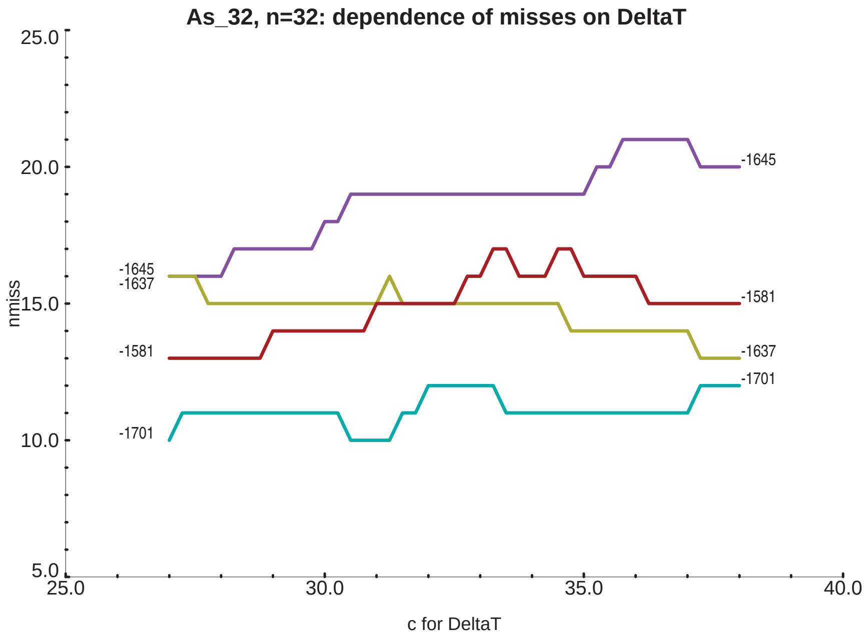


**Fig. 8** Sensitivity of miss counts to ΔT, Ammiṣaduqa data; $n = 32$.

We note that in the figures for the Ammiṣaduqa data (Fig. 8), for the Ammiditana (+1) data (Fig. 10), and for their combination (Fig. 12), the default ΔT ($c = 32.5$) yields a local maximum of the miss counts for the High chronology, with a local minimum for $c$ between 30.5 and 31.25. This minimum is reached by a decrease in ΔT of about 25 minutes. I was almost tempted to derive an improved estimate of ΔT for the OB period from this. At the same time, lowered values of the miss counts would much improve the rejection levels in Section 5.2, see in particular Section 5.2.1. However, I do not intend to insist on these arguments.

But in any case, this sensitivity study shows that our default choice for ΔT, by leading to a local maximum for the miss counts, happens to be conservative.

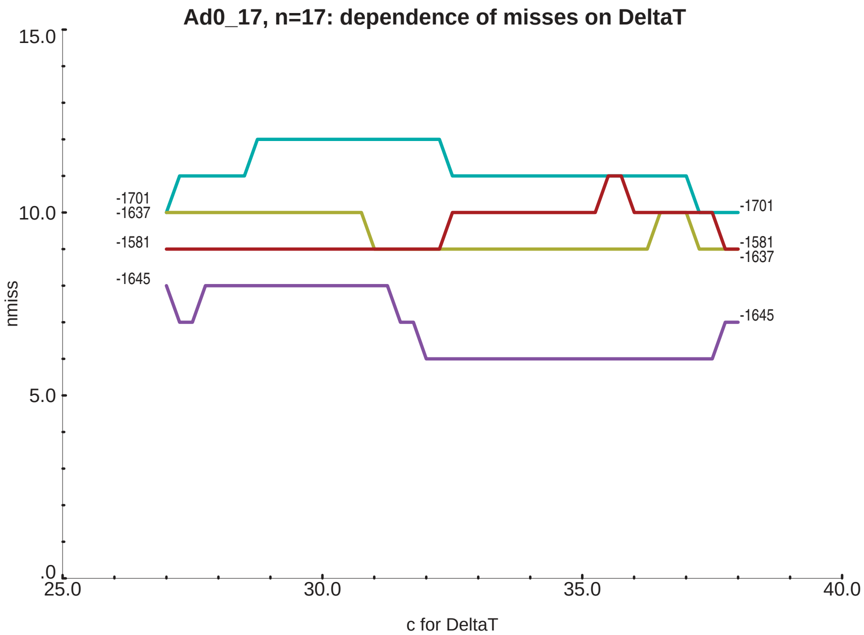


Fig. 9 Sensitivity of miss counts to ΔT, Ammiditana data, +0 intercalation; $n = 17$.

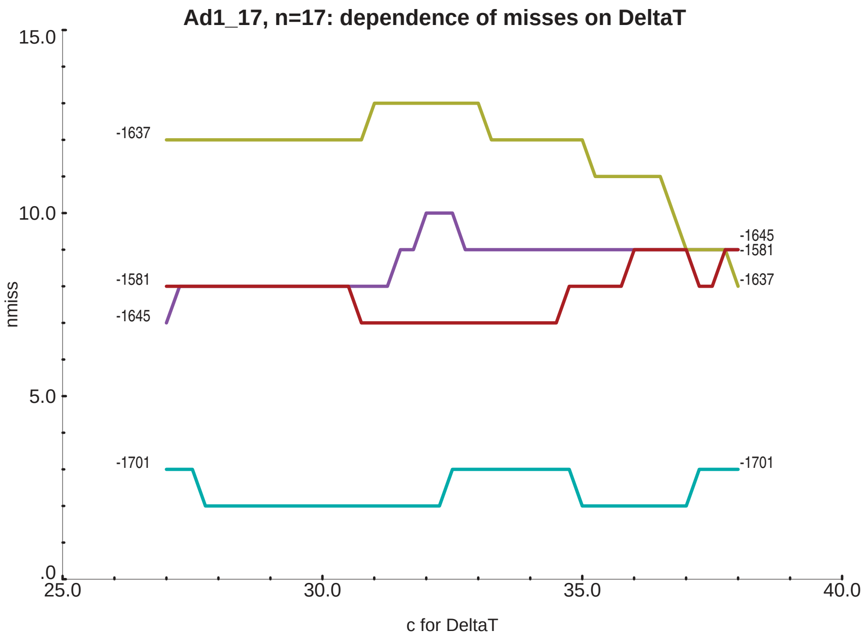


Fig. 10 Sensitivity of miss counts to ΔT, Ammiditana data, +1 intercalation; $n = 17$.

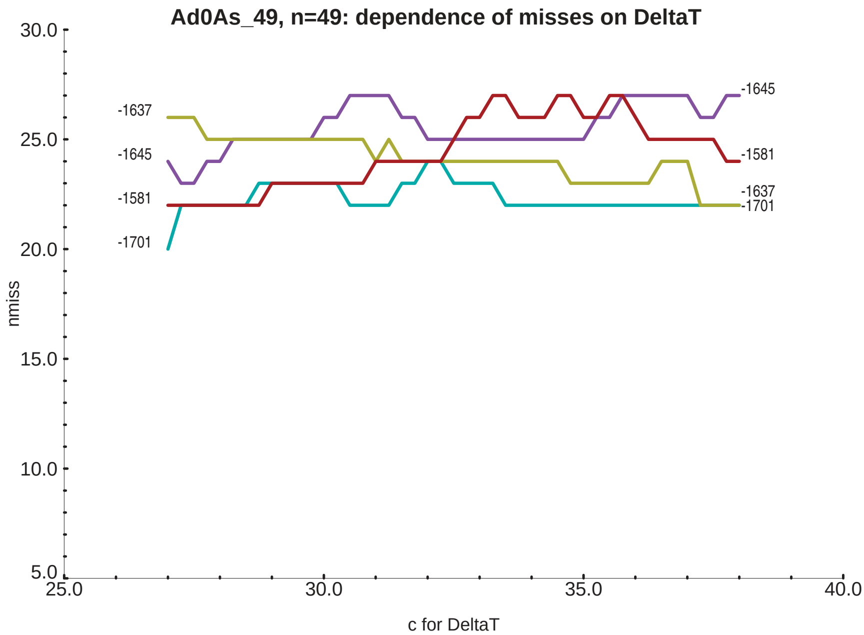


**Fig. 11** Sensitivity of miss counts to ΔT, Ammiditana-Ammiṣaduqa data, +0 intercalation; $n = 49$.

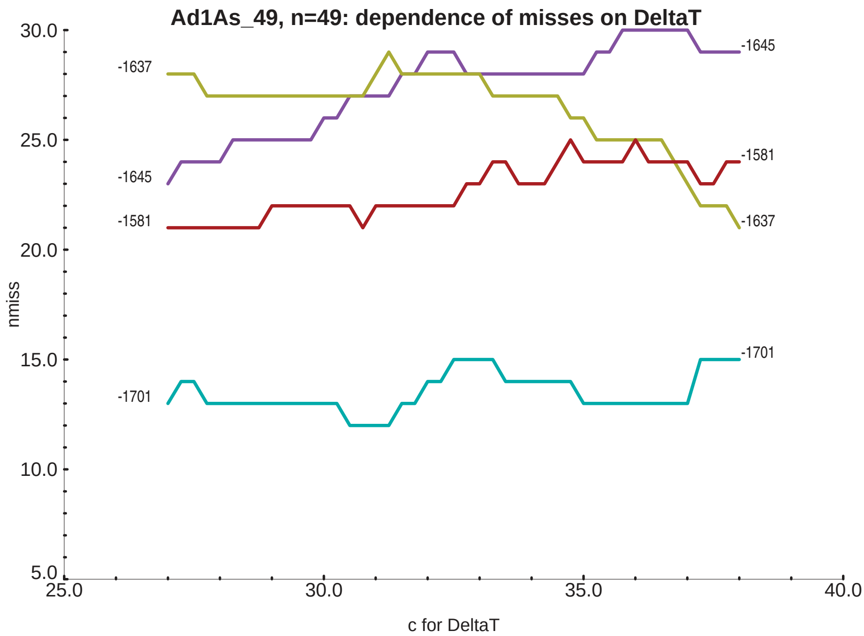


**Fig. 12** Sensitivity of miss counts to ΔT, Ammiditana-Ammiṣaduqa data, +1 intercalation; $n = 49$.

# 10 Appendix: the underlying data base

## 10.1 Intercalations during the reign of Ammiṣaduqa

The connection of the Venus text with the reign of Ammiṣaduqa had been established by Kugler in 1912, when he identified the year name 'Year of the Golden Throne' occurring in the 8th year of the Venus text with the name of the 8th year of king Ammiṣaduqa.[43] Since then, some doubts about the conclusiveness of the identification have been voiced (there are other year names involving a Golden Throne), but we now can establish the connection beyond doubt with the help of the intercalations.

The Venus text has first visibilities of Venus in the morning of Year 1 XI 18 and of Year 17 XII 14, thus spaced 16 years and 1 month. Between these dates there are 10 synodic periods of Venus, corresponding to 5840 days or 198 synodic months. As 16 lunar years contain only $16 \times 12 = 192$ months, there must be 5 intercalations between these two dates. In order to obtain the correct spacing between Venus phenomena, the 5 intercalations in question must have occurred as:

(1) (4A or 4U), (5U), (9A or 10U), (11U attested), (13U or 13A or 14U).

Here, A stands short for a second Addāru ($XII_2$), U for a second Ulūlu ($VI_2$).[44] On the other hand, contemporary administrative documents attest the following intercalations for the first 16 years of Ammiṣaduqa:

(2) 4A, 5U, 10U, 11U, 13A.

In addition, they attest a 17A. Note that texts from Sippar Amnanum show that the years previously provisionally denoted $17 + a$ and $17 + b$ can be identified with the years 17 and 18.

The probability that an agreement as good as that between (1) and (2) occurs by chance is less than 1 in 1000. This can be calculated as follows. There are $\frac{15\times14\times13\times12\times11}{6\times2}$ $= 30\,030$ possibilities for placing 3 U tokens and 2 A tokens in 15 slots (the 15 years from 2 to 16). However, not all are feasible. Intercalations are inserted to keep the years in step with the agricultural seasons, and on average a regular year decreases the New Year longitude by 10.7°, while an intercalary year increases it by 18.4°. If we only permit intercalation patterns that keep the difference between the maximal and minimal New Year longitude below 45° or 50° – for the actual Ammiṣaduqa intercalations this difference is 44.5° – merely between 20% or 30% of the possibilities remain feasible. Among the 12 patterns of intercalations made possible by the Venus text, 4 satisfy the requirement

43 Kugler 1912.

44 Following the convention of Parker and Dubberstein 1956, 6.

that there are 3 U and 2 A tokens. Thus, the probability of hitting by chance a pattern compatible with the Venus text is approximately 4 in 6000 trials, that is 0.0007.

This has important consequences. It shows that the Venus text refers to the time of Ammiṣaduqa, and that the traditional year count of the Venus tablet agrees with years 1 to 17 of that king. Moreover, we know that we have a complete list of all intercalations of years 1 to 17, except that perhaps an intercalation 1U might be missing.

Tab. 8 lists the intercalations attested or implied by the Venus Tablet, and those attested by contemporary contracts. Unpublished data mentioned in LFS are highly unreliable; among them, 5U has now been confirmed by the Cornell text CUSAS 8 55,[45] while 14U in all probability is wrong. The second-but-last column counts the number of months preceding the beginning of the year, and the last column gives the deviation of the New Year syzygy longitude from that obtained for Year 5 (which for all chronologies within $1^\circ$ corresponds to the median value).

Year 18 is missing in the Venus text, and years 19–21 constitute the highly questionable 'Section III' of the text. We shall ignore evidence derived from that section of the Venus text.

Seth Richardson points out that the names of years 13 and 17 are almost indistinguishable, so some texts may have been misclassified. This should not create problems with regard to intercalations (both years have a second Addāru), but might do so with regard to month-lengths.

### 10.2 Intercalations during the reign of Ammiditana

The intercalations in Tab. 9 are attested for the 37 years of Ammiditana.[46] The last column gives an arbitrary count of month numbers preceding the begin of the year (as in Section 10.1 above).

For the first 21 years of Ammiditana only 4 or 5 intercalations are attested, whereas the expected average is 7 in 19 years, so some appear to be missing. For the last 16 years (years 22 to 37) 8 intercalations are attested. There is a surprising sequence of 4 consecutive intercalary years (25 to 28), and even if we delete the improbable month $XI_2$(!?) in year 25, we are still slightly above the expected average. For 25 $XI_2$ the text has ITI $ZIZ_2$ DIRI(= SI.A) instead of the expected ITI $ZIZ_2$.A, and we can assume that this is a scribal error. So it is possible that we have the full pattern of intercalations for the years 22 to 37.

45 Search for this text in http://www.archibab.fr/4DCGI/recherche5.htm, under Mois: 6-bis, Roi: Ammiṣaduqa, Année: 5 (visited on 17/7/2017).

46 From Huber, Sachs, et al. 1982.

| Year | Venus Tablet | Contracts | MNU | NYL |
|---|---|---|---|---|
| 1 | | | 0 | 12° |
| 2 | | | 12 | 2° |
| 3 | | | 24 | –8° |
| 4 | A or U implied | A VAS 7 76; BM 17563 | 36 | –18° |
| 5 | U implied | U CUSAS 8 55 | 49 | 0° |
| 6 | | | 62 | 18° |
| 7 | | | 74 | 7° |
| 8 | | | 86 | –4° |
| 9 | 9A or 10U | | 98 | –14° |
| 10 | 9A or 10U | U YOS 13 532; BE 6/1 106; BM 81130; BM 26602 | 110 | –24° |
| 11 | U attested | U CT 8 3a; BM 81350 | 123 | –6° |
| 12 | | | 136 | 12° |
| 13 | 13U or 13A or 14U implied | A YOS 13 404; TLB 1 211; BIN 7 208-9; BM 78461; BM 79435; BM 81396; BM 81747; Dalley, Edinb. No.20; OLA 21 no.69; CUSAS 8 13 | 148 | 1° |
| 14 | | (U LFS unpublished) | 161 | 19° |
| 15 | | | 173 | 9° |
| 16 | | | 185 | –2° |
| 17 | | A TCL 1 171; BAP 9; VAS NF II 99; YOS 13 53; BM 79010 | 197 | –13° |
| 18 | | | 210 | 5° |
| 19 | U attested | U YOS 13 146 | 222 | –5° |
| 20 | A or U implied | | 235 | 13° |
| 21 | | | | |

**Tab. 8** Intercalations attested or implied by Venus Tablet and those attested by contemporary contracts. Previously uncertain year names: $17 + a = 17$; $17 + b = 18$; $17 + c = 2$; $17 + d = 19$. For $17 = 17 + a$ and $18 = 17 + b$, see Nahm 2014.

| Yr | Type | Text | MNU |
|---|---|---|---|
| 1 | regular | | |
| 2 | regular | (cf. BE 6/1 82:14–22. Between Ae 28 III 30 and Ad 5 XII 30 there are 5 years and 10 months.) | |
| 3 | regular | | |
| 4 | $XII_2$ | BE 6/1 91; YOS 13 205 | |
| 5 | | | |
| 6 | | | |
| 7 | | | |
| 8 | | | |
| 9 | | | |
| 10 | $XII_2$ | PBS 8/2 202; AO 8126; BM 17313; BM 78465 | |
| 11 | ($XII_2$ | LFS unpubl.) | |
| 12 | | | |
| 13 | $XII_2$ | BM 22522 | |
| 14 | $XII_2$ | YOS 13 1 = HSM 48 (coll. Moran) | |
| 15 | | | |
| 16 | | | |
| 17 | | | |
| 18 | | | |
| 19 | | | |
| 20 | | | |
| 21 | | | |
| 22 | $XII_2$ | YOS 13 197; PSBA 34 24; see YOS 13 p.1 and JCS 13 39a | 799 |
| 23 | | | 812 |
| 24 | | | 824 |
| 25 | $XI_2$(!?) | YOS 13 272 | 836 |
| 26 | $XII_2$ | CT 6 39a = BM 80596; BM 16684(?, coll. Walker) | 848 |
| 27 | $XII_2$ | BE 6/2 109 | 861 |
| 28 | $XII_2$ | BM 80977 | 874 |
| 29 | | | 887 |
| 30 | | | 899 |
| 31 | | | 911 |
| 32 | $XII_2$ | BM 78668; BM 16535; MHET 1/1 11 | 923 |
| 33 | $XII_2$ | BE 6/2 112 | 936 |
| 34 | | | 949 |
| 35 | | | 961 |
| 36 | | | 973 |
| 37 | $XII_2$ | Kraus, Edikt 28 3′; RA 63,48 37–39; YOS 15 72; BM 79897 | 985 |

**Tab. 9** Intercalations attested for the reign of Ammiditana.

However, the intercalary pattern is highly irregular. There are 3 consecutive intercalations in the years 26–28. I do not think that in the immediately preceding or following 3 years intercalations are missing, but for the 7 years between Ammiditana 34 and Ammiṣaduqa 3 only a single intercalation is attested. Note that on average a regular year decreases the New Year solar longitude by 10.7°, while an intercalary year increases it by 18.4°. Thus, the 3 intercalary years 26–28 increase the New Year longitude by 55°, while the 7 years from year 34 on, containing 6 regular and 1 intercalary years, decrease it by 46°.

Given the irregular pattern of intercalations, the lack of attested intercalations between the years 15–21, and the wide spread of the New Year longitudes (their range is 58° for Ammiditana, 44° for Ammiṣaduqa), we cannot exclude the possibility that near the border between the Ammiditana and Ammiṣaduqa blocks an unattested intercalation is missing, for example a $XII_2$ in Ammiditana Year 36, or a $VI_2$ in Ammiṣaduqa Year 1. This choice shifts the entire block of attested month-lengths (from 24 IV to 36 XII) together by 1 month. We should keep the possibility of an additional intercalation in mind.

### 10.3 The Ammiṣaduqa month-lengths

The month-lengths in Tab. 10 are attested in contracts from the reign of Ammiṣaduqa, years 1–19.

The list is taken from *Astronomical Dating of Babylon I and Ur III*,[47] 21 months with 11 later additions (6 from Marten Stol, between 1982 and 2010, and 5 from Seth Richardson in 2013, the latter with superscript R and noted with n for 'new' in the last column). Later on, too late to be used in the calculations, Michael Roaf supplied a table with month-lengths (mostly collated by Frans van Koppen); it was merged into the list in Tab. 10. For the 32 entries used in the calculations, the MNU column gives the month number, arbitrarily counted from month I of Year 1 (assuming that year 1 is regular).

Roaf noted: BM 92520 (Meissner BAP 107) and CBS 01346 (Van Lerberghe Mél De Meyer 159–168) are duplicates. VS 7 109 is dated As 16-02-08 but line 3 mentions iti $bara_2$-zag-gar $u_4$-30-kam as the date of a recent expenditure. Old Babylonian legal and administrative texts from Philadelphia by Karel van Lerberghe. Note that MLC 1517 (YOS 13 65) has 30 written on top of 2 or vice versa (Year 19 X 2/30) and so is not included in the list in Tab. 10.

47 Huber, Sachs, et al. 1982, 65.

| Year | Month | Day | Texts | MNU | |
|---|---|---|---|---|---|
| 1 | VII | 30 | MAH 16218 (TJDB p.31) | 7 | |
| | VIII | 30 | VAT 06253 (VS 7 68) | 8 | |
| | XII | 30 | BM 78640; BM 79869[R] | 12 | |
| 3 | IV | 30 | BM 92606 | 28 | |
| | VI | 30 | VAT 06380A (VS 7 73) | 30 | |
| 4 | XI | 30 | BM 26350a | 47 | |
| | $XII_2$ | 30 | VAT 06238 (VS 7 76) | 49 | |
| 5 | VIII | 30 | BM 16644[R] | 58 | n |
| | XII | 30 | MLC 1349 (YOS 13 165) | 62 | |
| 6 | VI | 30 | BM 80804 | 68 | |
| 7 | XII | 30 | MLC 0452 (YOS 13 126) | 86 | |
| 11 | II | 30 | BM 80984[R]; BM 97370[R] | 125 | |
| | VII | 30 | BM 97733[R] | 131 | n |
| | IX | 30 | BM 97623 (De Graef, AuOr 20 82f. no. 06) | | |
| 12 | IV | 30 | IM 50423 (Edzard, ed Der no.49) | 140 | |
| | VII | 30 | BM 80896[R] | 143 | n |
| | VIII | 30 | BM 81105 | 144 | |
| 13 | I | 30 | BM 97250[R] | 149 | n |
| | II | 30 | BM 17146 | 150 | |
| | VI | 30 | IM 81586 (Van Lerberghe, Mél. Tanret, 592-594) | | |
| | X | 30 | CBS 01219 | | |
| | XII | 30 | MLC 0828 (YOS 13 220); BM 81677[R]; BM 97495[R] | 160 | |
| | $XII_2$ | 30 | BM 78459[R], BM 81096, CBS 01473 (Van Lerberghe, OB Legal 069) | 161 | n |
| 14 | IV | 30 | BM 79287 | 165 | |
| | VI | 30 | Strasbourg 324 (Frank 28) | 167 | |
| | VIII | 30 | CBS 01734 (JCS 11 p.93) | 169 | |

*(continued on next page)*

*(continued from previous page)*

| Year | Month | Day | Texts | MNU |
|---|---|---|---|---|
| 15 | II | 30 | MLC 0822 (YOS 13 221) | 175 |
| | X | 30 | BM 13596 (RA 69 p.188) | 183 |
| | XII | 30 | BM 80167 (CT 2 18) | 185 |
| 16 | I | 30 | BM 92520 (Meissner BAP 107); VAT 06382 (VAS 7 109); CBS 01346 (Van Lerberghe, Mél. De Meyer, 159–168) | 186 |
| | XI | 30 | CBS 01672 (PBS 14 pl.64 no.1078); VAT 05925, 05938 (Kugler, SSB II p.246) | 196 |
| | XII | 30 | VAT 05391 (VS 7 121); BM 97495 | 197 |
| 17 | XI | 30 | VAT 06287 (VS 7 133) | 208 |
| | XII | 30 | VAT 06224 (VS 7 139); BM 80404 (CT 48 76) | 209 |
| 18 | III | 30 | BM 87292 + 87337 | |
| | V | 30 | BM 81624 (CT 48 78) | 215 |
| | X | 30 | CUNES 51-01-045 (CUSAS 8 40) | |
| 19 | III | 30 | BM 81079[R] | |

**Tab. 10** Month lengths attested in contracts from the reign of Ammiṣaduqa.
Previously uncertain year names: $17 + a = 17$; $17 + b = 18$; $17 + c = 2$; $17 + d = 19$.

### 10.4 The Ammiditana month-lengths

The month-lengths in Tab. 11 are attested in contracts from the reign of Ammiditana.

In view of the incomplete list of intercalations, only month-lengths from the years 22–37 are usable. The list is taken from *Astronomical Dating of Babylon I and Ur III*:[48] 13 months, plus 4 from Richardson 2013, the latter with superscript R and noted with n in the last column. Later on, too late to be used in the calculations, Michael Roaf supplied a table with month-lengths (mostly collated by Frans van Koppen, but in particular the 26.VI and 26.IX and 36.I need to be confirmed); it was merged into the list in Tab. 11. For the 17 entries used in the calculations, the MNU column gives an arbitrary count of month numbers.

48 Huber, Sachs, et al. 1982, 64–65.

| Year | Month | Day | Texts | MNU | |
|---|---|---|---|---|---|
| 1 | X | 30 | VAT 6655 (VAS NF 2 15) | | |
| | XII | 30 | BM 80336; BM 81465; Bu 1891-05-09, 0473 (CT 6 26b) | | |
| 2 | XI | 30 | Di 720 (K. van Lerberghe) | | |
| | XII | 30 | BM 17482; BM 80623 | | |
| 4 | VIII | 30 | BM 78704 = ? (CT 33 47b) | | |
| | $XII_2$ | 30 | CBS 0723 (BE 6/1 91) | | |
| 5 | XII | 30 | CBS 0110 (BE 6/1 82) | | |
| 6 | IV | 30 | BM 80161 (CT 45 46) | | |
| 7 | XII | 30 | U.7183 (UET 5 518); MLC 00452 (YOS 13 126); CBS 0125 | | |
| 14 | III | 30 | BM 78182 (CT 45 48) | | |
| | IX | 30 | BM 109169 | | |
| | $XII_2$ | 30 | HSM 48 (YOS 13 1) | | |
| 24 | I or V | 30 | BM 81569 | | |
| | IV | 30 | BM 80513 | 828 | n |
| | VII | 30 | AO 01679 (TCL 1 153) | 831 | |
| | VIII | 30 | BM 80513 | | |
| 26 | VI | 30 | VAT 5912 (Kugler, SSB II p.246) | 854 | |
| | IX | 30 | VAT 5806 (Kugler, SSB II p.246) | 857 | |
| 27 | VII | 30 | BM 97441 | | |
| | $XII_2$ | 30 | CBS 0366 (BE 6/2 109) | 874 | |
| 29 | II | 30 | MLC 1291 (YOS 13 254) | 889 | |
| 30 | IV | 30 | TJAUB pl.39(H 31) | 903 | |
| | VII | 30 | BM 97013$^R$ | 906 | n |
| 31 | II | 30 | CBS 1241 (BE 6/1 83) | 913 | |
| | XII | 30 | CBS 1512 (BE 6/1 84) | 923 | |
| 32 | VIII | 30 | BM 96990$^R$ | 931 | n |
| | XII | 30 | BM 78609 | 935 | |
| 33 | IX | 30 | BM 97447$^R$ | 945 | n |
| 34 | VIII | 30 | MLC 0440 (YOS 13 79) | 957 | |
| | IX | 30 | VAT 6392 (VS 7 60) | 958 | |
| 36 | I | 30 | VAT 06258 | | |
| | II | 30 | MLC 0425 (YOS 13 57) | 975 | |
| | XII | 30 | BM 78719 | 985 | |
| 37 | IV | 30 | BM 97057 | | |

**Tab. 11** Month lengths attested in contracts from the reign of Ammiditana.

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## Illustration and table credits

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# A Text Containing Observations of Mars from the Time of Nebuchadnezzar II

## Summary

This paper dates and analyzes a cuneiform text from Uruk containing planetary observations. I show that the observations date to the first fourteen years of the reign of Nebuchadnezzar II (604–591 BC) and concern the planet Mars. The date of this text places it among the earliest texts containing detailed records of astronomical observations from Babylonia.

Keywords: Babylonia; Mars; observations; Nebuchadnezzar II; Uruk.

In diesem Beitrag wird ein Keilschrifttext aus Uruk datiert und analysiert, der Planetenbeobachtungen enthält. Es wird gezeigt, dass diese Beobachtungen in die ersten vierzehn Jahre der Regierungszeit von Nebukadnezar II (604–591 v. Chr.) datieren und den Planeten Mars betreffen. Die Datierung dieses Textes macht ihn zu einem der frühesten Texte mit detaillierten Aufzeichnungen von astronomischen Beobachtungen aus Babylonien.

Keywords: Babylonien; Mars; Beobachtungen; Nebukadnezar II; Uruk.

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## 1 Introduction

The tablet W 23009, published as SpTU V 266 by von Weiher,[1] contains observations of the synodic phenomena of a planet for the first 14 years of the reign of a king whose name is not preserved. The tablet was excavated from the so-called 'house of the *āšipu*' in Uruk (excavation area U 18) along with several other astronomical tablets.[2] The tablet is small and badly damaged and preserves only a few observation reports from years 12 to 14. Despite the paucity of preserved observational data, it is possible to identify Mars as the planet whose observations are recorded and to determine that the observations date to the reign of Nebuchadnezzar II.

The principal interest of this tablet lies not in the details of the observations themselves, which as mentioned are badly preserved, but rather in its date. Only four other texts containing planetary observations of the kind found on SpTU V 266 are known from before the end of Nebuchadnezzar's reign:

- BM 41222:[3] Observations of Saturn, approaches of Mars and Mercury, and phenomena of Mars, covering parts of the period from (at least) year 8 of Ḫumbaḫaldašu (681 BC) to year 12 of Nabopolassar (614 BC). Positions of the planets relative to stars are measured in cubits.

- HSM 1899.2.112:[4] Observations of the synodic phenomena of Mars from the beginning of Šamaš-šumu-ukin's reign (681 BC) to (at least) year 39 of Nebuchadnezzar (566 BC). The early part of the text gives only very brief statements of the dates of first and last visibilities (often accompanied by a statement that the phenomena was not observed); the last part of the text, from the time of Nebuchadnezzar, contains detailed observations of first and last visibilities, stations, and acronychal risings including the position of Mars relative to a star measured in cubits.

- BM 76738 + 76813:[5] Observations of the first and last visibilities of Saturn from (at least) the beginning to year 14 of Kandalanu (648–634 BC). Occasionally, the position of Saturn relative to a star is given with measurements in degrees.

- W 22797:[6] Observations of first and last visibilities and stations (but not acronychal risings) of Saturn from (at least) years 28 to 31 of Nebuchadnezzar II (577–574 BC). The position of Saturn relative to a star is given with measurements in cubits.

1 The abbreviation *SpTU V* refers to the volume von Weiher 1998.
2 von Weiher 1998, 1; Clancier 2009, 47–72; Robson 2008, 227–240; Ossendrijver (unpublished).
3 Published: Hunger, Sachs, and Steele 2001, No. 52.
4 Published: Britton 2004.
5 Published: Walker 1999.
6 Published: SpTU IV 171 by von Weiher (1993); discussion: Hunger 2000.

The first three tablets in this list are all almost certainly from Babylon. The last tablet, hereinafter SpTU IV 171, was excavated from the same 'house of the *āšipu*' in Uruk as SpTU V 266. It is interesting, therefore, that we have two tablets from this house containing collections of planetary observations from the time of Nebuchadnezzar. It is believed that this house was occupied by two families of scholars, one during the late fifth and early fourth centuries BC and the other during the late fourth and early third centuries BC.[7] Thus, both SpTU IV 171 and SpTU V 266 must have either been brought to this house by one of these later scholars or be a copy of an earlier tablet. It remains an open question whether the observations recorded on either of these tablets were made in Uruk or in Babylon.[8]

## 2 The text

SpTU V 266 is a fragment from the upper left corner of a tablet. Almost all of the obverse is lost, but a decent amount of text is preserved on the reverse. It is unclear from von Weiher's copy whether the tablet originally contained more than one column on each side. If it did, then each column of each side probably contained entries for 3 or 4 years, which would imply that more or less the whole of the original height of the tablet is preserved; if it is only a one column tablet, then a little under one half of the height of the tablet is preserved. Context would suggest that little is lost at the end of lines 5–7 on the reverse, whether or not the tablet originally contained one or two columns.

In addition to a copy, von Weiher gives a transliteration but no translation of the tablet. Von Weiher's transliteration is mainly just an attempt to identify the preserved signs without trying to understand the astronomical content of the tablet. Many of his readings are marked with a question mark (and some do not agree with his copy). I have therefore attempted a new reading of the tablet, guided both by the copy and the transliteration, but also making a number of educated guesses to correct what seem to be likely misreadings. These educated guesses go beyond what I would normally allow myself when trying to read damaged portions of a tablet, but seem to be the only way to make any progress in understanding the text at this time as, unfortunately, the tablet itself is currently inaccessible in Iraq and I have not been able to obtain a photograph to allow a proper collation. I discuss all my corrections to von Weiher's transliteration in the critical apparatus.

7 Clancier 2009, 47–72.

8 For discussions of whether there was a tradition of observational astronomy at Uruk, see Ossendrijver (unpublished) and Steele 2016.

### 2.1 Transliteration

*Obverse*

(1) MU-1 $^{\mathrm{Id}}$A[G$^{?}$.NÍG.DU.ŠEŠ]

(2) ⸢GU$_4^{?}$ 4 + x$^{?}$⸣ […]

remainder lost

*Reverse*

(1′) x x […]

(2′) MU-12 BAR 24 x [IGI]

(3′) GAN 24 10 UŠ *ina* IGI [DELE]

(4′) *šá* ⸢IGI ABSIN$^{?}$⸣ *ina* IGI ABSIN U[Š TA x]

(5′) *ana* ŠÚ LAL-*sa* AB$^{?}$ IK$^{?}$ ŠE ⸢14$^{?}$⸣

(6′) 1$^{!}$ *šá* GIŠ.KUN-*šú* *ana* ŠÚ DIB UŠ

(7′) *e-lat* GAL$^{?}$ KI-*tum* GU$^{?}$ *ana* GE$_6$ NA

(8′) MU-13 ZÍZ 11 Š[Ú]

(9′) MU-14 SIG 18 ŠU ⸢x⸣ […]

(10′) *ina* IGI GIŠ.R[ÍN UŠ …]

### 2.2 Translation

*Obverse*

(1) Year 1, Ne[buchadnezzar$^{?}$ …]

(2) Month II$^{?}$, the 4(+x$^{?}$)th$^{?}$ […]

(remainder lost)

*Reverse*

(1′) … […]

---

(2′) Year 12, Month I, the 24th, [first visibility]

(3′) Month IX, the 24th, 10 degrees in front of [the Single Star]

(4′) in Front of the Furrow, in front of the Furrow, it was station[ary. From the xth]

(5′) it moved back to the west. … Month XII, the 14th$^{?}$

(6′) it passed the 1 (Star) of his Rump to the west and was stationary

(7′) above …

---

(8′) Year 13, Month XI, the 11th, la[st visibility.]

---

(9′) Year 14, Month III, the 18th … [first visibility. The nth]

(10′) in front of Lib[ra it was stationary.]

### 2.3 Critical apparatus

*Obverse*

(1) Von Weiher read SIG after the year number, but Mars's last visibility took place in month II of that year and the planet was not visible again until month V. We would in any case expect a king's name after the year number in the first line of the tablet and the traces in the copy are consistent with $^{\mathrm{Id}}$A[G.NÍG.DU.ŠEŠ] for Nebuchadnezzar.

(2) The traces at the beginning of this line in the copy are consistent with the reading $GU_4$ for Month II, but my reading is based upon the expected date of Mars's last visibility and must be treated with caution.

*Reverse*

(2′) The sign at the end of this line appears to begin with three vertical wedges in the copy and so may perhaps be either a distance measurement or a NA interval but in the former case there does not seem to be enough space for a star name before the end of the line and in the latter case we would expect IGI 'first visibility' to appear before the NA interval. Perhaps the traces are simply a damaged IGI.

(3′–4′) There must be a star name written at the end of line 3′ and/or the beginning of line 4′. On the given date, Mars was about 7 degrees to the east of the Normal Star $\gamma$ Virginis 'The Single Star in Front of the Furrow', which is usually written DELE *šá* IGI ABSIN. There is space at the end of line 3′ for DELE and line 4′ begins with a *šá*. The damaged signs which follow the *šá* are most likely, therefore, to be read IGI ABSIN, which is just about consistent with the traces in the copy. Following these traces we have signs which von Weiher read TI-*qé* for a form of the Akkadian verb *leqû* 'to take away'. I cannot make sense of such a verb here and so propose to read these signs as *ina* IGI ABSIN. This still problematical, however, as it would appear to follow directly another statement of *ina* IGI DELE *šá* IGI ABSIN. It is possible that the scribe here has mistakenly given the star name twice (reading the signs as DELE ⟨*šá*⟩IGI ABSIN); alternatively, he may be giving a second, general statement of the position of Mars as 'in front of the (constellation) Furrow'. The broken text at the end of line 4′ can be restored either TA x 'From the xth' or simply *u* 'and' referring to the following statement about the retrograde motion of the planet at the beginning of line 5′.

(5′) Von Weiher read the beginning of this line as *ana* ŠÚ ½ SA DU-*ik*, but he copied an AB sign rather than a DU sign. The signs as given in von Weiher's transliteration do not make sense without emending the SA to a KÙŠ to give *ana* ŠÚ ½ KÙŠ DU-*ik* 'it proceeded ½ cubit to the west.' However, at this time, Mars was moving retrograde (to the west as stated) and DU is normally only used for direct motion (to the east). I suggest instead assuming that the sign read as ½ is a misreading of LAL and we have the phrase *ana* ŠÚ LAL-*sa* 'it moved back to the west,' a common phrase in early observational texts (see, for example, SpTU IV 171 line 16). The following signs remain problematical, however. It would be possible to take AB as 'Month X,' but I do not know how to then read the following IK sign. Mars did have its acronychal rising around the 28th of Month X, but acronychal risings are not normally reported in texts from early in Nebuchadnezzar's reign, and I see no way of reading the IK sign as a day number followed by a sign (e.g. E) or phrase (e.g. *ana* ME E-*a*) referring to acronychal rising.

(6′) The star GIŠ.KUN A 'The Rump of the Lion' (θ Leonis) is one of the Normal Stars used in later astronomical texts. The star group 2 *šá* GIŠ.KUN-*šú* '2 (Stars) of his Rump,' which presumably includes θ Leonis, appears in the standard list of 25 *ziqpu* stars,[9] and one might expect that the reference to the '1 (Star) of his Rump,' is a mistake for '2 (Stars) of his Rump.'

(7′) The reading and interpretation of this line is very problematical. A reference to Mars being above a star makes sense; however, at that time Mars was slightly to the west and about 6 degrees below θ Leonis (a star with a very high positive latitude of about 9.65 degrees). Thus, Mars must be above another star, as well as being below θ Leonis. A plausible candidate would be χ Leonis. However, I am unsure how to make a star name out of the signs in this line. Perhaps KI-*tum* is referring to the area between the legs of the Lion, and we are to assume a missing UR before the sign I read as GU (but note von Weiher reads SAL + UD for the signs I read as GU *ana*).

(8′) The traces of the sign at the end of this line are probably part of ŠÚ for 'last visibility' rather than *ina* as read by von Weiher. The gap between the day number and ŠÚ suggests that Mars's position on this day was not included in the observation report and so the scribe has spaced out the signs to fill up the whole line.

(9′) Following the day number we would expect either IGI 'first visibility' or a reference to the position of Mars. Von Weiher reads the sign KU plus some traces, which could be the first part of *ku-t*[*al*] 'back' (the preserved traces following the KU would allow for such a reading), but we would then expect *ina* before *ku-tal* 'in the back of' (*ina ku-tal* is used in this context in the early observational text BM 41222 Side A II 7′). Furthermore, von Weiher copied a ŠU not a KU sign. Nevertheless, *ku-t*[*al*] makes

9 Steele 2015.

more sense than ŠU here, so I have provisionally accepted this reading. On this date, Mars was in the rear part of the Twins, so 'back' may refer either to a part of one of the Twin's anatomy or is used in the general sense to mean the rear part of the constellation.

(10′) Von Weiher read the star name as GIŠ.K[UN] (θ Leonis) but Mars was at a longitude of approximately 185 degrees at the time of its first station, which places the planet in Libra. From the copy, a reading GIŠ.R[ÍN …] would be possible. The star is very probably α Libra, a Normal Star called RÍN *šá* ULÙ 'The Southern Part of the Scales', probably here written GIŠ.R[ÍN *šá* ULÙ].

## 3 Date

The reverse of the tablet records the following dated observations:

- Year 12 Month I Day 24 [first visibility]
- Year 12 Month IX Day 24 [first station] 10 degrees in front of γ Virginis
- Year 12 Month XII Day 14? second station near θ Leonis
- Year 13 Month XI Day 11 [last visibility]
- Year 14 Month III Day 18 [first visibility]

It is immediately apparent that the observations concern a planet with a synodic period of a little over 2 years. This is sufficient to identify the planet as Mars. The distribution of the dates of the synodic phenomena is also characteristic of Mars. Knowing that the text contains observations of Mars, a search of the tables of the phenomena of Mars computed by N. A. Roughton and kindly made available to the author,[10] quickly shows that only during the reign of Nebuchadnezzar II do the dates of the phenomena recorded in the text agree with modern computation. This date is confirmed by comparing the positions of Mars given for the observations of first and second station, which agree well with modern computation.

Other characteristics of the text also argue for an early date: (1) the use of degrees rather than cubits for the measurement of celestial distances is rare and only found in early observational texts, and (2) the writing GIŠ.RÍN rather than RÍN is much more common in early texts rather than late texts.

10 For details of Roughton's tables, see Roughton 2002, 370.

| Babylonian date | Julian date | Phenomena | Computed date | Difference |
|---|---|---|---|---|
| Year 12 I 24 | 23/5/593 BC | First visibility | 25/5/593 BC | –2 days |
| Year 12 IX 24 | 14/1/592 BC | First Station | 5/1/592 BC | +9 days |
| Year 12 XII 14? | 2/4/592 BC | Second Station | 26/3/592 BC | +7 days |
| Year 13 XI 11 | 17/2/591 BC | Last visibility | 13/2/591 BC | +4 days |
| Year 14 III 18 | 24/6/591 BC | First visibility | 24/6/591 BC | 0 days |

**Tab. 1** A comparison of the observed dates of the synodic phenomena of Mars with those computed in Roughton's tables.

## 4 The observations

Now that the date of the observations in SpTU V 266 has been established it is possible to analyze the observations it contains. Tab. 1 compares the fully preserved dates of the observed phenomena with the results of modern computation. The dates of the observed phenomena were converted to Julian dates using the tables of Parker and Dubberstein.[11] Note that Parker and Dubberstein's date may differ from the true Babylonian calendar by one day; a one-day error, however, is insignificant for this analysis. Computed dates were taken from Roughton's tables. These tables were calculated for an observer in Babylon, but the dates of the synodic phenomena should vary by no more (and usually much less) than one day than these if the observations were made in Uruk. Any resulting one-day error caused either by the visibility criteria or the date conversions is significantly less than the uncertainty in the date of visibility phenomena caused by the day-to-day variation in local observing conditions due to weather etc.

In general, the observed dates of visibility phenomena are in good agreement with the computed dates, with a tendency for the computed dates to be slightly later for first visibilities and slightly earlier for last visibilities, suggesting that Schoch's visibility criteria for Mars are slightly too high. In general, the differences between observed and computed dates are of the same magnitude to those found by Britton in his analysis of early the Mars observations from the time of Nebuchadnezzar on HSM 1899.2.112 and Walker in his analysis of the Saturn observations from the time of Kandalanu on BM 76738 + 76813.[12] The dates of the stationary points are considerably less accurate, both late by several days. The lateness of these observations no doubt reflects the difficulty in determining exactly when Mars changes from direct to retrograde motion; for

11 Parker and Dubberstein 1956.

12 Britton 2004; Walker 1999. See also de Jong 2002, a study of the Saturn observations on BM 76738 + 76813 and SpTU IV 171.

several days around the station, Mars moves very slowly (less than about 0.15 degrees for 5 days before and after the station).

Only one detailed measurement of the position of Mars at a synodic phenomenon is fully preserved: Mars was 10 degrees in front of $\gamma$ Virginis on the 24th of Month IX of year 12. According to the NASA Horizon online ephemeris, Mars's longitude was 146.77 degrees and its latitude +4.04 degrees on this date. The longitude and latitude of $\gamma$ Virginis at this period was 154.40 degrees and +3.01 degrees respectively.[13] Various studies have shown that the term 'in front of' refers approximately to a displacement eastwards in celestial longitude.[14] The computed longitude difference between Mars and $\gamma$ Virginis on the date of the observation is 7.63 degrees, slightly less than the 10 degrees stated in the observation report; it is not unreasonable to suppose that the 10 degrees stated in the text is a rounded figure.

## 5 Conclusion

SpTU V 266 provides further evidence that the practice of regular observation of planetary synodic phenomena was already well established by the early sixth century. The observations contained in this text are recorded in a remarkably similar style to later texts; although there are small differences in terminology, especially in the names of stars, the basic format of a planetary observation report as it existed in the early sixth century BC continued until the Seleucid period. This text, the other early planetary texts, and the existence of compilations of lunar eclipse observations and of lunar six data from this period,[15] also show an interest in the systematic collection of astronomical data concerning one planet or lunar phenomena, which must surely be linked to the development of predictive methods at this period.[16]

13 The coordinates of $\gamma$ Virginis were taken from Sachs and Hunger 1988, 18, for the year –600.

14 See most recently Jones 2004.

15 For the lunar eclipse texts, see Hunger, Sachs, and Steele 2001, Nos. 1, 6, and 7; and for the lunar six texts, see Huber and Steele 2007.

16 On this topic, see, for example, Brack-Bernsen 1999; Britton 2008; Steele 2000; Steele 2011; and, in general terms, Brown 2000, 161–207.

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## Table credits

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Gerd Graßhoff and Erich Wenger

# The Coordinate System of Astronomical Observations in the Babylonian Diaries


## Summary

A large number of the astronomical observations in the Babylonian diaries are occurrences of close conjunctions of moving objects, such as the Moon or planets with bright stars, in the vicinity of the ecliptic. In 1995, Graßhoff proposed the hypothesis that the observations fit best when one assumes that the Babylonians used an ecliptical coordinate system. In the following we present a test that excludes an equatorial coordinate system as an alternative system of measurement.


Keywords: Babylonian astronomy; observations; coordinate system; astronomical diaries; lunar observations; C. Ptolemy.

Ein Großteil der astronomischen Beobachtungen in den Babylonischen Tagebüchern handelt von Konjunktionsereignissen sich bewegender Objekte, wie dem Mond oder Planeten mit hellen Sternen in der Nähe der Ekliptik. 1995 argumentierte Graßhoff, dass die Beobachtungen am meisten Sinn ergäben, wenn man davon ausginge, dass die Babylonier ein ekliptikales Koordinatensystem nutzten. Im Folgenden stellen wir einen Test vor, der ein äquatoriales Koordinatensystem als alternatives Messsystem ausschließt.

Keywords: Babylonische Astronomie; Beobachtungen; Koordinatensystem; astronomische Tagebücher; Mondbeobachtungen; K. Ptolemaios.

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## 1 Introduction

In 1987 Otto Neugebauer suggested that Gerd Graßhoff reanalyze what seemed to be observational reports in the Babylonian astronomical diaries, which were being prepared for publication by Hermann Hunger on the basis of the notes of the late Abraham Sachs. Hunger kindly gave Graßhoff access to his text files so that he could process the astronomical data. Thus, Graßhoff undertook a comparative analysis of the calculated positions of the Moon of the first two volumes of the *Astronomical Diaries* using the just published algorithms of Chapront-Touzé[1]. Until then, no one had carried out a systematic interpretation of the observational reports, which had fueled much debate between Neugebauer and Noel Swerdlow at Princeton, and which had led them to question whether the *Astronomical Diaries* had anything in common with the ACT. During one particular summer of intense discussion on the difficulties of interpreting the reports, they had even argued about whether the Babylonian observers had used any modern astronomical coordinate system at all. In 1990 the early results showed that the observations of planetary configurations had been recorded using the ecliptical coordinate system. Swerdlow promptly took up the challenge and investigated the implications for Babylonian planetary theory.[2] The results concerning the Babylonian coordinate system were presented at a Dibner Institute workshop at the MIT in Boston in 1995. As statistical tests could not distinguish clearly between ecliptical and equatorial coordinates, the late John Britton suggested that future researchers look for properties in the data that would yield an *experimentum crucis* between both coordinate systems. This paper is a response to his suggestion.

The three volumes of late Babylonian texts, edited by Abraham Sachs and Hermann Hunger, and published by 1996,[3] contain the observations of more than 5 000 planetary and lunar configurations. Observations of this type record close approximations of the Moon or planets with bright stars in the vicinity of the ecliptic. According to Graßhoff, the general form of the observed configurations can be tabulated as shown in Tab. 1.

A number of the observational reports mention more than one topographical relationship and their quantity. These expressions have been translated as 'low to the south,' 'high to the north,' 'back to the west,' and 'passed to the east,' followed by another quantitative value. A schematic form of these expressions is:

at $t$: $O_1$ stands ['in front of' / 'behind'] $O_2$ with $D$, low to the south with $N$.

'Low to the south' and 'high to the north' measures ecliptical differences. The first object stands in the north if its difference of latitudes with the second object is positive.

1 Chapront-Touzé and Chapront 1991.

2 Swerdlow 1998.

3 Sachs and Hunger 1988–1996.

| | standard | *back to the west* | *passed to the east* | *balanced* | further specification |
|---|---|---|---|---|---|
| | ecliptical difference of latitude, $\beta_1 > \beta_2$, $D_1 = \beta_1 - \beta_2$ | | | | |
| *above* | small difference of longitude | difference of longitude $\lambda_1 < \lambda_2$, $D_2 = \lambda_2 - \lambda_1$ | difference of longitude $\lambda_1 > \lambda_2$, $D_2 = \lambda_1 - \lambda_2$ | very small difference of longitude | — |
| | ecliptical difference of latitude, $\beta_1 < \beta_2$, $D_1 = \beta_2 - \beta_1$ | | | | |
| *below* | small difference of longitude | difference of longitude $\lambda_1 < \lambda_2$, $D_2 = \lambda_2 - \lambda_1$ | difference of longitude $\lambda_1 > \lambda_2$, $D_2 = \lambda_1 - \lambda_2$ | very small difference of longitude | — |

| | standard | *high to the north* | *low to the south* | *balanced* | further specification |
|---|---|---|---|---|---|
| | ecliptical difference of longitude, $\lambda_1 < \lambda_2$, $D_1 = \lambda_2 - \lambda_1$ | | | | |
| *in front of* | undetermined difference of latitude | difference of latitude $\beta_1 > \beta_2$, $D_2 = \beta_1 - \beta_2$ | difference of latitude $\beta_1 < \beta_2$ $D_2 = \beta_2 - \beta_1$ | very small difference of longitude | occasionally with planets: *to the west* |
| | ecliptical difference of longitude, $\lambda_1 > \lambda_2$, $D_1 = \lambda_1 - \lambda_2$ | | | | |
| *behind* | undetermined difference of latitude | difference of latitude $\beta_1 > \beta_2$, $D_2 = \beta_1 - \beta_2$ | difference of latitude $\beta_1 < \beta_2$, $D_2 = \beta_2 - \beta_1$ | very small difference of latitude | occasionally with planets: *to the east* |

**Tab. 1** Summary of the meaning of the relational expressions (rows) and the additional remarks (columns) used; for configurations between celestial bodies $O_1$ und $O_2$ with ecliptical coordinates $\lambda_1$, $\beta_1$ and $\lambda_2$, $\beta_2$. The measurement is denoted as $D_1$, accompanied by $D_2$ in the case of dual coordinates. Cf. Graßhoff 1999.

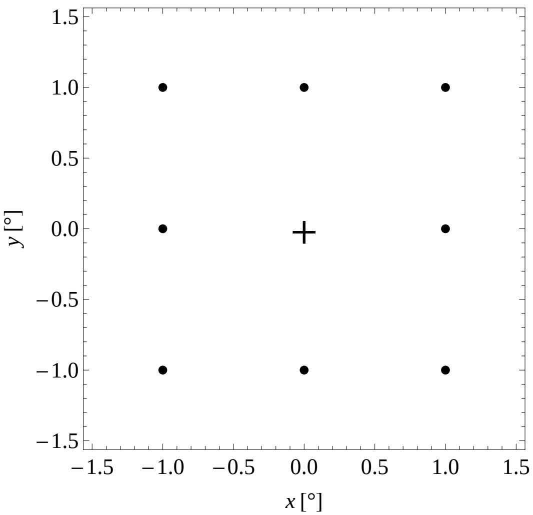


**Fig. 1** Two hypothetical objects displayed in a distance relationship using an abstract coordinate system.

## 2 Babylonian astronomical observations of planetary and lunar configurations

### 2.1 Measuring coordinate differences

Let us begin with looking at an abstract generalization of the observation of a configuration of two objects with dual coordinates in a measuring plane $(x, y)$ as shown in Fig. 1. The position of the second (slower or fixed) object is marked in the center of the observation window by a cross. The positions of eight other objects are marked by bullet points, with their respective coordinate differences. In standard Babylonian formulation they would be mentioned as the first objects. Their position follows a square of a length of one degree around the cross in the center. The standard form of a configuration statement is:

> 'At date D, the second object (e.g. the Moon) is situated at distance A below object 2 (e.g. the star *Beta Tauri*) and at distance B towards the east.'

As in the aforementioned example, we here have the simulation of a coordinate difference (A, B) of two hypothetical objects defined by their distance. The coordinates themselves refer to a coordinate system, and the coordinate differences are indicated on the measuring plane $(x, y)$. It is our goal to identify which coordinate system the Babylonians used.

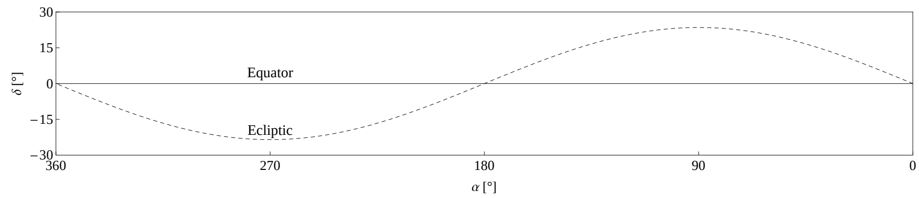


**Fig. 2** The Sun moving along the ecliptic.

## 2.2 Comparison of observations using recalculated positions

The procedure for identifying the coordinate system is based on a method which uses the systematic errors that occurred in the data when the coordinate system assumed to have been used by the Babylonians to take their measurements is, in fact, the wrong one. As the characteristic errors for an assumed measurement procedure usually appear as systematic errors in the obtained data differences, we, therefore, analyzed the observations by comparing them with modern recalculations. The coordinate differences between the observed values and the recalculated values can give us clues about the correctness of the assumed coordinate system, as such differences appear much the same in different coordinate systems, although they are not uniform for different regions of the sky.

If the Babylonian observations had been recorded as angular differences in a coordinate system that differs from the coordinate system we used to recalculate the differences, then characteristic errors, which vary in size and direction across the sky, would occur. It is, therefore, not enough to take the mean deviations of the calculated positions of the stars for all the observations made (irrespective of their position in the sky) or to examine statistically their dispersion. Because of the variations in coordinate differences, one cannot simply use standard fits to identify the underlying coordinate system.

## 2.3 Systematic errors

The annual path of the Sun follows the ecliptic, and is plotted as a line of dots in Fig. 2. The ecliptic crosses the celestial equator at the spring (0°) or autumn equinoxes (180°). All our calculations refer to the equinoctial points and solstices at the time of observation. When we could compare the coordinate differences originally recorded using an equatorial coordinate system with the recalculated positions within an ecliptical reference system, we introduced a specific systematic error.

First, we show the effect with simulated data. Let us superimpose the zero points of the equatorial and ecliptical celestial coordinate systems in Fig. 3: the left column shows the position of stars plotted using the equatorial coordinate system. It should be noted that the values increase from right to left.

Now, if the observations had been made in one of the coordinate systems, and the positions of the two objects had been calculated in the other coordinate system, then typical systematic errors should have occurred:

- The coordinate systems would have rotated against each other at the spring and autumn equinoxes. The difference vectors would have rotated as well.
- The coordinate systems would have lain parallel at the summer and winter solstices. And there would be no differences in the measurements of angular distances at the solstices.

At the equinoxes the ecliptic is inclined maximally towards the equator; the corresponding directions of the angular distances between the two celestial objects incline by the same degree.

The aim of our procedure is to determine whether such a systematic turn can be detected in the reconstructed data or not. If the angular distances recorded in the observational reports are compared with the recalculated differences using the wrongly assumed coordinate system, a characteristic rotation would show up in the data. Therefore, the method should clearly falsify the incorrect assumptions made about the assumed measurement procedures. If the right coordinate system was chosen for the recalculations, then the systematic rotational error should not appear. In order to simulate the effects of the presumed coordinate systems, we will now take a look at the positions of pairs of celestial bodies in their respective regions of the sky.

### *2.3.1 True equatorial system compared with equatorial data*

First, we test the assumption that the observations were based on the equatorial system by transferring the abstract observations from Fig. 3 to the equatorial coordinate system. This is done four times at the aforementioned key positions. We then distribute eight objects in the square around object two in the middle column, each with an angular difference of 1° in one of the coordinates (left of Fig. 3). The corner coordinates of the respective points are situated at a distance of 1° up or down and 1° to the right or left from the reference object in the middle of the square. When we calculated the positions of all the celestial objects using the equatorial coordinate system, we got a figure identical to the one at the left-side diagram of Fig. 3. In general, rotations would only occur if we used the wrong coordinate system to recalculate the positions.

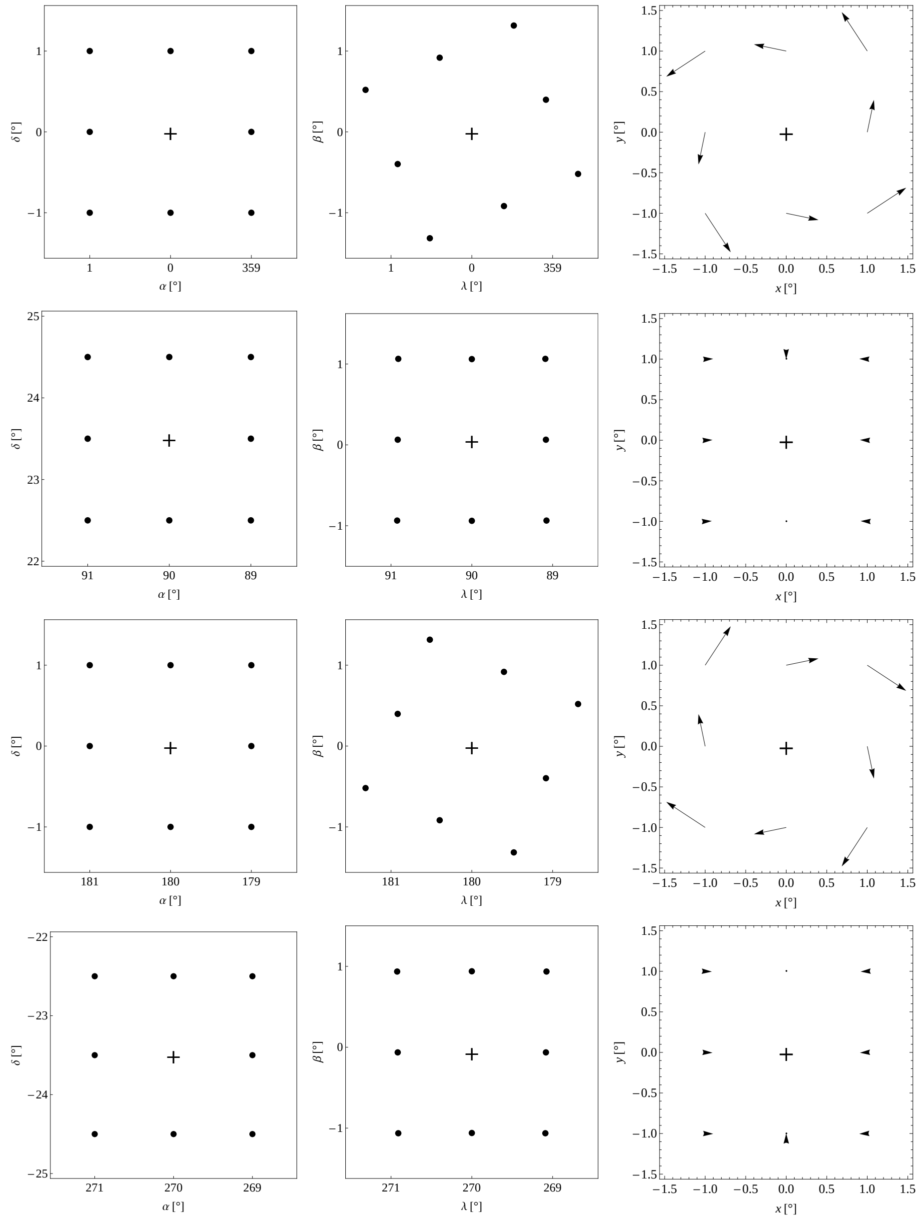


**Fig. 3** Left column: coordinate differences on all four cardinal points of an equatorial system. Middle column: coordinate differences on all four cardinal points of an ecliptical system. Right column: differences of the distances. Rows from top to bottom: spring equinox (SE), summer solstice (SS), autumn equinox (AE), winter solstice (WS).

### 2.3.2 *True equatorial system compared with ecliptical data*

If we now calculate the same object using the corresponding ecliptical coordinate system, Fig. 3 shows that no discernible deviations can be noticed in the solstices. However, there is a noticeable rotation at the equinoxes, which falsifies the hypothesis that the original observations were made using an ecliptical coordinate system.

### 2.3.3 *True ecliptical system compared with ecliptical data*

We will now assume that the Babylonians made their measurements using the ecliptical system, and we will then calculate the deviations between both the ecliptical and the equatorial recalculations. If the coordinate system used for making the observations and the recalculations are the same, then no noticeable rotation should appear at the equinoxes, and no rotation at the solstices.

If we look for the vernal point at the top row diagram, the shaft ends of the arrows stand where the celestial bodies are located in accordance with the equatorial coordinate system of Fig. 3, whereas the arrowheads indicate the shifted position of the object's coordinates according to the recalculated ecliptical coordinate system.

As the Babylonian observations recorded the angular distances between two objects, the systematic deviations of the angular distances systematically superimposed the measured values: there are hardly any differences at the solstices, but there are rotations at the vernal and autumnal points.

It is important to note that we depict the differences between the calculated and observed positions using the measuring system $(x, y)$ and not using a celestial coordinate system. By way of example: the greater the distance between object 1 and object 2 in the top row illustration, the greater the deviation will be. At the solstices, the axes of the two coordinate systems are parallel to each other and so there should be no deviations. The systematic rotation thus only appears relatively in the measuring system, not absolutely in the sky. Hence, it is only visible when comparing Babylonian observations with distances, recalculated using different test coordinate systems.

### 2.3.4 *The ecliptical coordinate system*

The systematic errors will be reversed when the assumed coordinate system is ecliptical, and the equatorial system is used for calculating the positions.

Again, the actual differences of the eight objects around the reference object in the middle are only shifted by $1^\circ$ in a coordinate and thus, in sum, all these deviations result in a square distribution around the second object.

If we superimpose the equatorial coordinate system over the actual ecliptical coordinate system, the typical rotations of the differences occur at the equinoxes – only in reverse. The values of the angular distances rotate clockwise at the vernal point and counterclockwise at the autumnal point. This is a general definition of the test method. Thus, the test method is defined in general terms.

### 2.4 Test procedure

Because of these complementary systematic errors, we can set up a test procedure for determining which coordinate system was used.

1. Based on the observed angular distances, we calculate the positions of object 1, in both the equatorial and in the ecliptical systems. We compare these positions using state-of-the-art coordinate calculations of the objects. We then plot the deviations in the measuring system $(x, y)$.

2. If the Babylonians did their measurements using the equatorial system, but we evaluated their findings using the ecliptical system, we should be able to observe a faulty counterclockwise rotation at the vernal point and a clockwise rotation at the autumnal point. No rotations should appear at the solstices.

3. If the Babylonians made their measurements using the ecliptical system, but we evaluated their findings using the equatorial system, then we should observe a faulty clockwise rotation at the spring point and a counterclockwise rotation at the autumn point. No rotations should appear at the solstices.

4. If we made our comparisions using the same coordinate system as the Babylonians, there should be no rotations at all.

We thus arrive at a sensible testing procedure (Tab. 2).

| | **ecliptically calculated** | **equatorially calculated** |
|---|---|---|
| ecliptical Babylonian | no rotations | SE: clockwise\|AE: countercl. |
| equatorial Babylonian | SE: countercl.\|AE: clockwise | no rotations |

**Tab. 2** Decision criteria for best-fitting coordinate system.

We thus obtain two different tests that enable us to ascertain which coordinate system the Babylonians used. Based on the observed angular distances of the two recorded celestial objects, we calculate their positions twice: once using the ecliptical system, once using the equatorial system. The distribution of the findings depicted in Tab. 2 reveals why one should apply the systems that the Babylonians used.

### 2.5 With stochastic errors

Before we analyze the real data, let us look at the effect of random errors in the observations, which occurred when the Babylonian took their measurements.

#### *2.5.1 Equatorial coordinate system for observation*

The observations of the Babylonian astronomers show small, random errors, as is the case for all empirically measured values. These so-called stochastic errors randomly influence the measured positions of the two objects. The question then arises as to whether these statistical errors overlap the systematic errors in such a way that we can no longer discern the rotations.

In Fig. 4, the eight positioned objects show a random error of deviating from 1° in $x$ and $y$. The first example is based on the equatorial coordinate system and the random errors concerning the equator are superimposed on both coordinate systems.

If we now calculate the positions of the objects using the ecliptical coordinate system, even if the observations were made equatorially, then the systematic error of the aforementioned discussion overlaps with the stochastic error of the individual observation. As a result, we can discern the meanwhile well-known rotation of the deviations of the angular distances of both objects.

#### *2.5.2 Ecliptical coordinate system for observation*

In the case of the observation of the positions in the ecliptical coordinate system and the subsequent calculation of the position in the equatorial coordinate system, we can observe a similar superimposition with stochastic errors. The only difference is that in this case the systematic errors rotate in the reverse direction in the solstices. In the solstices only random errors are visible.

If, in this case, no rotation appears, but we can could observe stochastic errors in such a dimension that a rotation of the coordinates is visible, then the calculated ecliptical coordinates in fact match the observed data.

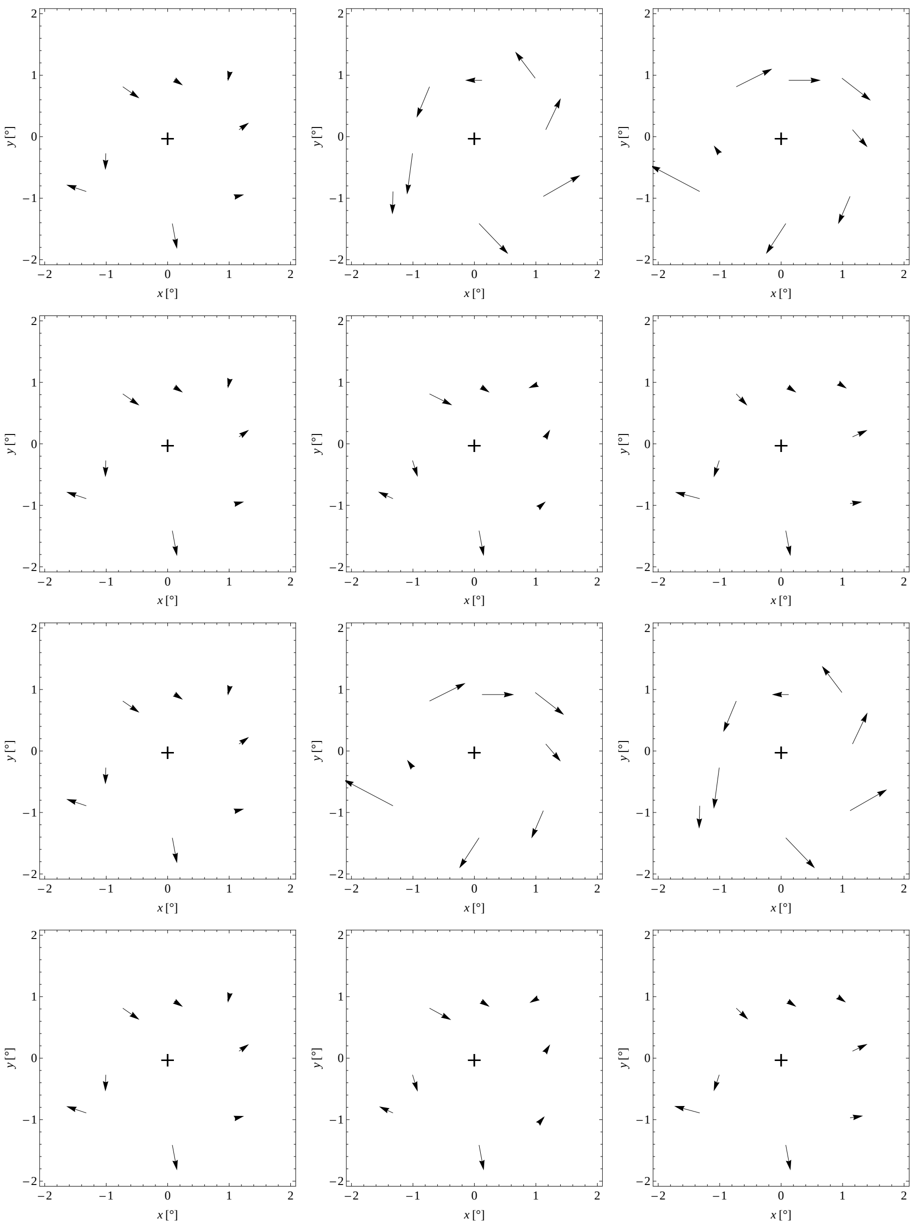


**Fig. 4** Superimposed stochastic errors plus systematic errors. Left column: no systematic errors. Middle column: systematic from recalculated ecliptical instead of equatorial system. Right column: equatorial instead of ecliptical coordinate systems. Rows from top to bottom: spring equinox (SE), summer solstice (SS), autumn equinox (AE), winter solstice (WS).

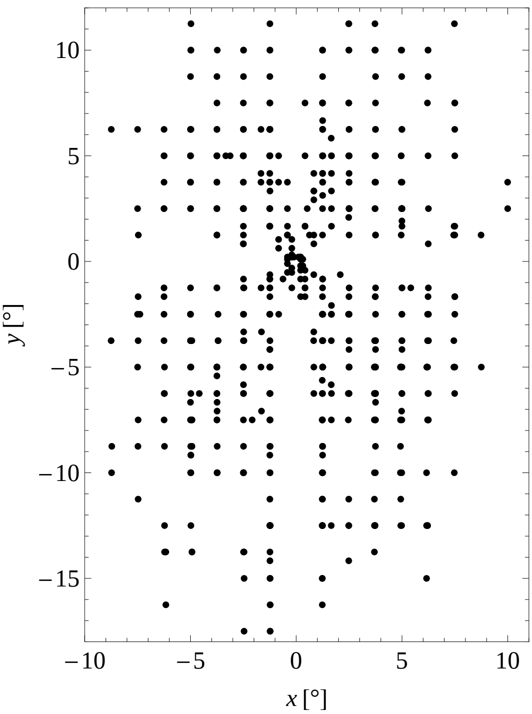


Fig. 5 Double coordinate observations of coordinate differences.

## 2.6 Analytical findings

In the following, we investigate the measurements of configurations with double coordinates. Outliers with a deviation of more than 5° have been excluded. The number of observations comprises 595 measurements. If we transfer the observed angular distances to the second object, which is situated at the origin of the measuring system, we get the distribution shown in Fig. 5. It can be seen clearly that the Babylonians measured the northern and southern distances in latitude for greater distances than in the case of the longitudes.

This is in fact a consequence of the moment when the measurement was made: The moment is recorded when the first object passed the second object above the horizon, with as little distance as possible – a movement which can be observed. As the solar system objects, including the Moon, move along the ecliptic, the smallest distance to the second object is determined by two aspects for both coordinates: the minimal latitude is given by the ecliptical difference in latitude of both objects and varies depending on the ecliptical latitude of the first object. The minimal length is determined by the

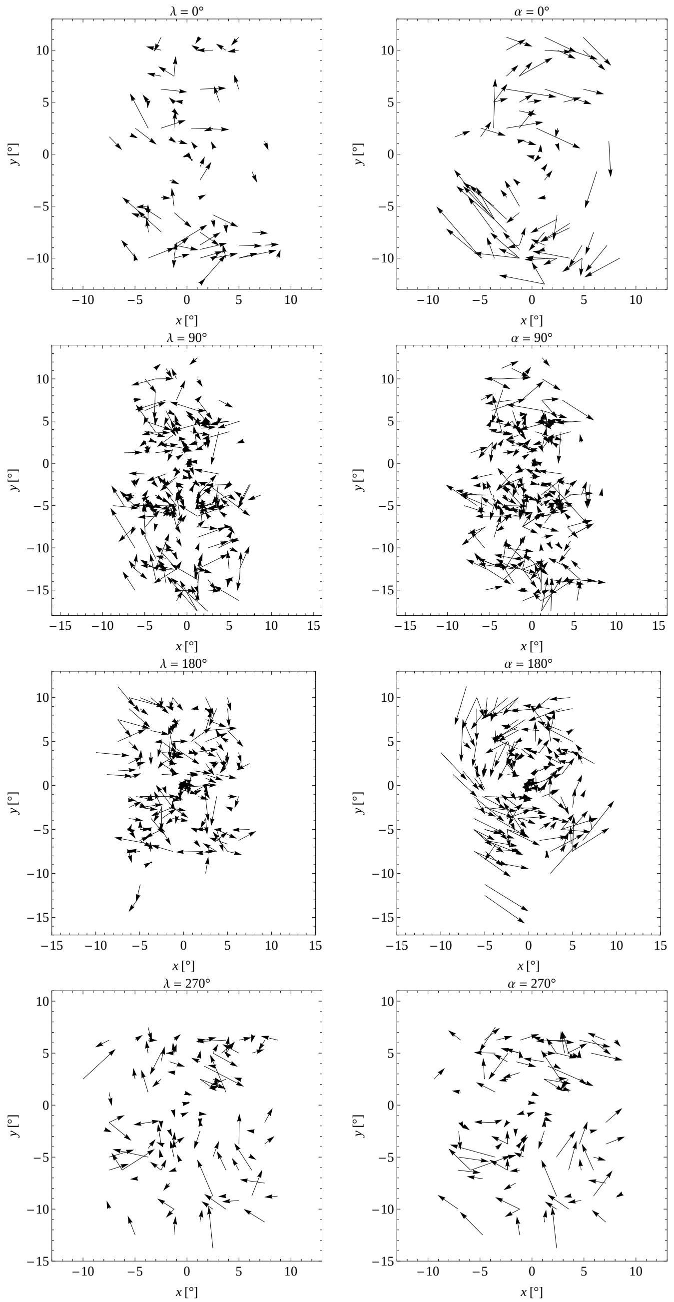


**Fig. 6** Differences of distance vectors using ecliptical coordinates (left) and equatorial coordinates (right) for all cardinal points. Rows from top to bottom: spring equinox (SE), summer solstice (SS), autumn equinox (AE), winter solstice (WS).

observation window, which was just big enough to make the measurement of the passage possible. It is due to these aspects that both measuring coordinates show a different dispersion in the measurement area. However, this does not mean that both coordinates comprise differently sized measurement errors. The measuring accuracy could still be similar for both coordinates.

We have now calculated the positions of the celestial bodies in ecliptical and equatorial coordinates in order to analyze the characteristics of the data. We have calculated the coordinate differences for our measurement system $(x, y)$, which can be deducted from these data.

Using four rectangles measuring $90° \times 180°$, we have chosen observations for which the second object is situated within this field. Fig. 6 comprises the findings: the left-hand column shows the calculated ecliptical coordinate differences starting from the vernal point (in the uppermost row) to the winter solstice in the fourth row. The right column shows the coordinate differences for the equatorial calculation for the same regions in the sky. Let us first look at the right column. In the uppermost row, we calculate the coordinates for the vernal point, using equatorial coordinates. We receive a characteristic rotation in a clockwise sense. If we calculate the errors in the ecliptical coordinate system in the first column, no rotations appear. The findings for the autumnal point show the same results. For the calculation in the equatorial coordinate system, the errors appear as counterclockwise rotations. The errors rotate counterclockwise as a result of a systematic error due to the fact that the equatorial coordinate system has erroneously been applied. As can be seen, the calculation of the coordinate difference for the ecliptical calculation shows no rotations. These findings unambiguously establish that the Babylonians used the ecliptical coordinate system to record their data. The reported quantities of the configurations of two celestial objects measure coordinate differences.

## 3 Further corroborating findings

### 3.1 Magnitude of error vectors

The orientation of the systematic error vectors is the crucial argument for deciding which coordinate system was used by the Babylonians. Nevertheless, the magnitude of the total error vectors (i.e. systematical plus stochastic errors) should support this argument. In Fig. 7 we compiled the errors, once assuming the ecliptical and once assuming the equatorial system.

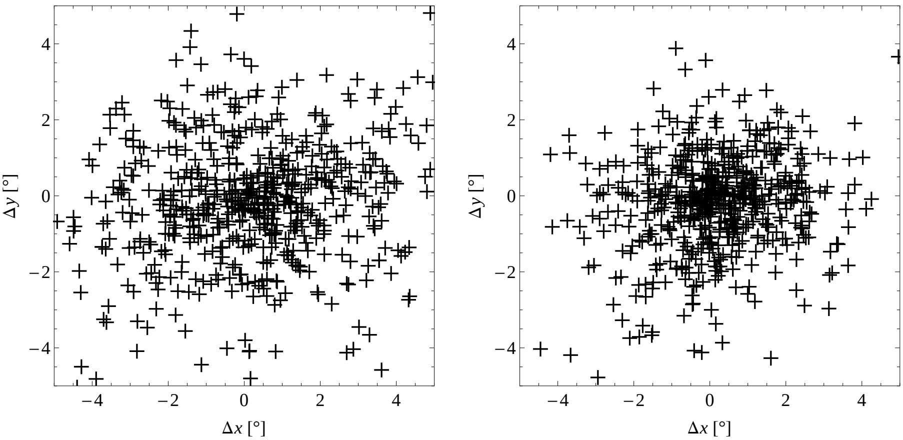


**Fig. 7** Distribution of positional errors: if (a) the recalculated positions are given in equatorial coordinates; or (b) in ecliptical coordinates. Note that the distribution is denser when ecliptical coordinates are used, which is equivalent to a better fit of the recalculated and the documented data.

The corresponding RMS errors are found in Tab. 3 and were calculated by ($n = 595$):

$$\text{RMS error} = \sqrt{\frac{1}{n}\sum_{i=1}^{n}\left(x_{i,\text{observed}} - x_{i,\text{computed}}\right)^2}$$

| | equatorial | ecliptical | error decrease |
|---|---|---|---|
| $x$-direction | 2.05° | 1.51° | –26.4% |
| $y$-direction | 1.57° | 1.27° | –19.1% |

**Tab. 3** RMS errors in $x$- and $y$-direction, assuming the ecliptical and equatorial coordinate system, respectively.

## 3.2 Special cases

In Fig. 8 we display a Babylonian observation that reads: 'in the last part of the night the moon is 5° above mars and 0.5° passed to the east' (observation no. 5975). Calculated in the ecliptical system, the Moon (dots) is always East from Mars (center cross) from the first sight possible of the Moon (Moonrise) until the last sight possible (Sunrise). Calculated in the equatorial system, however, the Moon is never East but always West of Mars

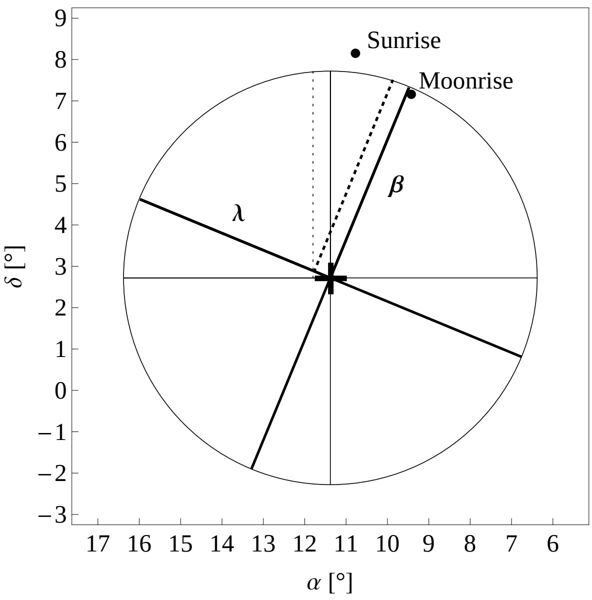


**Fig. 8** Observation No. 5975. Cases of different relational configurations depending on the compared coordinate system.

throughout the possible time of visibility. Thus, if the Babylonian used the equatorial system, they must have confused the directions East and West.

Now, there are 30 observations, which were coincidentally made in one of the four quadrants built by the equatorial and the ecliptical coordinate axes (see Fig. 8). 25 of these observations can be explained in both systems, due to the uncertainty of the observation time, whereas five observations (like the one mentioned) cannot. For each and every of these five observations, the ecliptical system fits, whereas the equatorial doesn't. So, if we assume the equatorial system as correct, we would have to accept that the Babylonians confused the directions East-West and North-South exclusively for these five observations. If we assume the ecliptical system as correct, all indications of directions (including the 25 others) are correct.

### 3.3 Other ancient witnesses

Babylonian astronomy strongly influenced Ptolemaic astronomy, particularly through the work of Hipparchus, in whose *Commentary on the Phenomena of Aratus and Eudoxus* we find extensive usage of Babylonian terminology.[4] Ptolemy referred to two observations

4 Cf. Neugebauer 1975, 279–281, 304, 544, and 591–593.

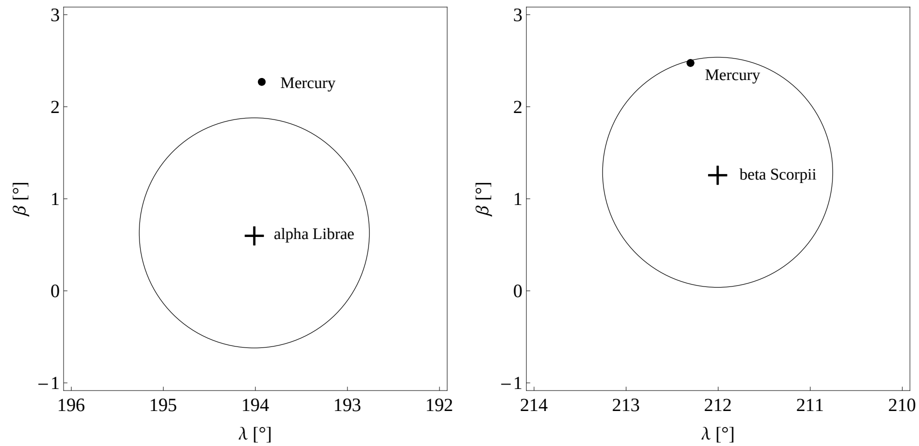


**Fig. 9** Mercury's approximate position relative to the stars, according to Ptolemy's description.

of Mercury that seem to be of Babylonian origin,[5] and used them to derive his planetary model. He quoted the two observations without fully converting them to the Greek metrological system:

> In the 75th year in the Chaldean calendar, Dios 14, at dawn, [Mercury] was half a cubit [ca. 1°] above [the star on] the southern scale [of Libra]. Thus at that time it was in ♎ $14\frac{1}{6}^{\circ}$, according to our coordinates. […]
>
> In the 67th year in the Chaldean calendar, Apellaios 5, at dawn, [Mercury] was a half a cubit [ca. 1°] above the northern [star in the] forehead of Scorpius (β). Thus at that time it was in ♏ $2\frac{1}{3}^{\circ}$, according to our coordinates.[6]

See Fig. 9 for the computation of Mercury's position relative to the stars for the aforementioned dates.

Ptolemy paraphrased both observations in the form of topographical relationships 'object 1 above object 2 by X cubits (Babylonian: kùš)', which is clear proof of their Babylonian origin. Even more interesting are the details of his evaluation. The quantities are measurements of the differences in latitude between Mercury and the particular star. Ptolemy, however, used this observation to determine planetary longitude. How did he arrive there?

5 Cf. Neugebauer 1975, 159.

6 Ptolemy, *Almagest* Book IX, ch. 7, cited from Toomer 1984, 452. The omission […] and the insertion in round brackets ('β') are made by the authors, all other bracketed insertions were added by Toomer.

If he reduced the stellar longitudes for the epoch of the observations, according to his theory he had just to subtract the value for the precession: $3^\circ\ 50'$ for 373 years in the case of the first observation and $4^\circ$ for 381 years for the second observation.[7] In the star catalog of the *Almagest*, β Scorpii has a longitude of ♏ $6^\circ\ 20'$ and $\alpha$ Librae a longitude of ♎ 18°. In the second case, the resulting longitude would be a little too large. It is plausible that Ptolemy did not reduce the longitudes from the star catalog, but used Hipparchus' value or, alternatively, that of the Babylonian astronomers, and then added the precession constant to these values.

Independent of the exact derivation of the longitude, it is remarkable that Ptolemy assumed that Mercury and the stars have *the same longitude*. He seemed to find the measured coordinate of half a kùš unimportant to the calculation, which demonstrates that he interpreted the Babylonian report in two ways:

1. The measured topographical relationship is a coordinate value, e.g. either longitude or latitude.

2. Since he identified the other coordinate with the longitude, the topographical relationships need to be understood in the framework of the ecliptical coordinate system.

Ptolemy took these excerpts from Hipparchus, who had extensive access to Babylonian ideas.[8] Without a doubt Ptolemy fully understood the meaning of the Babylonian observation reports.

## 4 Conclusion

At the time of the first publication of the Babylonian diaries by Hermann Hunger, it was completely unclear whether the observations of the moon passages along the stars or planets were at all measured, and if they were, which astronomical reference system had been applied. In 1995, the research results on Babylonian astronomical diaries were presented at the Dibner Institute in Boston. According to these results, the ecliptical system has been 'diagnosed' to be the system that matches the documented data best.[9]

7 Note that Ptolemy uses a precession constant of one degree per one hundred years, which is much too small.

8 I have purposefully avoided referring to 'sources', although it is highly probable that Hipparchus had comprehensive access to either original or transcribed Babylonian sources, considering the wealth of Babylonian concepts that he utilized. Cf. Toomer 1984.

9 Alexander Jones extended the testing of the hypothesis to the case of configuration observations of planets in Jones 2004.

A more elaborate argument has been developed in the article presented here. This argument tries to examine to what extent the assumption of a specific coordinate system would generate characteristic errors in the statistical data, and whether these errors would rule out the application of such a coordinate system. This elimination procedure is very specific and statistically significant, and surpasses the levels of significance of usually applied evaluation criteria. The comparison of the two main hypotheses for the reconstruction of the Babylonian coordinate system presented here show clear differences with regard to their exclusion criteria. The equatorial coordinate system creates specific rotation effects in the reconstructed quantitative data, which change their rotation direction according to the celestial quadrant. The rotational quadrants can be identified in the database. Thus, the configuration data of the Babylonian diaries was not recorded in an equatorial system, as the alternative ecliptical system does not show these rotation effects.

This evidence, together with the earlier research results, strongly supports the hypothesis that the Babylonian astronomers either directly observed or calculated passages in the ecliptical coordinate system and systematically noted down their observations on a day-to-day basis.

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Teije de Jong

# On the Origin of the Lunar and Solar Periods in Babylonian Lunar Theory

## Summary

In this investigation, I sketch the way in which Babylonian astronomers may have derived the basic parameters of their lunar theory. I propose that the lunar velocity period of 6247 synodic months which underlies the construction of functions Φ and F of system A is derived by fitting a multiple of the Saros period of 223 synodic months within an integer number of solar years using the 27-year Sirius period relation. I further suggest that the lunar velocity period of 251 synodic months used to construct function F of system B is a direct derivative of the 6247-month period. I also briefly discuss the origin of the periods of the solar velocity function B (of system A) and of the solar longitude function A (of system B) suggesting that the periods of these functions may have been derived from a refined version of the 27-year Sirius period. I finally discuss the timeframe of the possible stepwise development of these early lunar and solar functions.

Keywords: History of science; history of astronomy; Babylonian astronomy; Babylonian lunar theory; Babylonian lunar and solar periods.

In dieser Untersuchung skizziere ich, auf welche Weise babylonische Astronomen die grundlegenden Parameter ihrer Mondtheorie möglicherweise abgeleitet haben. Die Mondgeschwindigkeitsperiode von 6247 synodischen Monaten, die der Konstruktion der Funktionen Φ und F des Systems A unterliegt, sind dadurch abzuleiten, dass man ein Vielfaches der Sarosperiode von 223 synodischen Monaten unter Verwendung der 27-jährigen Siriusperiode in eine ganzzahlige Anzahl von Sonnenjahren einpasst. Des Weiteren schlage ich vor, dass die Mondgeschwindigkeitsperiode von 251 synodischen Monaten, die für die Konstruktion von Funktion F des Systems B genutzt wird, ein direktes Ergebnis der Periode von 6247 Monaten ist. In aller Kürze diskutiere ich auch die Ursprünge der Perioden der Sonnengeschwindigkeitsfunktion B (des Systems A) und der Sonnenlängenfunktion A (des Systems B) und schlage vor, dass die Perioden dieser Funktionen eventuell aus einer präzisierten Version der 27-jährigen Siriusperiode hervorgehen. Abschließend wird der

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Zeitrahmen der möglicherweise schrittweisen Entwicklung der frühen Mond- und Sonnenfunktionen diskutiert.

Keywords: Wissenschaftsgeschichte; Geschichte der Astronomie; Babylonische Astronomie; Babylonische Mondtheorie; Babylonische Mond- und Sonnenperioden.

I have profited from discussions with and from critical remarks of many colleagues of whom I wish to mention here Lis Brack-Bernsen, the late John Britton, Alex Jones, Mathieu Ossendrijver and John Steele.

## 1 Introduction

One of the most basic questions in the field of Babylonian astronomy, "How did the scholars get from the observations as recorded in the *Astronomical Diaries*[1] to the theoretical computations as we know them from the ACT-type[2] texts", is still incompletely answered. What we do know is that the development of Babylonian lunar and planetary theory is based on periodicities in the orbital motion of the Sun, Moon, and planets. From the observed periods longer theoretical 'great' periods (of order several centuries up to about one millennium) were constructed by linear combination, and using these 'great' periods the observed variations in orbital velocity were cast in strictly periodic step functions and/or zigzag functions. These functions are based on linear difference schemes and involve extensive computation. The specific choice of the parameters characterizing these functions appears often to have been based on arithmetic convenience with the purpose of simplifying the calculations.[3]

In this paper I will limit myself to Babylonian lunar theory and I will concentrate on an investigation into the observational basis of the derivation of the basic periods used in the computation of the angular velocity of the Moon (column F in the ephemerides of systems A and B) and of the Saros function Φ (system A). It was through the work of Lis Brack-Bernsen[4] and several of her lectures that I became initially interested in – and after a while fascinated by – function Φ and its secrets and intricacies. This paper

1 Sachs and Hunger 1988–2001; henceforth referred to as the Diaries.

2 *Astronomical Cuneiform Texts* (Neugebauer 1955); henceforth referred to as ACT.

3 This is most obvious in the choice of the values for the angular velocities of the Sun and the planets in the ephemerides of system A. There we find that the angular velocities have different values in different sections of the Zodiac which are related by simple ratios. For instance in ephemerides where the 360° zodiac is divided into two sections we find for Saturn angular velocities in the proportion 21 : 25, for the Sun 15 : 16, and for Jupiter 5 : 6 (see Aaboe 2001, Tab. 3).

4 Brack-Bernsen 1997.

may be considered as a progress report of an investigation into the early development of Babylonian lunar theory which grew out of this fascination.

The time frame for the early development of Babylonian lunar theory is constrained by the lunar text BM 36737 + 47912 and its duplicate BM 36599 discussed by Aaboe and Sachs.[5] This text contains full-fledged versions of functions $F_1$ and $\Phi_1$ of lunar system A (the index 1 refers to functions evaluated at New Moon, while index 2 refers to Full Moon) for the years 474–457 BC and may have been written shortly afterwards. Functions Φ and F of system A are zigzag functions based on the same long period of 6247 synodic months. Lunar system A contains one more function with this same period: function G which gives a first approximation to the excess in days of the synodic lunar month over 29 days.

The other text discussed by Aaboe and Sachs is BM 36822 (+ 27022).[6] It also contains fully developed versions of functions $F_1$ and $\Phi_1$ computed for 398 BC and in addition a crude system A like function for the solar longitude, as well as primitive versions of functions G, C (length of daylight) and M (time between syzygy[7] and sunset/sunrise).

So it seems that functions F (lunar velocity) and Φ (excess time of one Saros of 223 months over 6585 days) were fully developed by the middle of the fifth century BC and that the solar longitude function B of system A was still under development around 400 BC. The earliest lunar ephemeris known so far (of system A) dates from 319 BC,[8] while the last known Babylonian lunar ephemeris (also of system A) dates from 49 BC (ACT 18).

Even a cursory treatment of Babylonian lunar theory is outside the scope of this paper but a short summary of its main features seems appropriate. For a detailed treatment the reader may be referred to Neugebauer's *History of Ancient Mathematical Astronomy*.[9] Lunar ephemerides come in two varieties, called system A and system B. In system A the lunar (solar) longitude at syzygy (function[10] B) is represented by a step function; all other functions are represented by (modified) zigzag functions. In system B all functions are represented by (modified) zigzag functions; the periods adopted for the construction of these functions in system B differ from those adopted in system A.

Representative examples of lunar ephemerides are ACT 5 (New Moons for S.E. 146–148 according to system A) and ACT 122 (New Moons for S.E. 208–210 according to

5 Aaboe and Sachs 1969.

6 Aaboe and Sachs 1969.

7 The term syzygy refers to the conjunction or opposition (Full Moon) of the Sun and Moon.

8 Aaboe 1969.

9 Neugebauer 1975, 474–540; henceforth referred to as HAMA.

10 In this paper I will often not discriminate between functions and columns. For example function B stands for the mathematical function reproducing the arithmetical sequence of numbers displayed in column B of the ephemeris.

system B). The ultimate goal of the construction of lunar ephemerides was to predict a number of lunar phenomena:

- length of the lunar month (columns G–K)
- lunar eclipse magnitudes (columns E and Ψ)
- date and time of syzygy (columns L–N)
- duration of first and last visibility of the Moon (columns O–P)

Prerequisites for the computation of these quantities are the function Φ which serves as an auxiliary function for the computation of function G in system A, the longitude of the Sun/Moon at syzygy (function B in both systems), the orbital velocity of the Moon expressed as its daily displacement (function F in both systems) and the length of daylight (function C in both systems). The fact that these basic functions occupy the first few columns in both systems may be related to the stepwise character of the computation of the ephemeris but it may also reflect the gradual development in time of the theoretical framework on which the computation is based.

In a recent series of papers Britton has argued that the construction of system A lunar theory was a singular creative act by an unknown author rather than a gradual development where a limited number of different scholars during several succeeding generations contributed to its final form as we know it from the surviving ephemerides of the Seleucid and Arsacid era.[11] He dates the invention of system A lunar theory to within a few years of 400 BC and the derivative system B theory about one century later.

In his study Britton emphasizes that the invention of the Babylonian zodiac of 360° must predate – or have been invented simultaneously with – the construction of system A lunar theory.[12] He suggests that the invention of the Babylonian zodiac must have taken place between 409 and 398 BC, consistent with his dating of system A lunar theory. His dating is somewhat late compared to the more generally accepted view that the Babylonian zodiac was introduced into Babylonian astronomy sometime during the second half of the fifth century BC.[13]

The parameters on the basis of which the ephemerides are constructed must have been derived from lunar observations, extensively and routinely carried out during centuries, probably those recorded in the Diaries from about 750 BC onward. As we have seen above about 10 to 20 different functions are needed to build a full-fledged lunar ephemeris. I will concentrate here on the periods of functions Φ and F of system A. Based on the textual evidence mentioned above it seems that these functions were the first ones developed by the Babylonian scholars and that both were fully developed by

11 Britton 2007b; Britton 2009; Britton 2010.

12 Britton 2010.

13 See e.g. Steele 2007, 301.

| | System | Column | Function | Π | Z | Years | Period |
|---|---|---|---|---|---|---|---|
| Moon | A | Φ, F, G | zigzag | 6247 | 448 | 505 | 13.9442 |
| Sun | A | B | step | 2783 | 225 | 225 | 12.3689 |
| Moon | B | F, G | zigzag | 251 | 18 | 20.3 | 13.9444 |
| Sun | B | A | zigzag | 10019 | 810 | 810 | 12.3691 |

**Tab. 1** Parameters of basic functions in lunar ephemerides.

the middle of the fifth century BC. In addition I will also briefly discuss the period of function F of system B as a derivative of function F of system A, and the periods of functions B (system A) and A (system B) which (are needed to) determine the position of the Moon and/or Sun at the moment of conjunction or opposition (syzygy). As I will argue later the development of these early basic functions was a gradual process taking place within the community of Babylonian astronomers during the late sixth and fifth century BC. The 'great' periods Π and wave numbers Z of these basic functions are summarized in Tab. 1.

Several important properties and features of these functions and the parameters defining them may be noted:

- The values adopted for the 'great' periods Π (in synodic months) and the wave numbers Z are generally so large that they must have been constructed from shorter (presumably observed) periods. This is also suggested by the fact that 6247 and 251 are prime numbers. To a lesser extent this also holds for the periods 2783 ($= 11^2 \times 23$) and 10 019 ($= 43 \times 233$) which can be factorized but only into products involving fairly awkward prime numbers. The construction of 'great' periods by linear combination of shorter observed periods in Babylonian astronomy is also known from planetary theory. If lunar theory was developed first this technique may have been pioneered in the construction of these early lunar functions.

- The specific choice of the parameter values may also have been influenced by arithmetical convenience. This is suggested by the factorization of the wave numbers Z into nice low prime integers: $448 = 2^6 \times 7$, $225 = 3^2 \times 5^2$, $810 = 2 \times 3^4 \times 5$, and $18 = 2 \times 3^2$. Notice that several of these wave numbers show nice behavior in sexagesimal arithmetic ($60 = 2^2 \times 3 \times 5$).

– Another important aspect of the construction process appears to be that the 'great' period Π generally spans an integer number of solar years. This is also known from planetary theory and is generally thought to be introduced to eliminate the effect of variable solar velocity (solar anomaly). The numbers in Tab. 1 suggest that the 27-year Sirius period (334 synodic months = 27 solar years) was used in the construction of the period of functions F and Φ because 6247 synodic months span almost exactly 505 years (minus 1 day) while according to the 19-year cycle (235 synodic months = 19 solar years) 6247 synodic months are equivalent to 505 years plus 28 days. On the other hand, the 19-year cycle does result in a better – albeit far from perfect – approximation to the periods of the solar functions B and A (errors of 3 and 17 days, respectively). Since the 19-year cycle was recognized as superior to the 27-year Sirius period by the end of the sixth century BC,[14] this suggests that the 6247-month period is the oldest period in the lunar theory and that its derivation dates from before 500 BC.

– The reason why the system B lunar period of 251 months does not fit an integer number of years may be related to the way in which that period was derived as will be discussed later in this paper.

– If function Φ was originally meant to represent the time difference between two eclipses one Saros apart as first suggested by Neugebauer,[15] it involved Full Moon dates only (function $\Phi_2$), running from one Full Moon to the next one with a time step of one synodic month. The new moon function $\Phi_1$ was probably derived later from an intermediate daily variant function $\Phi^*$ by applying a phase shift of 15 tithis's with respect to the $\Phi_2$-values.[16] It is consistent with this scenario that the original function $\Phi_2$ – and not $\Phi_1$ – contains the 'nice value' 2,0,0,0,0,0 which may have been adopted as initial value. The values of function Φ are generally 'dirty' sexagesimal numbers with 5 'decimal' places.[17] Of all 6247 entries of function $\Phi_2$ only two are 'nice' numbers, both having the value 2,0,0,0,0,0, one on an ascending branch and one on a descending branch. Note that 2 'large hours' correspond to 2,0 UŠ,[18] equivalent to 480 minutes of time or 8 equinoctial hours, indeed about the average time interval between eclipse times of two eclipses one Saros apart. The average value of function $\Phi_2$ equals 2;7,26,26,20,0 'large hours', equivalent to 8 ½ equinoctial hours.

14 Britton 2002, 30.
15 Neugebauer 1957.
16 See HAMA, 499–502.
17 This may be illustrated by listing an arbitrary set of five consecutive values of function Φ: 2,2,45,55,33,20; 2,5,31,51,6,40; 2,8,17,46,40,00; 2,11,3,42,13,20; 2,13,49,37,46,40; etc.
18 See HAMA, 367.

- Using the known relation of function $\Phi_2$ to the Babylonian lunar calendar,[19] one finds that $\Phi_2$ attained the value 2,0,0,0,0,0 (on an ascending branch) on the Full Moon date of month VIII in year 1 of Cambyses, corresponding to Julian date 17 November 529 BC, a date listed in the Early Saros Scheme[20]. On this date a partial lunar eclipse took place in Babylon with first contact occurring 45 UŠ after sunset. The observation of this eclipse is recorded in the lunar eclipse text BM 36879.[21] The date associated with the other value 2,0,0,0,0,0 of $\Phi_2$ (on a descending branch) is the Full Moon date of month VII of year 25 of Artaxerxes I, corresponding to 26 October 440 BC. This date is not associated with a lunar eclipse.
- From ephemerides of system A one finds that functions $\Phi$ and F do not only have the same period but also have the same phase. This is somewhat counter-intuitive because function $\Phi$ is supposed to model the time elapsed between two lunar events a whole number of lunar anomaly periods apart while function F models orbital velocity (the lunar anomaly itself) so that one might a priori expect them to be 180° out of phase rather than in phase. Following a suggestion by John Britton, Aaboe provides an explanation for this.[22]
- Finally I note that the accuracy of the astronomical parameters implicit in the periods and wave numbers displayed in Tab. 1 is remarkably good. Dividing the periods $\Pi$ by the wave numbers Z we display in the last column of Tab. 1 the length of the anomalistic lunar period (the number of synodic months after which the Full Moon returns to the perigee of the lunar orbit, the point of closest approach of the Moon to the Earth and – by definition – the position of maximum lunar velocity) and the length of the (sidereal) solar year expressed in synodic months (the basic Babylonian unit of time). Apparently Babylonian astronomers managed to determine these parameters with an accuracy of about $10^{-5}$ and $10^{-4}$, respectively.

## 2 The 27-year Sirius period and the solar year

The first visibility of the bright star Sirius has played an important role in Babylonian calendar regulation from early times onward. This is attested by several passages in the astronomical compendium MUL.APIN[23] dating from the late second millennium BC.[24] It ultimately resulted in the intercalation pattern of the 19-year calendar cycle adopted in Babylonia shortly after 500 BC.[25]

19 See HAMA, 484.
20 Steele 2000.
21 See Huber and De Meis 2004, 94.
22 Aaboe 1968, 10–11.
23 Hunger and Pingree 1999, 57–83.
24 De Jong 2007.
25 Sachs 1952; Britton 2007a.

The early text BM 45728 containing Babylonian period relations includes a 27-year Sirius period. This text was first discussed by Kugler and dated by Britton to around 600 BC.[26] Use of the 27-year Sirius period is attested in the early text BM 36731+ in which rising and setting dates of Sirius are computed for the years 627–562 BC.[27]

Observations of the first visibility of Sirius show that after 27 years Sirius rises again on about the same date in the Babylonian lunar calendar. This implies that 27 solar (sidereal) years correspond to 334 synodic months. This period relation is not very accurate because the dates shift backward by about 1.5 days in the lunar calendar after each cycle. Due to variations in the atmospheric extinction (weather) the dates of first visibility of Sirius may vary by up to about 3 days around the nominal date so that it may have taken the Babylonian astronomers about one century before they found out about the limited accuracy of the 27-year period.

One interesting aspect of the 27-year cycle is the implicit existence of the 8-year and 19-year cycles. A period of 8 years corresponds to $8/27 \times 334 = 98;57,46,40$ synodic months and 19 years corresponds to 235;02,13,30 months. Thus according to the 27-year cycle 99 months is 1;06,40 tithi longer than 8 years, while 235 months is 1;06,40 tithi shorter than 19 years. Around 500 BC when the 19-year cycle was adopted as the fundamental calendar cycle, the Babylonian scholars had apparently realized that the 27-year cycle was about 1–2 tithis short and that the 19-year cycle was of superior accuracy.

## 3 Lunar Four observations and the Saros

The velocity of the Moon varies during its course through the heavens. Thanks to Johannes Kepler (1571–1630) we know now that this variability is due to the ellipse form of the lunar orbit. The Moon reaches its largest velocity ($\sim 16°$ per day) at perigee (minimum distance to the Earth) and its lowest velocity ($\sim 12°$ per day) at apogee (maximum distance to the Earth). The perigee progresses about 3° per synodic month so that it takes the Moon longer to return to its perigee (27.55 days) than to return to the same position in the sky (27.32 days). Since the Sun moves about 30° per month it takes even longer for the Moon to move from one Full Moon to go the next one (29.53 days). The deviation from circularity of the lunar orbit is known as its anomaly (after Ptolemy) and the time it takes for a Full Moon at perigee to return to the next Full Moon at perigee (13.94 synodic months) is called the anomalistic period of the Moon. After one anomalistic period the Moon has completed 15 orbital revolutions and an additional 24° in the sky. The ellipse form of the lunar orbit is a modern notion; Babylonian astronomers were thinking in terms of variable velocity of the Moon.

26 Kugler 1907, 45–48; Britton 2002, 26.

27 Britton 2002.

Early Babylonian awareness of a roughly 14-month velocity period of the Moon is attested in Atypical Text C, first discussed by Neugebauer and Sachs and most recently by Brack-Bernsen and Steele.[28] This awareness probably originates from inspection of long sequences of so-called Lunar Four data,[29] which were routinely recorded in the Diaries. The Lunar Four consist of the set of four observations of the time elapsed between sunrise/-set and moonrise/-set on days around full moon: ŠU (sunrise to moonset around sunrise), NA (moonset to sunrise around sunrise), ME (moonrise to sunset around sunset), and $GE_6$ (sunset to moonrise around sunset).

The earliest collection of Lunar Four observations dates from the late seventh century BC (BM 38414).[30] The well-preserved text Strassmaier Cambyses 400 (BM 33066) contains Lunar Four observations for the seventh year of Cambyses II (523/522 BC). The fact that the data set in Cambyses 400 is virtually complete implies that missing observations (e.g. due to bad weather) must have been filled in by some predictive method. Britton suggests that most probably previous data – one or more Saroi back – were used for this.[31]

Brack-Bernsen and Schmidt have shown that the Lunar Four observations play an important role in the early development of Babylonian lunar theory.[32] They realized that the sum of the observed values of the Lunar Four, a quantity they called Σ, provides a good approximation to twice the lunar velocity at full Moon. Thus the availability of long sequences of Lunar Four observations enabled the Babylonian scholars to study the variability of the lunar velocity. In this way they must have first discovered the crude 14-month period in the return of the Full Moon to maximum (or minimum) lunar velocity and later the refinement of this period to $223/16 = 13.9375$ synodic months based on the Saros period of 223 synodic months. The latter is based on the realization that 16 lunar velocity periods are more accurately approximated by 223 than by 224 synodic months.

Lunar and solar eclipses are already mentioned in the Old-Babylonian omen series 'Enuma Anu Enlil' (second millennium BC). Reports and letters sent by Assyrian and Babylonian astronomers to the Assyrian kings Esarhaddon and Assurbanipal in the seventh century BC show awareness that lunar eclipse possibilities occurred at intervals of 6 and (occasionally) 5 months. Lunar eclipses were recorded routinely in the Diaries. The oldest preserved Diary dates from 652 BC.

From the available texts it appears that at the end of the seventh century BC, a detailed scheme to predict lunar eclipses based on an 18-year cycle (the so-called Saros) had been worked out.[33] These texts suggest that apparently a continuous lunar eclipse record was available from $\sim$750 BC onward.

28 Neugebauer and Sachs 1967; Brack-Bernsen and Steele 2011.

29 See Hunger and Pingree 1999, 196–198.

30 Huber and Steele 2007.

31 Britton 2008; see also Brack-Bernsen 2002.

32 Brack-Bernsen and Schmidt 1994.

33 Steele 2000.

The Saros consists of a sequence of 38 lunar eclipse possibilities distributed in a fixed pattern of groups of 8 or 7 eclipses at 6-months intervals, each group separated from the next one by a 5-month interval, altogether totaling 223 synodic months, equivalent to about 18 years. After one Saros the Sun, Moon, and Earth return to approximately the same relative geometry and a nearly identical lunar eclipse will occur. The Saros derives from the approximate equality: 223 synodic months ($29^d.530588$) $\approx$ 242 draconitic months ($27^d.212220$) $\approx$ 239 anomalistic months ($27^d.554550$) $\approx$ 6585 1/3 days = 18 years + 10 (or 11) days + 8 hours. The Saros is referred to in the texts as '18 MU.MEŠ' ('18 years').

It turns out that all eclipses mentioned in Babylonian astronomical texts – either predicted or observed – between 750 and 300 BC are part of the so-called 'Early Saros Scheme.'[34] The Early Scheme breaks down around 300 BC because the resonances between the different periods on which it is based are not perfect. There is evidence that the Saros scheme was revised several times after 300 BC. The revision around 260 BC resulted in the so-called 'Saros Canon.'[35]

The Babylonian scholars must have discovered the Saros period by inspecting their large database of hundreds of lunar eclipses and recognizing that after one Saros lunar eclipses repeat with similar magnitude, occultation pattern and duration.[36] This similarity evolves quite slowly so that it typically persists for some hundred years for successive eclipses in one and the same line of the Saros scheme.

Measured in days the Saros period corresponds to 6585 1/3 days so that for lunar eclipses in one and the same line of the Saros scheme, we have:

- After three Saros periods (about 54 years) similar lunar eclipses occur at about the same time of night,
- Lunar eclipses in a Saros line often occur in pairs, separated by one or two unobservable day-time eclipses.

## 4 The 6247-month lunar period

After 223 synodic months the Sun has progressed $\sim 10^\circ$ with respect to its position one Saros earlier so that the exact length of the Saros is affected by the variable velocity of the Sun (the solar anomaly). At maximum solar velocity 10 days (the excess of one Saros over 18 years) correspond to $\sim 10^\circ$ and at minimum velocity to $\sim 9^\circ$ so that the average time between two eclipses one Saros apart may differ by about 2 hours (the time for the Moon to traverse $1^\circ$).

34 Steele 2000.

35 The Saros scheme discovered by Strassmaier in the 1890s; see Aaboe, Britton, et al. 1991.

36 Pannekoek 1918.

Thus an improved lunar velocity period can be derived by eliminating the effect of solar anomaly or – phrased in Babylonian language – the variable velocity of the Sun in its orbit. As we know from Babylonian planetary theory this is achieved by constructing a new 'great' period from a linear combination of shorter observed periods such that they span an integer number of solar years.

For this construction we need a relation between the length of the Saros, the period after which the Moon returns to its orbital velocity (the lunar anomalistic period), and the solar year after which the Sun returns to its orbital velocity (the solar anomalistic period), both expressed in synodic months. By definition the Saros is equivalent to 16 lunar velocity periods spanning 223 synodic months. The length of the solar year may be expressed in synodic months by using the Sirius period relation discussed above where we have seen that 27 solar years correspond to 334 synodic months. We then find that one solar year lasts $^{334}/_{27} = 12.37$ months, corresponding to 12 months and 11 days, or approximately 12 $^{1}/_{3}$ months. The Sirius period relation further implies that three Saroi (669 synodic months) correspond to 54 solar years (668 synodic months) + 1 month so that one Saros of 223 synodic months lasts 18 years + $^{1}/_{3}$ month. Using these relations the Babylonian astronomers may have computed the smallest common multiple of the synodic month, the solar year and the Saros to find that

$$
\begin{aligned}
37\,\text{Saroi} &= 37 \times 223\,\text{months} = 8251\,\text{months} \\
&= 37 \times (18\,\text{years} + {}^{1}/_{3}\,\text{month}) = 666\,\text{years} + 12\,{}^{1}/_{3}\,\text{months} = 667\,\text{years}.
\end{aligned}
$$

Thus 37 Saroi are equivalent to $37 \times 16 = 592$ lunar velocity periods or $37 \times 223 = 8251$ synodic months and last 667 solar years.

Using this relation as a starting point I display in Tab. 2 other relations that fit within an integer number of years by subtracting 54 years (= 3 Saroi − 1 month) in steps. The entries in Tab. 2 are the only linear combinations of an integer number of Saroi and at most 12 lunar months that result in periods of an integer number of solar years defined according to the 27-year Sirius period relation. These relations may also be considered as linear combinations between 223-month periods (Saroi) and the more primitive 14-month periods providing improved approximations to the lunar velocity period. Thus starting with $8251 = 37 \times 223 + 0 \times 14$ months making up 592 lunar velocity periods, we have $7583 = 33 \times 223 + 16 \times 14$ months making up 544 velocity periods, $6915 = 29 \times 223 + 32 \times 14$ months making up 496 velocity periods, $6247 = 25 \times 223 + 48 \times 14$ months making up 448 velocity periods, etc.

| Period [years] | Saroi | Months | Months added | Π [months] | Largest factor | Z | Largest factor | Vel. per. [months] |
|---|---|---|---|---|---|---|---|---|
| 667 | 37 | 8251 | 0 | 8251 | 223 | 592 | 37 | 13.937500 |
| 613 | 34 | 7582 | 1 | 7583 | 7583 | 544 | 17 | 13.939338 |
| 559 | 31 | 6913 | 2 | 6915 | 461 | 496 | 31 | 13.941532 |
| 505 | 28 | 6244 | 3 | 6247 | 6247 | 448 | 7 | 13.944196 |
| 451 | 25 | 5575 | 4 | 5579 | 797 | 400 | 5 | 13.947500 |
| 397 | 22 | 4906 | 5 | 4911 | 1637 | 352 | 11 | 13.951705 |
| 343 | 19 | 4237 | 6 | 4243 | 4243 | 304 | 19 | 13.957237 |
| 289 | 16 | 3568 | 7 | 3575 | 143 | 256 | 2 | 13.964844 |
| 235 | 13 | 2899 | 8 | 2907 | 17 | 208 | 13 | 13.975962 |
| 181 | 10 | 2230 | 9 | 2239 | 2239 | 160 | 5 | 13.993750 |
| 127 | 7 | 1561 | 10 | 1571 | 1571 | 112 | 7 | 14.026786 |
| 73 | 4 | 892 | 11 | 903 | 301 | 64 | 2 | 14.109375 |
| 19 | 1 | 223 | 12 | 235 | 47 | 16 | 2 | 14.687500 |
| (i) | (ii) | (iii) | (iv) | (v) | (vi) | (vii) | (viii) | (ix) |

**Tab. 2** Linear combinations of Saros periods and lunar months resulting in an integer number of solar years.

Based on the data in Tab. 2 one can make the following observations:

- The 6247-month period is among the constructed periods Π listed in column (v).
- The values of the 'great' periods Π are often prime numbers and not reducible to products of nice numbers as follows from the largest factors in column (vi).
- The wave numbers Z result from multiplication of the number of Saroi in column (ii) by 16 (the number of anomaly periods contained in one Saros)
- Only about half of the wave numbers Z are reducible to factors smaller than 10 (column (viii)).
- The relation 28 Saroi + 3 months = 6247 months which underlies the derivation of the 6247-month period naturally explains the ratio 3/28 which plays a central role in the arithmetical structure of function Φ and in the computation of function G.[37]

37 See HAMA, 484–488 and 497–499.

– The 6247-month period provides the best approximation to the modern value of the anomalistic period of 13.943355 synodic months (column (ix)).

Why did the Babylonian scholars select the 6247-month period from the possible periods listed in Tab. 2? I propose that the answer to this question may be sought in a combination of astronomical considerations and numerical convenience. It is clear that a Saros of 223 months provides a much better approximation to 16 lunar velocity periods than 14 months to one velocity period. This implies that the best 'great' period $\Pi$ must be chosen from the candidate periods in the upper half of Tab. 2 because they may be considered as linear combinations of 223-month and 14-month periods which are most strongly dominated by the 223-month period. Among those candidate periods the 6247-month period is the only one for which all 6247 function values are different because 6247 is a prime number *and* for which the wave number Z is reducible to a small fairly 'nice' integer number. The fact that the 6247-period also provides the most accurate approximation to the value of the lunar velocity period must then be considered as accidental.

## 5 Function F of system A

Brack-Bernsen and Schmidt were the first to realize that the sum of the Lunar Four (designated $\Sigma$ by them) provided a good approximation to twice the lunar velocity around Full Moon.[38] That this must have provided the basis for the choice of the other parameters characterizing function F of system A (the amplitude and the average value) can best be demonstrated by showing how remarkably well the lunar velocity function F of system A reproduces the observed $\Sigma/2$ values. This is done in Fig. 1 where I have plotted $\Sigma/2$-values during 10 years in the middle of the sixth century BC together with function F values computed from its defining parameters: $\Pi = 6247$, $Z = 448$, $d = 0°;42$, and $\mu = 13°;30,30$.[39] Notice that 42 is a multiple of 7, the largest factor in the wave number Z (column (viii) in Tab. 2). In view of the excellent fit of function F to the variable lunar velocity I believe that the 6247-month period was first and foremost constructed to provide an improved lunar velocity period and that function F was the first lunar function developed.

38 Brack-Bernsen and Schmidt 1994.

39 See HAMA, 478–479.

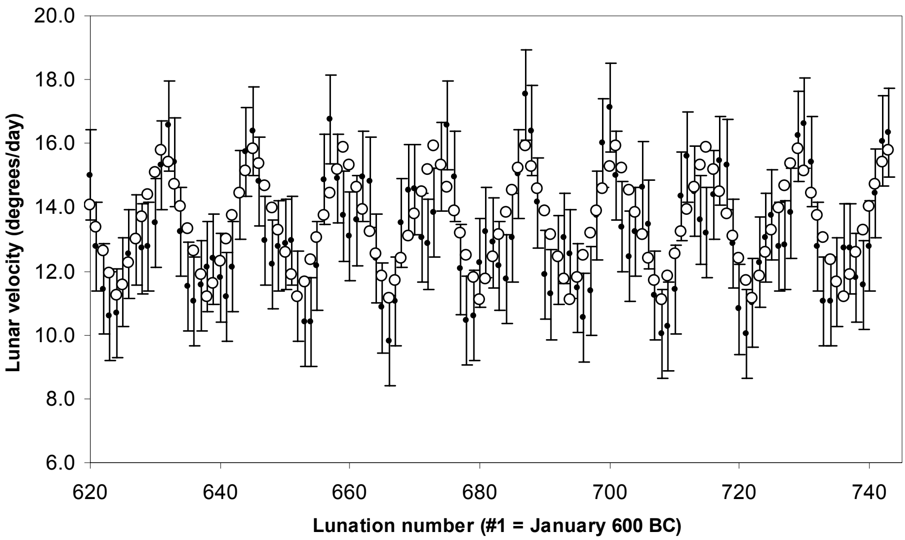


**Fig. 1** Synthetic lunar velocity data (small dots with error bars) and function $F_2$ of system A (large open dots) for the years 550–540 BC. The lunar velocity data are computed from synthetic Lunar Four data taken from a database generated for the period 750–0 BC. Error bars in the (synthetic) observational data are estimated by comparing the Cambyses 400 Lunar Four data (Britton 2008) with synthetic data and noting that the errors in Σ (the sum of the Lunar Four) are twice those in the individual Lunar Four values and that Σ/2 is displayed.

## 6 Function Φ of system A

According to the text BM 36705+ function Φ was meant to represent the magnitude of the change in the time difference (of about 8 hours) between two lunar eclipses one Saros apart,[40] and thus originally applied to Full Moon dates only (designated $\Phi_2$).[41] The text mentions the small number 0;17,46,40 as the magnitude of this change. Since 0;17,46,40 UŠ corresponds to 1;11,6,40 minutes of time it follows that function Φ predicts that the time difference between two eclipses one Saros apart changes only very slowly. This is qualitatively in agreement with observation but quantitatively too small because in reality eclipse time differences change by up to about 3 UŠ between eclipses one Saros

40 Neugebauer 1957.

41 An alternative interpretation of function Φ was suggested by Brack-Bernsen (Brack-Bernsen 1990; Brack-Bernsen 1997). Struck by the fact that function Φ provided a remarkably good fit to the sum Σ of the Lunar-Four with 100 UŠ added, she suggested that function Φ was meant to represent the quantity Σ + 100 UŠ. Her suggestion was recently criticized and refuted by Britton (2009, 415–416). However, Brack-Bernsen's basic observation that function Φ and the quantity Σ are in phase is correct. Instead I have argued above that the fact that the quantity Σ provides a good approximation to twice the daily lunar velocity at Full Moon (see Fig. 1) may have been used by the Babylonian scholars to construct function F rather than function Φ.

apart.[42] In view of the limited accuracy of their measurement of eclipse times,[43] it is amazing that the Babylonian scholars managed to observe this small gradual change at all. They correctly concluded that this slow change must be due to the variable lunar velocity and could therefore be modeled by a zigzag function with the same period and wave number as the lunar velocity function F. The fact that they also realized that functions Φ and F have the same phase is more miraculous because that is far from obvious as argued earlier in this paper.[44]

I will show elsewhere[45] that function $\Phi_2$ indeed provides a fairly satisfactory fit to the differences of eclipse times between successive lunar eclipses for all 38 Saros lines in the Saros scheme. This fit is superior to the early zigzag function in BM 45861[46] in the sense that one and the same function $\Phi_2$ fits eclipse time differences both for odd *and* even Saros lines and that it models the slow change in the eclipse time difference with time but it is inferior in the sense that the accuracy with which it fits the eclipse time differences as a whole is less than that of the fits of the zigzag function in BM 45861 for odd and even Saros lines separately.

In the early texts in which full-fledged versions of functions Φ and F are encountered they are given in their truncated form,[47] while in the later ephemerides we only find the pure versions. I think that this may have to do with the fact that initially $\Phi_2$ was meant to model eclipse time differences but that later its use in the ephemerides was limited to the chronological connection of ephemerides and to its application as auxiliary function for the computation of function G.[48]

## 7 Function F of system B

Lunar velocity periods of 251 and 223 lunar months are very hard to detect in Lunar Four observations because their effect is drowned in the more crude but fairly obvious 14-month period. The fact that 251 synodic months, the period chosen for function F of system B, equals $223 + 2 \times 14$ synodic months is not of much help, because it is not clear why this particular linear combination of 223 and 14 synodic months may have been chosen. I believe that the most straightforward way by which Babylonian astronomers

42 See Appendix C1 of Britton 2007b.

43 In Mesopotamia eclipse times were measured with respect to sunrise and sunset presumably with the aid of water clocks. Steele, Stephenson, and Morrison (1997) have shown that the random errors in the Babylonian measurements of eclipse times amount to about 2 UŠ while systematic errors of about 10% are expected due to clock drift.

44 See again Aaboe 1968, 10–11.

45 This paper is a progress report of a more extensive study on the early development of Babylonian lunar theory that I intend to publish separately.

46 Discussed by Steele 2002 and Brack-Bernsen and Steele 2005.

47 Aaboe and Sachs 1969.

48 See HAMA, 505–513.

were able to single out the velocity period of 251 synodic months is based on its superior accuracy as derived by continuing function F of system A.

We have seen above that after one Saros of 223 synodic months function $\Phi_2$ returns to a value that differs from its previous value by the small amount of 0;17,46,40 UŠ. Similarly, according to function F of system A (which has the same amplitude and phase as function Φ but a different amplitude and initial value), after 223 months the lunar velocity attains a value that differs only 0°;4,30 per day from its value 223 months before. While this is quite a small difference it is not the smallest one possible. It is easy to show by numerically continuing function F of system A that the smallest difference between all possible pairs of its 6247 function values is 0°;0,5,37,30 per day for a pair separation of 2998 synodic months. The next one up has a velocity difference of 0°;0,11,15 per day, twice larger than the smallest value, at a pair separation of 251 months. This 'period' is the first one found by continuing function F beyond 223 months, and the one apparently chosen for function F of system B.

In view of the algorithms developed by Babylonian astronomers to numerically check some of the computations in their ephemerides, one would expect that they should also have been able to find the larger more accurate velocity period of 2998 months but that the 251-months period was chosen because of numerical convenience. The fact that 251 synodic months does not correspond to an integer number of solar years might indicate that it was indeed not found from a linear combination of smaller periods as is the case for most other Babylonian lunar and planetary periods. The argument presented here for the choice of the 251-month period for function F of system B suggests that system B must have been developed after system A.

## 8 Early solar models

The most obvious starting point for early solar models is the 27-year Sirius period because it defines a period after which the Sun returns exactly to its position in the sky expressed in synodic months, the time unit of Babylonian astronomy. Now, as we have seen before, the accuracy of the 27-year period is limited because the lunar calendar date of first visibility of Sirius regresses ~1 day after 27 years. Thus a better approximation is provided by a modified 27-year period: 334 synodic months − 1 day = 27 solar years. Making use of the identity 30 days (tithis) = 1 month, and multiplying both sides of this relation by 30 we immediately find the system B solar period relation: 10 020 − 1 month = 10 019 months = 810 years (see Tab. 1). Cast in Babylonian sexagesimal notation we find that after a period of Π = 2,46,59 months the Sun has completed Z = 13,30 revolutions. This period relation is used in system B to construct the zigzag function A for the solar velocity. It yields a year length of $10019/810 = 12;22,8,53,20$ synodic months.

Since year-length enters a lot of astronomical computations it is convenient to use a truncated or rounded-off value, i.e. 12;22,08 or 12;22,09 synodic months. Both values can be translated into period relations. We find: $\Pi = 2783$ (46,23), $Z = 225$ (4,45) for a year-length of 12;22,08 months, and $\Pi = 14843$ (4,7,23), $Z = 1200$ (20,0) for 12;22,09 months. Apparently Babylonian astronomers chose the smaller period for their system A function B, possibly because an 'epact'[49] of 11;04 tithis is more attractive for computational purposes than 11;04,30 tithis.

## 9 Discussion

I begin this discussion about the time frame and evolution of the early phase of Babylonian lunar theory by noting that function $\Phi$ was originally constructed to represent eclipse time differences and thus by definition applied to full moon dates only (designated $\Phi_2$). Using the known relation of $\Phi_2$ to the Babylonian calendar, we have seen that $\Phi_2$ attains the value 2,0,0,0,0,0 on day 13, month VIII in year 1 of Cambyses, corresponding to Julian date 17 November 529 BC, the date of an attested lunar eclipse listed in the Early Saros Scheme.[50] I suggest that this nice sexagesimal number was chosen as initial value of function $\Phi_2$. Notice that Brack-Bernsen and Steele in their analysis of the early attempt to fit eclipse time differences by zigzag functions in BM 45861 suggest that these functions were constructed around 530 BC,[51] surprisingly close to the eclipse date of the initial value of the more sophisticated function $\Phi_2$.

The lunar eclipse one Saros after the one of 17 November 529 BC took place in the morning of day 13, month VIII in year 11 of Darius, corresponding to Julian date 29 November 511 BC. This lunar eclipse was visible in Babylon with first contact occurring at 40 UŠ before sunrise,[52] but there is no record of this eclipse in presently known astronomical cuneiform texts. The eclipse time difference between these two eclipses is 2,01 UŠ, within the measurement error identical to the initial value adopted for function $\Phi_2$. This does not only hold for this eclipse pair but it can be shown that the average eclipse time difference between all lunar eclipses in this Saros line during the sixth and the first half of the fifth century BC equals $2,0 \pm 0,01$ UŠ.[53]

49 The 'epact' is defined as the excess of a solar year over the lunar year of 12 synodic months. An epact of 11;04 tithis is also used in Babylonian planetary theories, both of system A and B (Neugebauer 1975, 395–396).

50 Steele 2000.

51 Brack-Bernsen and Steele 2005.

52 Huber and De Meis 2004, 188.

53 See Britton 2007b, Appendix C1.

If the 529 BC lunar eclipse is indeed associated with the initial value of function $\Phi_2$ it provides a 'terminus post quem' for the construction of functions Φ and F. If the full-fledged versions of both functions for the years 475–457 BC in BM 36737+ may be considered as a 'terminus ante quem' functions Φ and F were conceived before 450 BC. Of course, the conception date may have been later if the computations in the text were carried out for comparison with older data. A strong argument in favor of an early date for the derivation of the 6247-month period is provided by the fact that it is partly based on the 27-year Sirius cycle which was used for calendar purposes in the sixth century BC but was superseded by the 19-year cycle around 500 BC.[54] This suggests that functions F and Φ were conceived in the late sixth century, consistent with an initial value for $\Phi_2$ associated with the lunar eclipse of November 529 BC.

Recently Britton has published a detailed study of Babylonian lunar theory in which he suggests that it dates from shortly after 404 BC and that its creation may be attributed to one single author.[55] While the final version of the theory as we know it from the lunar ephemerides of the Seleucid and Arsacid era may well have been formulated by one single Babylonian scholar I prefer to think that it is the end product of a more gradual process to which several generations of Babylonian scholars have contributed. As argued here this gradual process may have started with the construction of the improved lunar anomaly period of 6247 synodic months on which functions F and Φ of system A are built.

Britton anchors function $\Phi_2$ in time by using the shortest 6-month time interval between two lunar eclipses since systematic records were maintained (about 750 BC) and by assigning the associated Φ-value of 2,8,53,20 to the syzygy corresponding to the eclipse of 18 August 404 BC at the end of this interval.[56] He assumes that the final formulation of Babylonian lunar theory was completed shortly afterwards and he uses this as constraints for dating the invention of the Babylonian theoretical zodiac.[57] I must confess that I find his reasoning far from convincing. One – but not the only – reason for this is that dating the minimum 6-month eclipse time interval by Babylonian observers is doomed to be extremely uncertain because the accuracy with which they could determine this interval from observed eclipse times is of the order of 1 hour (see note 43 above).

If my suggestion for the construction of the solar periods for system A and B is correct they both derive from the same refined 27-year Sirius relation. In system B the period relation obtained was directly used for the construction of function A. In system A the original period relation was modified to obtain a numerically suitable value

54 Britton 2002; Britton 2007a.

55 Britton 2007b; Britton 2009; Britton 2010.

56 Britton 2009, 404–405.

57 Britton 2010.

of the year length, associated with the derivation of function B. This does not provide a direct clue about the priority of the two functions.

Given the fact that the lunar function F (system A) may already have been fully developed by about 450 BC and that a primitive version of the solar function B (system A) with a year length of 12;23 months was used in a text from about 400 BC,[58] we may conclude that the system A solar model (function B) was developed after the lunar velocity model (function F) and that it took at least half a century, and probably longer, to reach its canonical form.

In summary, I propose that the development of Babylonian lunar theory was a gradual process. It started in the late sixth century BC with the derivation of the 'great' period of 6247 synodic months for the lunar velocity variation. Based on this period the lunar velocity function F and the eclipse time difference function Φ of system A were constructed shortly afterwards. The next step was to model the position of the Moon at syzygy and during eclipses. Therefore the position of the Sun at syzygy was needed, as well as a theoretical coordinate system. Several early attempts of system A-type solar functions are textually attested (BM 36737+ and BM 36822+). The Babylonian 360° zodiac may have been introduced around 450 BC while it took until the early fourth century BC before the solar longitude function B of system A reached its canonical form. System A lunar theory was apparently finished by 320 BC. System B lunar theory may have been a later invention, possibly dating from around 300 BC.

58 Aaboe and Sachs 1969, Text A.

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## Illustration and table credits

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# BM 76488 – a Babylonian Compendium about Conjunctions and Other Planetary Phenomena

## Summary

This paper discusses the cuneiform tablet BM 76488, which partly preserves a hitherto unknown Babylonian compendium about planetary phenomena. In several of the preserved sections, periods are assigned to pairs of planets – a topic not attested elsewhere in Mesopotamian astral science. The analysis presented here suggests that some of the periods describe the empirical behavior of planetary conjunctions.

Keywords: Babylonian astronomy; planets; synodic phenomena; conjunctions; periods.

Dieser Beitrag beschäftigt sich mit der Keilschrifttafel BM 76488, auf der ein bisher unbekanntes babylonisches Handbuch zu Planetenphänomenen teilweise erhalten ist. In mehreren der erhaltenen Sektionen werden Perioden zu Planetenpaaren zugeordnet – ein Thema, das an keiner anderen Stelle in der mesopotamischen Sternkunde belegt ist. In der präsentierten Analyse wird vorgeschlagen, dass einige der Perioden das empirisch erfasste Verhalten von Planetenkonjunktionen beschreiben.

Keywords: Babylonische Astronomie; Planeten; Synodische Phänomene; Konjunktionen; Perioden.

The author thanks the Trustees of the British Museum for providing access to their cuneiform collection and for permission to publish the tablet.

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The fragment BM 76488 (measures: 9.2 × 7.6 × 2.2–3.2 cm) preserves the upper part of the obverse and the lower part of the reverse of a tablet (Figs. 1–2).[1] The obverse is heavily eroded and very difficult to read. The lower part of the obverse is partly blackened, presumably as a result of burning. On the reverse the surface is in a better condition, except for two damaged spots in column ii. The provenance of the tablet cannot be established with certainty. The remains of a colophon on the reverse do not preserve a date, place or name of a scribe. The tablet belongs to the Sippar collection of the British Museum, where it was registered on 18 January 1883 (accession number: 83-1-18, 1858).[2] This lot comprises five cases of tablets that were excavated unscientifically by Hormuzd Rassam and his coworkers in Babylon, Sippar (Abu Habba), Borsippa (Birs Nimrud), as well as one or more Assyrian sites.[3] Several features allow us to narrow down the provenance and date of the tablet. Since it is inscribed in Babylonian cuneiform, an Assyrian origin is unlikely. Moreover, its cushion-like shape would be unusual for Babylon, which speaks in favor of Sippar or Borsippa. A precise date cannot be determined, since the colophon does not mention one, nor does the text report any datable phenomena. According to the colophon, the tablet was copied from a wooden board (inlaid with wax). Orthographical and terminological features suggest that the tablet and the original text date between about 500 and 300 BCE. In particular, the occasional use of the Late Babylonian variant of the numeral 9 (rev. ii 9′, 22′) suggests that the tablet was written after about 450 BCE, while the use of *waṣābu* (DAḪ) for addition, instead of the synonymous *ṭepû* (TAB), suggests that the original was written before the Seleucid era (300 BCE).[4]

# 1 Transliteration and translation

## 1.1 Obverse

| | | | |
|---|---|---|---|
| §1 | (1) | IGI *u* ŠU$_2$ *ša*$_2$ $^{d}$SAG.ME.GAR *ana* ⸢IGI.LA$_2$-*ka* MU *ana* MU *ša*$_2$ x-*ka* xxx.MEŠ TA x⸣ IGI *u* ŠU$_2$ ⸢x⸣ [xx] | In order for ⸢you to see⸣ the appearance and setting of Jupiter, ⸢year by year: …⸣ the appearance and setting ⸢…⸣ […] |
| | (2) | ⸢xxx⸣-*ṣi ina* MU-*ka ina* 1⸢5 xx BE x⸣ [xxx] ⸢xxx *lu-maš* xx⸣ 30 ŠE$^?$ ⸢x DU x⸣ | ⸢…⸣ … in your year in 1⸢5 …⸣ […] ⸢… constellation (?) …⸣ 30 … ⸢…⸣ |

1 An earlier version of this paper was presented at the workshop *The Antikythera Mechanism: Science and Innovation in the Ancient World*, Leiden, 17–21 June 2013.

2 The tablet is listed in the third volume of the catalog of the Sippar collection (Leichty, Finkelstein, and Walker 1988, 63).

3 For the collection 83-1-18 see Leichty 1986, xxxiv; Leichty, Finkelstein, and Walker 1988, xii.

4 There are two instances of the word *lumāšu*, 'zodiacal sign' or 'constellation', in damaged or badly understood passages (obv. 2; rev. i 12′). If the former

**Fig. 1** BM 76488 Obverse.

| | | | |
|---|---|---|---|
| | (3) | ⸢xx⸣ $^{d}$UTU ⸢10 + x *u*$_4$⸣-*mu ana* ⸢UGU x DAḪ⸣ 17 *u*$_4$-*mu ana* ⸢UGU x DAḪ [xxx] ⸢EN?⸣ 12 MU.MEŠ ⸢xx⸣ *qaq-qar*.ME ⸢x⸣ | ⸢…⸣ the Sun, ⸢10 + x days you add⸣ to ⸢…⸣, 17 days you add to ⸢…⸣ […] ⸢until?⸣ 12 years, …, positions ⸢…⸣ |
| | (4) | ⸢xx *ina* NIM xxxxxx IM xxx *ina* xxxxx⸣ DAL 17 ⸢*u*$_4$-*mu* x *ki-i* xx⸣ DAL 13 *u*$_4$-*mu* LA$_2$ ⸢x⸣ | ⸢… in … wind … in …⸣ … 17 ⸢days … when …⸣ … 13 days lacking ⸢…⸣ |
| §2 | (5) | ⸢xx⸣ IGI ⸢MU.MEŠ xxxxxxxxxxxxxxxx xxxxxxxxx⸣-*tu*$_4$ ŠID-*ma* KI ⸢xxxxx⸣ | ⸢…⸣ appearance(?) ⸢years …⸣ … you compute, and … ⸢…⸣ |
| | (6) | [xxx] ⸢xxxxxxxxxxx⸣ IGI *u* ⸢xx⸣ [xxxx] A ⸢xxx⸣ 26 ⸢xxxx⸣ | […] ⸢…⸣ appearance and ⸢…⸣ […] … ⸢…⸣ 26 ⸢…⸣ |

translation is correct, which is entirely uncertain, this would suggest a date after about 400 BCE.

| | | | |
|---|---|---|---|
| | (7) | ⸢xxxxxxxxxxxxx⸣ [xxxxxxxxxx] ⸢xx⸣ *ki-i* ŠU$_2$ ⸢xx⸣ [xxxxxxx] | ⸢…⸣ […] ⸢…⸣ when setting(?) ⸢…⸣ […] |
| | (8) | ⸢xx KA 30 50 IB$_2$.ME.A GAR⸣-*an* ⸢xxxxx⸣ [xxxxx] ⸢x 20 x 20 xx⸣ [xxx] | ⸢… 30 50 you put down as a prediction. …⸣ […] ⸢… 20 … 20 …⸣ […] |

| | | | |
|---|---|---|---|
| §3 | (9) | ⸢IGI *u* ŠU$_2$⸣ *ša*$_2$ $^{d}$*dil-bat ana* ⸢xxxxxxxx⸣ [xxxx] ⸢1.30 BI xxxxx⸣ [xx] | In order for you to […] ⸢the appearance and setting⸣ of Venus ⸢…⸣ […] ⸢1.30 …⸣ […] |
| | (10) | 2 *u*$_4$-*mu* ⸢TA⸣ IGI 2 U$_4$ ⸢xxx⸣ [xxxxxx xxx] 30 20 IB$_2$.ME.A GAR-*an* | 2 days ⸢from the appearance, 2 days ⸢…⸣ […] 30 20 you put down as a prediction. |
| | (11) | 4 *u*$_4$-*mu* TA IGI ⸢5$^?$ *u*$_4$-*mu* x⸣ [xxxxxx] ⸢xxx IB$_2$.ME.A GAR-*an* xx⸣ [xx] | 4 days from the appearance, ⸢5$^?$ days …⸣ […] ⸢… you put down as a prediction. …⸣ […] |
| | (12) | ⸢x⸣ *u*$_4$-*mu* TA IGI 8 *u*$_4$-⸢*mu* xx⸣ [xxxxx] ⸢xxx IB$_2$.ME.A GAR-*an* x⸣ [xx] | ⸢…⸣ days from the appearance, 8 ⸢days …⸣ […] ⸢… you put down as a prediction. …⸣ […] |
| | (13) | ⸢xxxx⸣ *ina* 8 ⸢MU.MEŠ *ki*⸣-*i* 3 ⸢x⸣ [xxx] ⸢x MU x TA⸣ IGI *ana* ŠU$_2$ IB$_2$.⸢ME.A⸣ [GAR-*an* xx] | ⸢…⸣ in 8 ⸢years when⸣ 3 ⸢…⸣ […] ⸢year … from⸣ appearance to setting you [put down] as a ⸢prediction⸣. […] |
| | (14) | ⸢xxx 5 xxx⸣ *ina* 56 ME ⸢2 xx MU xx⸣ [xx] ⸢x⸣ [xxxxxx] | ⸢… 5 …⸣ in 56 days ⸢2 … year …⸣ […] 2 ⸢…⸣ […] |

| | | | |
|---|---|---|---|
| §4 | (15) | ⸢xxxxxxxxxx⸣ [xxxxxx] 30 ⸢xxx A xxx⸣ [xxxx] | ⸢…⸣ […] 30 ⸢…⸣ […] |
| | (16) | [xx] *ina* ⸢UGU xxxxxxxxxxxx⸣ [xxxx] | […] in ⸢…⸣ […] |
| | (17) | [xx] ⸢x⸣ 1.20 ⸢xxx BE xxxxxx⸣ [xxxx] | […] ⸢…⸣ 1.20 ⸢…⸣ […] |
| | (18) | [xx] ⸢MU.MEŠ *ki-i* xxxxxxx⸣ [xxxxx] | […] ⸢years when …⸣ […] |
| | (19) | [xx] ⸢xxxxx ½-*šu*$_2$ xxxxx⸣ [xxxxxxx] | […] ⸢… half of it …⸣ […] |
| | (20) | [xx] ⸢xxx AN *ana* MU-*ka* xx⸣ [xxxxxx xxxxxx] | […] ⸢… to your year …⸣ […] |

| | | | |
|---|---|---|---|
| §5 | (21) | [xxxxxxx] ⸢xxxxxxxxxx⸣ [xxxxxxxx] | […] ⸢…⸣ […] |

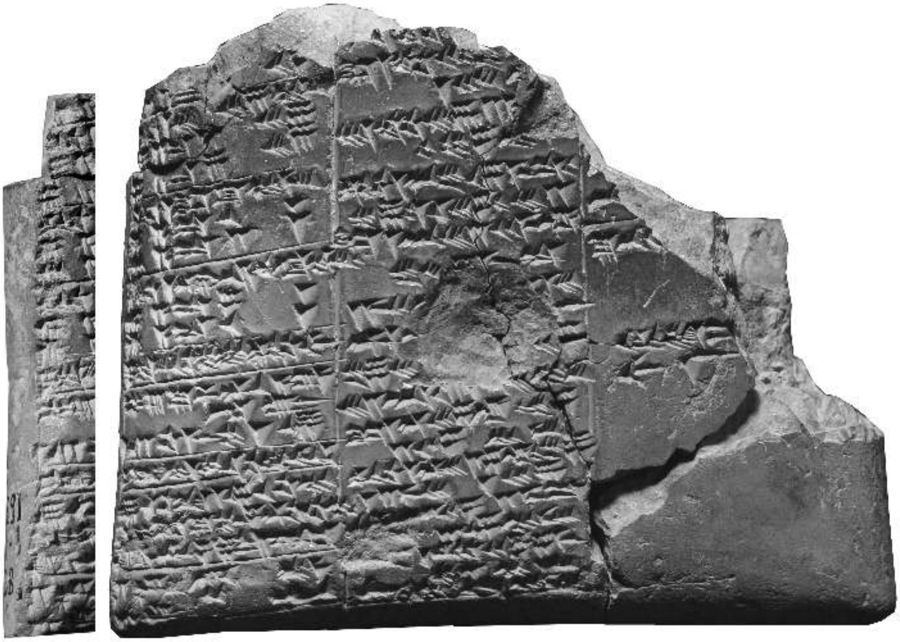

**Fig. 2** BM 76488 Reverse.

### 1.2 Reverse

*Column i*

| | | | |
|---|---|---|---|
| §6′ | (–1′) | [x *ana* x 24 MU.MEŠ] | [… to … 24 years.] |
| | (0′) | [x A.RA$_2$ x 24] | [… times … is 24.] |
| | (1′) | ⸢x⸣ A.RA$_2$ [x] 2⸢4⸣ | ⸢…⸣ times […] is 2⸢4⸣. |
| §7′ | (2′) | ⸢d⸣UTU *ana sin* 36 MU.⸢MEŠ⸣ | Sun to Moon 36 years. |
| | (3′) | ⸢18⸣ A.RA$_2$ ⸢2⸣ 36 | ⸢18⸣ times ⸢2⸣ is 36. |
| | (4′) | 6 A.RA$_2$ 6 36 | 6 times 6 is 36. |
| §8′ | (5′) | GU$_4$.UD *ana* GENNA 1-*šu* MU.MEŠ | Mercury to Saturn 60 years. |
| | (6′) | 6 A.RA$_2$ 10 1 | 6 times 10 is 1,0. |
| | (7′) | 30 A.RA$_2$ 2 1 | 30 times 2 is 1,0. |

| | | | |
|---|---|---|---|
| §9′ | (8′) | GU$_4$.UD *ana* $^{d}$*ṣal-bat-a-nu* 1-*šu* MU.MEŠ | Mercury to Mars 60 years. |
| | (9′) | 15 A.RA$_2$ 4 1 | 15 times 4 is 1,0. |
| | (10′) | 6 A.RA$_2$ 10 1 | 6 times 10 is 1,0. |
| §10′ | (11′) | 7 MU.MEŠ 10 *u*$_4$-*mu* DAḪ GU$_4$.UD IGI | 7 years 10 days you add, Mercury appears. |
| §11′ | (12′) | *bi-rit lu-maš* AN *u* ILLU | Inside a constellation (?): rain and flood, |
| | (13′) | *ina* NIGIN$_2$-*u*$_2$ | when it is surrounded (?). |
| | (14′) | 1.12 *u*$_4$-*me* ŠE 1 18 | 1.12 days … 1 18. |
| §12′ | (15′) | DU$_3$-⸢*uš*$^{?}$⸣ MU *ana* MU : 11 *u*$_4$-⸢*mu*⸣ IGI *u* ŠU$_2$ | Procedure. Year by year: 11 days, appearance and setting. |
| §13′ | (16′) | 19 11 UD NE 1 15 10 *u*$_4$-*mu* | 19 11 … 1 15 10 days. |
| | (17′) | ⸢*ki-i*⸣ TA DIB BU GAR AN UD 10 *u*$_4$-*mu* | ⸢When⸣ from … 10 days. |
| | (18′) | ⸢KIMIN x⸣-*ti* GAR 8 KASKAL.2 ⸢GUR UŠ⸣ | ⸢Ditto …⸣ … 8 … ⸢it turns back the path, becomes stationary⸣. |
| §14′ | (19′) | 2-*u*$_2$ DAL EDIN | Second, … open country. |
| | (20′) | 3-*u*$_2$ BAD SUKUD | Third, … high ground. |

*Column ii*

| | | | |
|---|---|---|---|
| §15′ | (1′) | ⸢*dil-bat ana* MUL$_2$.BABBAR⸣ [xx] | ⸢Venus to Jupiter⸣ […] |
| §16′ | (2′) | GENNA *ana* MUL$_2$.BABBAR 20 : ⸢50$^{?}$⸣ [ME DU$^{?}$] | Saturn to Jupiter 20 : ⸢50$^{?}$⸣ [days the *deficit* (?)] |
| | (3′) | 1.29 ⸢MU⸣.[MEŠ xx] | 1,29 ⸢years⸣ […] |
| §17′ | (4′) | *sin ana* MUL$_2$.BABBAR 36 2 ME [x] | Moon to Jupiter 36, 2 days […] |
| | (5′) | : 54 ⸢MU.MEŠ⸣ [xx] | : 54 ⸢years⸣ […] |

| | | | |
|---|---|---|---|
| §18′ | (6′) | GU$_4$.UD *ana dil-bat* 40 15 ME DU : | Mercury to Venus 40, 15 days the *deficit*: |
| | (7′) | 49 U$_4$ 11 34 : 32 | 49 days 11 34 : 32 |
| §19′ | (8′) | GENNA *ana dil-bat* 32 20 ME DIRI | Saturn to Venus 32, 20 days the excess. |
| | (9′) | 59 ⸢*6*$^?$ ME⸣ DU | 59, ⸢*6*$^?$ days⸣ the *deficit*. |
| | (10′) | 1-*me* 20 ⸢MU⸣.[MEŠ x] ⸢x⸣ 1.30 | 120 ⸢years⸣ […] ⸢…⸣ 1.30. |
| §20′ | (11′) | AN *ana* ⸢*dil*$^?$⸣-[*bat* xxx] LA$_2$ | Mars to ⸢Ve⸣[nus …] lacking, |
| | (12′) | 48 ⸢30⸣ [xxx] 24 | 48 ⸢30⸣ […] 24 |
| | (13′) | 32 *dil-bat* ⸢1.12 xx⸣.30 MU.MEŠ | 32, Venus,⸢1,12 …⸣.30 years. |
| §21′ | (14′) | GENNA *ana* AN 30 5 DIRI : 45 | Saturn to Mars 30; 5 the excess : 45, |
| | (15′) | 2.24 MU.MEŠ | 2,24 years. |
| §22′ | (16′) | GU$_4$.UD *ana* AN 32 5 DIRI : 48 | Mercury to Mars 32, 5 the excess : 48, |
| | (17′) | 1.2 ME 30 1.36 | 1,2 days 30 1,36. |
| §23′ | (18′) | ⸢*šamaš*$_2$(20) *ana* AN⸣ 2.24 2 ME DU | ⸢Sun to Mars⸣ 2,24, 2 days the *deficit*. |
| | (19′) | [xx] 2.15 MU.MEŠ | […] 2,15 years. |
| §24′ | (20′) | ⸢GENNA$^?$⸣ *ana* GU$_4$.UD 27 5 ME | ⸢Saturn$^?$⸣ to Mercury 27, 5 days, |
| | (21′) | 52 MU.MEŠ | 52 years. |
| §25′ | (22′) | ⸢*sin*$^?$ *ana*$^?$ GU$_4$.UD$^?$⸣ 19 MU.MEŠ | ⸢Moon$^?$ to Mercury$^?$⸣ 19 years. |

*Column iii*

| | | | |
|---|---|---|---|
| §26′ | (1′) | ⸢x⸣ [xxxxxxxxx] | ⸢…⸣ […] |
| | (2′) | *ina* ⸢20$^?$⸣ [xxxxxxxxx] | In ⸢20$^?$⸣ […] |
| | (3′) | UDU.IDIM ⸢x⸣ [xxxxxxx] | planet ⸢…⸣ […] |
| Col. | (4′) | *ul-tu* ⸢$^{giš}$⸣[DA xxxxxxx] | [copied] from a ⸢wooden⸣ [board …] |
| | (5′) | IGI.LA$_2$ [xxxxxxx] | checked […] |

## 2 Philological remarks

### 2.1 Obverse

(1) IGI *u* $\check{S}U_2$: the context implies that IGI and $\check{S}U_2$ denote the synodic phenomena of first appearance (IGI) and last appearance ($\check{S}U_2$). Their Akkadian readings are probably *nanmurtu*(IGI), 'appearance' and *rabû*($\check{S}U_2$), 'setting,' respectively.[5]

*ana amārika*($IGI.LA_2$-*ka*), 'In order for you to see,' is a common introductory phrase of Late Babylonian astronomical procedure texts.[6]

(3) DAḪ = *waṣābu*, 'to add' (lit.: 'to append').

(4) DAL (or RI?): interpretation unclear.

(8) $IB_2$.ME.A: this logogram, which also appears in obv. 10–12, is not mentioned in the dictionaries and sign lists. It is probably a variant spelling of *qību*(ME.A), 'prediction.' This is suggested by similar logograms in which a verbal root is preceded by the (pseudo-Sumerian) prefix $IB_2$, e.g. $IB_2.TAG_4$ = *riḫtu*, 'remainder.'[7]

### 2.2 Reverse

*Column i*

(12′) *birīt* = 'in between; inside' (prep.). *lumāšu* = 'constellation; zodiacal sign.' Perhaps a reference to a planet (Mercury?) standing inside a constellation and hence being surrounded by it (see 13′).

(13′) $NIGIN_2$-$u_2$: the phonetic complement suggests *lamû* G inf., 'to surround; be surrounded.'

(14′) The interpretation of this phrase remains unclear.

(16′) 19: the old version of the numeral 9 is used here.

(17′) GAR AN UD: even though the signs are clear, their correct reading is not obvious. Neither *ša*$_2$ $^{d}$UTU, 'of the Sun,' nor GAR-*an* UD, 'you put down ...' appears to yield a meaningful sentence.

(18′) The damaged sign following KIMIN might be KAL or DIRI.

(19′) DAL: due to the numerous possible readings of this sign the interpretation remains unclear. EDIN = *ṣēru*, 'open country.'

5 For an overview of the synodic phenomena of the planets see Ossendrijver 2012, 58.

6 Ossendrijver 2012, 36.

7 CAD, Vol. 14, R, 337b.

(20′) BAD: due to the numerous possible readings the interpretation remains unclear. SUKUD = *mēlû*, 'height.'

*Column ii*

(6′) DU: this is the first of several instances where DU appears to signify the subtractive nature of the correction to the date, as opposed to DIRI ('to exceed'), which signifies an additive correction (see rev. ii 8′, 14′, 16′). A very preliminary, pragmatic translation 'deficit' has been adopted for all of these instances. The Akkadian reading of DU (or GUB) is not clear – perhaps a form of *alāku*, 'to go,' or *izuzzu*, 'to stand.' This usage of DU is not attested elsewhere in the astronomical corpus as far as known. A subtractive number of days to be applied to the date of an astronomical phenomenon is usually marked by $LA_2$ = *maṭû*, 'to be lacking' (thus in BM 41004 rev.) or its D stem, *muṭṭû*, 'to diminish' (thus in BM 45728 rev. 6).

(7′) 49: the old version of the numeral 9 is used here. UD: reading uncertain; everywhere else in the text 'day' is written $u_4$-*mu* or ME.

(8′) DIRI: probably to be read itter, 3 m. sg. pres. of (*w*)*atāru* G, 'exceeds,' or the cognate noun *atartu*, 'excess.'

(9′) ⸢6?⸣: the upper three wedges are preserved, which implies a number between 4 and 9 (old version).

(17′) 1 2 ME 30: the correct reading of these signs is not clear. The initial 1 might also be read ana, 'to,' and ME might be *ūmu*, 'day,' or ME, 'hundred.' None of these options appears to yield a plausible interpretation.

(18′) ⸢*šamaš*$_2$(20) *ana* AN⸣: only the upper parts are preserved.

(22′) ⸢*sin*? *ana* $GU_4$.UD?⸣: *sin*(30) might also be *šamaš*$_2$(20).

*Column iii*

(1′) x: perhaps *ina*, 'in.'

(3′) UDU.IDIM: either the word 'planet' or the determinative preceding the name of a planet. UDU.IDIM is followed by two damaged winkelhakens, perhaps part of $MUL_2$.BABBAR, 'Jupiter,' or a number 20 or 30. Note however that *šamaš*$_2$(20), 'Sun' and *sin*(30), 'Moon,' are never preceded by the determinative for planet.

## 3 Commentary

The preserved portions of the text can be divided into four distinct parts, here labeled I (§§1–5), II (§§6′–9′), III (§§10′–14′) and IV (§§15′–26′). Taken together they constitute some sort of compendium about planetary phenomena. The colophon may hint at this, since it contains the word or determinative 'planet' (rev. iii 3′), but this may also be the catchline of another, related tablet. The different parts were compiled without much effort to produce a consistent terminology. For instance, Mars is called AN as well as $^{d}$*ṣal-bat-a-nu*, and Jupiter is called $^{d}$SAG.ME.GAR as well as $MUL_2$.BABBAR. Generally speaking, the terminology of the text is very similar to that of the Late Babylonian non-mathematical astronomical and astrological procedures. In particular one may mention BM 41004 and BM 45728,[8] two compendia about planetary phenomena from Babylon, and TU 11,[9] a compendium from Seleucid Uruk with Goal-Year procedures mainly concerned with the Moon. Throughout this paper, several references will be made to these compendia in order to interpret certain passages. Certain isolated passages from the text are known from other Late Babylonian astronomical tablets (see below). However, no duplicates of the text, or of any of its parts, have been identified. In particular, the reverse deals mainly with planetary conjunctions, a topic that is not addressed in any previously published Babylonian astronomical procedure text.[10]

Part I occupies the entire obverse of the fragment. Unlike the reverse, this side of the fragment is not subdivided into columns. Due to the strong erosion, not much can be read in part I. It appears that all four partly preserved sections are concerned with the prediction of the synodic phenomena of the planets, i.e. their first and last appearances, stations, and oppositions. One or more instances of the word 'prediction' (obv. 8–13), a period expressed in years (obv. 2, 3), a number of days that is 'lacking' (obv. 4) and the instruction '(you add) to your year' (obv. 20) are clear references to the so-called Goal-Year method. This method is based on the empirical fact that many of the planetary and lunar phenomena that were observed by Babylonian astronomers repeat in a future year – the Goal Year – near the same celestial position and calendar date as in the year that precedes it by a characteristic period which is different for each planet.[11] In addition to the periods, which are expressed in years, the Goal-Year method involves small additive or subtractive corrections to the dates, which are expressed in days. Apart from synodic phenomena, the Goal-Year method was also used for predicting when a planet would pass by one of the so-called Normal Stars, a group of reference stars. For some planets

8 BM 41004: Brack-Bernsen and Hunger 2005/2006; BM 45728: Britton 2002.

9 Brack-Bernsen and Hunger 2002.

10 However, a tablet from Babylon (Hunger, Sachs, and Steele 2001, No. 58) with reports of conjunctions between the Moon and Mars and between the Moon and Saturn for the period 423–400 BCE confirms that Babylonian astronomers were collecting empirical data on planetary conjunctions.

11 For the Goal-Year method cf. Brack-Bernsen and Hunger 2002; Hunger and Sachs 2006; Gray and Steele 2008; Gray and Steele 2009; Steele 2011.

a different period was used for these star passages. Furthermore, even if the same basic period was used for both types of phenomena, the corrections to be applied to the date are usually different. Also note that the Goal Year periods and the corrections are to be understood in relation to the Babylonian luni-solar calendar. That is, the whole number of years by which a Goal Year period was labeled by the Babylonian astronomers is always to be understood as a shorthand for a whole number of calendar months, and the correction in days is to be added to or subtracted from that number of months.

As in the Goal Year procedures BM 41004 and BM 45728, some of these corrections appear to be mentioned in part I. Section 1 begins with instructions for Jupiter, but an additive correction of +17 days mentioned in obv. 3 is not attested for that planet.[12] However, it is consistent with the expected correction associated with the 12-year period for Jupiter's synodic phenomena (see Tab. 1). That period is not known to be a Goal Year period that was actually used, but it is in fact mentioned in obv. 3. It is also explicitly assigned to Jupiter in a Seleucid astrological procedure text.[13] The planets dealt with in §2 remain unidentified, but the following section (§3) clearly deals with Venus. In obv. 13 its standard 8-year Goal Year period is mentioned. This is followed by some instruction concerning the interval between Venus's first appearance and its last visibility as morning or evening star. Some of the corrections (2 days in obv. 10, 4 days in obv. 11) might be connected to the 8-year period (compare Tab. 1).

Parts II–IV are written on the reverse, which is divided into three columns. In both parts II and IV each procedure mentions two planets and an associated period expressed in years, sometimes also other data pertaining to these planets. Part II consists of four identically structured procedures. Each of them contains three statements, the first of which is of the type 'planet 1 to planet 2: *p* years.' It appears that in part I planet 1 is repeated in subsequent sections while planet 2 varies from section to section. All preserved values of *p* are 'pleasant' numbers, being multiples of 12. Furthermore, the values of *p* in §§7′–9′ can all interpreted as the sums of known Goal-Year periods of the involved planets. The 36 years that are assigned to the Sun and the Moon (§7′) can be interpreted as 18 + 18 years, twice the saros period, the standard Goal-Year period for lunar and solar eclipses (Tab. 1). In §8′, the 60 years that are assigned to Mercury and Saturn equal 1 + 59, where 59 years is the standard Goal-Year period for Saturn and 1 year is a valid, although unattested Goal-Year period for Mercury (Tab. 1). The 60 years assigned to Mercury and Mars in §9′ equals 13 + 47, where 47 years is the standard Goal-Year period

12 The standard Goal Year periods for Jupiter are 71 years (for synodic phenomena) and 83 years (for star passages); the associated corrections are of the order +1 d and +5 d, respectively (Gray and Steele 2008; Steele 2011).

13 TU 20, rev. 2 (Hunger 1976). This period is not used as a Goal Year period. It is close to Jupiter's sidereal period (11.86 yr).

for the star passages of Mars and 13 years is a valid, but non-standard Goal-Year period for Mercury's synodic phenomena which is mentioned in BM 41004 rev. 16. In §6′ the names of the planets are not preserved. It seems likely that planet 1 is the Sun, since this is also the case in §7′. Hence 24 years might be interpreted as $18 + 6$ years. However, a Goal-Year period of 6 years is not attested, so the identification of the planets in §6′ remains unclear.

From an astronomical point of view, the Goal-Year periods of two different planets do not add up to a meaningful period for conjunctions between these planets (cf. below). Hence there must have been other considerations, presumably astrological or numerological, that motivated the pairwise addition of these periods. The fact that all values of $p$ are 'pleasant' numbers may be seen as confirmation of a numerological motivation. Furthermore, each procedure continues with two representations of $p$ as the product of two numbers, i.e. $p = q \cdot r$. No obvious astronomical significance can be attached to the values of $q$ and $r$, except in §7′, where $r = 18$ (rev. i 3) can be interpreted as the saros period.

After §9′, column i continues with part III, which consists of five short procedures (§§10′–14′) that do not appear to have much in common. Part III deviates from parts II and IV in that the procedures are not concerned with pairs of planets, but with single planets, or other astronomical, astrological or, perhaps, lexical topics. In §10′ a period of 7 years and an additive correction of 10 days are assigned to Mercury. The formulation of this rule is entirely analogous to the Goal-Year procedures in BM 41004 (rev.) and BM 45728 (rev.). Each procedure mentions a period measured in years and a correction expressed in days. As mentioned, they are to be understood in relation to the Babylonian luni-solar calendar. That is, the 7 years actually stands for 86 months, the closest whole number of months corresponding to 7 years, and 10 days is the correction that must be added to this number of months. This is a valid Goal-Year rule for Mercury (Tab. 1), which is not attested elsewhere as far as known. The correction of $+10$ days is close to the value of $+9$ days obtained from a modern computation.

The meaning of §11′ is largely unclear. The terms 'rain and flood' are often mentioned together in astronomical diaries and in certain astrological texts concerned with weather prediction.[14] The significance of the numbers in rev. i 13′–14′ is also not clear; perhaps they represent a period for these phenomena.

In §12′ we are again on solid ground, since this procedure mentions the well-known interval of approximately 11 days by which the solar year exceeds 12 synodic months. This parameter, known by the modern term yearly epact, is mentioned or implied in numerous Babylonian astronomical texts. In an ordinary year of twelve months, i.e.

14 See Hunger 1976; Sachs and Hunger 1988; Brack-Bernsen and Hunger 2002.

| planet | phenomena | nr. of elementary periods | years | months (m) +days (d) | BM 76488 | |
|---|---|---|---|---|---|---|
| Moon, Sun | synodic | 223 $P_{syn}$ | 18.030 | 223 m | 18 yr | §7′ |
| | synodic | 669 $P_{syn}$ | 54.089 | 669 m | 54 yr | §17′ |
| Mercury | synodic | 3 $P_{syn}$ | 0.9515 | 12 m – 7 d | 1 yr | §8′ |
| | synodic | 22 $P_{syn}$ | 6.9780 | 86 m + 9 d | 7 yr +10 d | §10′ |
| | synodic | 41 $P_{syn}$ | 13.005 | 161 m – 5 d | 13 yr | §9′ |
| | synodic | 164 $P_{syn}$ | 52.018 | 643 m +11 d | 52 yr | §24′ |
| Venus | synodic | 5 $P_{syn}$ | 7.993 | 99 m – 5 d | 8 yr [… d] | §3 |
| | star passages | 13 $P_{sid}$ | 7.997 | 99 m – 2 d | 8 yr [… d] | §3 |
| | synodic | 20 $P_{syn}$ | 31.973 | 395 m +13 d | 32 [yr] | §20′ |
| | synodic | 30 $P_{syn}$ | 47.960 | 593 m +10 d | 48 [yr] | §20′? |
| | synodic | 75 $P_{syn}$ | 119.899 | 1484 m –29 d | 120 yr | §19′ |
| Mars | star passages | 25 $P_{sid}$ | 47.020 | 581 m +17 d | 47 yr […d] | §9′ |
| Jupiter | synodic | 11 $P_{syn}$ | 12.013 | 148 m +17 d | 12 yr +17 d? | §1 |
| Saturn | synodic | 57 $P_{syn}$ | 59.003 | 730 m – 6 d | 59 yr – 6 d | §8′, §19′ |
| | synodic | 86 $P_{syn}$ | 89.022 | 1101 m – 1 d | 89 yr [… d] | §16′ |
| | synodic | 142 $P_{syn}$ | 146.990 | 1118 m + 2 d | 144 yr (error for 147 yr?) | §21′? |

**Tab. 1** Goal-Year type periods for synodic phenomena and Normal Star passages: modern data and BM 76488.

a year without intercalation, the dates of all stellar phenomena, i.e. heliacal risings ('appearances') and settings ('disappearances'), are shifted by this amount. Hence §12′ is probably concerned with stellar, not planetary phenomena.

Section 13′ is difficult to interpret and the correct reading of some signs could not be established. It seems to be concerned with a period, a correction expressed in days, the Sun and certain planetary phenomena, including retrograde motion and stations (rev. i 18′). Section 14′ contains two short, numbered statements that appear to be lexical glosses. Their meaning remains opaque and it is not clear to which of the preceding statements, if any, they are connected.

After a break of unknown length column ii continues with part IV which contains at least ten sections, each concerned with a pair of planets (§§15′–26′). The sequence of the pairs of planets is different from part II, since planet 2 is repeated in subsequent sections while planet 1 varies from section to section. Each pair of planets occurs only once. They may be divided into five distinct types: (1) conjunctions with the Moon (§§17′, 25′); (2) conjunctions with the Sun (§23′); (3) conjunctions between two inner planets (Mercury and Venus) (§18′), (4) conjunctions between an inner planet and an outer planet (Mars, Jupiter, or Saturn) (§§15′, 19′, 20′, 22′, 24′); (5) conjunctions between two outer planets (§§16′, 21′). In every section, the statement 'planet 1 to planet 2' is followed by a period measured in years and a correction for the date analogous to the one in §10′. With some exceptions, these periods are not attested elsewhere, as far as known. Unlike the periods from part II, they may have been derived from astronomical observations. At least some of them are empirically meaningful values of the mean time between one or more *clusters* of conjunctions of the involved planets; for a modern derivation see Appendix A, i.e. Section 4 of this article. Note that for conjunctions of type 4 the mean periods for conjunctions are expected to coincide with Goal-Year type periods of the outer planet, for type 2 with those of the planet. The most interesting periods are therefore those for conjunctions of types 1, 3, and 5, since they should differ from the Goal-Year periods for individual planets. Sometimes one or two additional periods are mentioned after the period for conjunctions. Some of these other periods are identifiable as Goal-Year periods for one of the involved planets – usually the planet that is mentioned in first position. Other aspects of the procedures in parts IV still defy interpretation.

In §§15′, 16′, and 17′, planet 2 is Jupiter, while planet 1 is successively equal to Venus, Saturn, and the Moon. In §15′ the period is not preserved. In §16′ two numbers are partly preserved, but the units are not. However, the common structure underlying each of the procedures §§15′–25′ suggests that the first number, 20, is the period measured in years, while the second one, probably 50, is the correction expressed in days. A period of 20 years is not attested elsewhere in the cuneiform literature in connection with Saturn or Jupiter. It cannot be interpreted as a sum of Goal-Year periods for these planets as was done in §§6′–9′. However, 20 years is a correct mean period for successive conjunctions between these planets (Tab. 2). It is in fact the shortest possible period for conjunctions between these planets, comprising one elementary period ($P_{co}$ in Tab. 3). As shown in Tab. 2, it can be expressed as 247 months, the closest whole number of months corresponding to 20 years, and a subtractive correction of 41 days. This suggests that the damaged number 50 (rev. ii 2′) was followed by a subtractive marker, probably DU, because that logogram appears to be used in this function throughout §§15′–25′ (see the philological remarks). Rev. ii 3′ mentions a period of 89 years, but the correction in days

| pair of planets | nr. of (clusters of) conjunctions | years | months, days | shift in longitude | BM 76488 | |
|---|---|---|---|---|---|---|
| Venus – Jupiter | 11 | 12.013 | 148 m +17 d | +5° | [...] | §15′ |
| Saturn – Jupiter | 1 | 19.858 | 247 m −41 d | −117° | 20 [yr] | §16′ |
| Moon – Jupiter | 478 | 35.982 | 445 m + 2 d | +12° | 36 [yr + ]2 d | §17′ |
| Mercury – Venus | ? | ? | ? | ? | 40 [yr] −15 d | §18′ |
| Saturn – Venus | 31 | 32.089 | 396 m +27 d | +32° | 32 [yr] +20 d | §19′ |
| Mars – Venus | 8 | 17.082 | 211 m + 8 d | +29.5° | [...] | §20′? |
| Saturn – Mars | 15 | 30.135 | 372 m +21 d | +5° | 30 [yr] + 5 d | §21′ |
| Mercury – Mars | 15 | 32.030 | 396 m + 5 d | +11° | 32 [yr] + 5 d | §22′ |
| Saturn – Mercury | 26 | 26.914 | 334 m −33 d | −31° | 27 [yr − ]5 d | §24′ |
| Moon – Mercury | 235 | 19.000 | 235 m | +0.2° | 19 yr | §25′ |

**Tab. 2** Mean time between multiple conjunctions: modern data and BM 76488.

is missing. This is a valid Goal-Year period for Saturn that can be construed as 30 + 59 years, the sum of two Goal-Year periods for this planet, both of which are mentioned in BM 41004 rev. 13–14, while 59 years is also mentioned in BM 45728 rev. 13.

Returning to §15′, it can be assumed that the missing period is some multiple of the mean period for successive clusters of conjunctions between Venus and Jupiter ($P_{co}$ in Tab. 3). This multiple was probably chosen in such a way that a close return to the same date and celestial position is achieved. We cannot be sure which period is to be restored, but a plausible one would be 12 years (see Tab. 2).

In §17′ planet 1 is the Moon. The period of 36 years is a correct value for the mean duration of 478 conjunctions between the Moon and Jupiter. This multiple may have been selected because it yields a very close return of the date as well as the celestial position (Tab. 2). The second period, 54 years (rev. ii 5′), is not followed by a correction for the date. It corresponds to another well-known Goal-Year period for the Moon, namely 669 months = 3 saros periods. Since a synodic period for the Moon always consists of a whole number of months, the absence of a correction expressed in days is expected.

In §§18′–20′ planet 2 is Venus, while planet 1 is set to Mercury, Saturn, and Mars, respectively. In §18′ a period of 40 years and a subtractive correction of 15 days follows the statement 'Mercury to Venus'. This period is not attested elsewhere. Its origin and justification remain unclear for the moment. The next line contains several numerals and, perhaps, the word 'day'. The interpretation remains unclear. The interval of 49 days could not be identified.

In §19′ a period of 32 years and an additive correction of 20 days are assigned to conjunctions between Saturn and Venus. This correction agrees quite well with the expected value (Tab. 2). The 32-year period does not produce a particularly close return to the same date and ecliptical longitude, but neither does any shorter period. A period of 10 conjunctions = 29.36 years does produce a much closer return of the ecliptical longitude (shift: $-1°$), but the remainder of 0.36 years yields a very large correction for the dates of about +4.5 months. The next two lines mention two further periods, the first of which, 59 years, is the standard Goal-Year period for Saturn (Tab. 1). The associated correction of −6 days is also mentioned in BM 41001 rev. 13. The second period, 120 years (rev. ii 10′), is not attested elsewhere. It might be interpreted as a Goal-Year period for Venus (Tab. 1). The expected subtractive correction may have been written in the gap. The meaning of the other numbers is not clear.

Section 20′ deals with Mars and Venus, but the period is broken away (rev. ii 11′). A plausible period that might have been mentioned here is 17 years corresponding to 8 conjunctions (Tab. 2). The number 48 (rev. ii 12′) can be readily interpreted as a Goal-Year period of Venus (Tab. 1). The meaning of the other numbers in that line is not clear. The third and final line of §20′ mentions another valid Goal-Year period of Venus (32 years) and two damaged numbers whose significance is not clear.

In §§21′–23′ planet 2 is Mars, while planet 1 is set to Saturn, Mercury, and the Sun, respectively. In §21′ a period of 30 years is assigned to Saturn and Mars. This is a valid mean period for conjunctions between these planets (Tab. 2). In fact, it corresponds to the smallest possible multiple of the basic period for conjunctions between these planets, $P_{\mathrm{co}} = 2.0$ years, that yields a reasonably close return to the same ecliptical longitude. However, the reported correction of +5 days differs significantly from the expected value of about +21 d.[15] The significance of the number 45 is unclear. The period of 144 years mentioned in the next line (rev. ii 15′) is neither a Goal-Year period of Saturn, nor of Mars. Note however that 147 years are a valid non-standard Goal-Year period of Saturn which is attested in BM 41001, rev. 15 (see also Tab. 2).

In §22′ a period of 32 years is assigned to Mercury and Mars. This is a valid period for conjunctions between these planets. The correction of +5 days agrees with the expected value (Tab. 2). The meaning of the other statements is not clear (cf. the philological remarks).

The period of 144 years assigned to the Sun and Mars in §23′ is problematic. Since planet 1 is the Sun, the mean periods for conjunctions should equal a Goal-Year type

15 Its magnitude does agree with the expected shift along the ecliptic measured in degrees, but it seems unlikely that the correction has this deviating meaning here.

period of Mars, but 144 years is not one of them. The period of 135 years (rev. ii 19′) remains unidentified; as far as known it is not a valid Goal-Year period of any planet.

In §§24′–25′ and presumably also in the missing first section of column iii, planet 2 is Mercury. In §24′, 27 years is assigned to Saturn and Mercury, a valid mean period for conjunctions between these planets (Tab. 2). In the next line (rev. ii 21′) a period of 52 years is mentioned. This is a valid Goal-Year period for Mercury (Tab. 1). The associated correction for the date is omitted. In §25′ planet 1 is probably the Moon. The period of 19 years is a valid mean period for conjunctions between the Moon and Mercury (Tab. 2).

In part IV planet 2 was successively equal to Jupiter, Venus, Mars, and Mercury. This leaves out Saturn, the Moon and the Sun as possible candidates for planet 2 in the sections that are missing in column iii between §25′ and §26′. It can be assumed that planet 1 was chosen in such a way that no pair of planets is repeated.

## 4 Appendix A: mean periods for planetary conjunctions

Two planets are said to be in conjunction when they have the same ecliptical longitude for the observer. Since this event is affected by the varying velocities of both planets, the time between successive conjunctions is not constant. However, a mean period, say $P_{co}$, can be derived from the sidereal periods of the involved planets by assuming that they move along the ecliptic at their mean velocity:

$$P_{co} = \frac{P_1 P_2}{|P_2 - P_1|}.$$

Here $P_1$ and $P_2$ are suitably chosen sidereal periods of planets 1 and 2, respectively. For the Moon and the outer planets (Mars, Jupiter, Saturn), the actual sidereal periods are to be used here. For the inner planets (Mercury, Venus) the appropriate sidereal period is that of the Sun (1 year), i.e. the motion of these planets with respect to the mean Sun is ignored. Hence the formula does not work for conjunctions between Mercury and Venus, because the denominator vanishes in that case ($P_1 = P_2 = 1\,\text{yr}$), but cf. below. The resulting values of $P_{co}$ and the associated mean displacements along the ecliptic are compiled in Tab. 3. By computing 360°/shift one can assess how many conjunctions are needed for a close return to the same ecliptical longitude. The resulting periods, converted to mean synodic months and a correction expressed in days, may then be compared with the periods mentioned in the text (Tab. 2).

Note that the time between two actual, individual conjunctions is subject to variation and can differ significantly from the values of $P_{co}$ thus computed. Moreover, all

| | Mercury | | Venus | | Mars | | Jupiter | | Saturn | |
|---|---|---|---|---|---|---|---|---|---|---|
| | $P_{co}$ [yr] | shift | $P_{co}$ [yr] | shift | $P_{co}$ [yr] | shift | $P_{co}$ [yr] | shift | $P_{co}$ [yr] | shift |
| Moon | 0.0809 | +29.1° | 0.0809 | +29.1° | 0.0779 | +14.9° | 0.0753 | +2.28° | 0.0750 | +0.92° |
| Mercury | | | *0.90* | *–36°* | 2.1353 | +48.7° | 1.0921 | +33.1° | 1.0351 | +12.7° |
| Venus | | | | | 2.1353 | +48.7° | 1.0921 | +33.1° | 1.0351 | +12.7° |
| Mars | | | | | | | 2.2354 | +67.8° | 2.0090 | +24.6° |
| Jupiter | | | | | | | | | 19.858 | –117° |

**Tab. 3** Mean periods and mean longitudinal shifts for planetary conjunctions (modern values).

five planets (Mercury, Venus, Mars, Jupiter, and Saturn) experience retrogradations, i.e. they occasionally change their direction of motion along the ecliptic. As a result, several conjunctions may occur in rapid succession within a single interval $P_{co}$. Hence $P_{co}$ represents the mean time between successive clusters of conjunctions rather than individual conjunctions. A modern table with computed conjunctions published by Meeus reveals that up to five conjunctions may form a single cluster in the case of Mercury and Venus or Mercury and Mars.[16] For other conjunctions involving Mercury, Venus, Mars, Jupiter or Saturn up to three conjunctions may form a single cluster. The only exception is the Moon, which moves much more rapidly than the planets, so that it never experiences more than one conjunction within the interval $P_{co}$. The correctness of the results for $P_{co}$ in Tab. 3 is confirmed by the data in the tables of Meeus.[17] As mentioned, the approach followed here does not work for conjunctions between Mercury and Venus. The tables of Meeus reveal that the mean time between successive clusters of conjunctions between these planets is 0.90 yr. That number and the associated mean longitudinal shift are shown in italics in Tab. 3.

16 Meeus 1995, 39–46.

17 Meeus 1995, 39–46.

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## Illustration and table credits

**ILLUSTRATIONS:** **1–2** Courtesy of the Trustees of the British Museum.

**TABLES:** **1–3** Mathieu Ossendrijver.

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# TIME AND TIME MEASUREMENT

Hermann Hunger

# The So-Called Report on Seasonal Hours (K 2077+): A New Interpretation

## Summary

The tablet K 2077+ was when first published taken to be an example of the use of seasonal hours in Babylonian astronomy. Since then, it has been joined to BM 54619. This article provides a new edition and discussion of the text which can now be seen to be not describing seasonal hours but can better be understood as giving a scheme for the seasonally varying motion of the sun.

Keywords: Daylight and night; seasonal hour; motion of sun and moon; astronomical report; Assyrian royal correspondence; Assyrian astronomy.

Die Tafel K 2077+ wurde bei ihrer Erstpublikation als Beispiel für die Verwendung von Temporalstunden in der babylonischen Astronomie angesehen. Inzwischen wurde sie mit BM 54619 zusammengefügt. Dieser Beitrag stellt eine Neuedition und Diskussion des Textes vor, der nun weniger als Beschreibung der Temporalstunden denn als ein Schema für jahreszeitlich variierende Sonnenbewegungen angesehen werden kann.

Keywords: Dauer von Tag und Nacht; Temporalstunden; Sonnen- und Mondbewegung; astronomischer Bericht; assyrische Hofkorrespondenz; assyrische Astronomie.

I thank the Trustees of the British Museum for permission to publish BM 54619.

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# 1 Introduction

The tablet K 2077+ was published by E. Reiner and D. Pingree.[1] Since then, an additional fragment (BM 54619) was joined to it (Figs. 1, 2, 3, and 4). The outline of the tablet is now almost complete; due to extensive damage of the obverse, however, the text is still only partly understandable.

The two parts of the tablet belong today to two different collections in the British Museum, one from Nineveh, the other from Babylon. However, not infrequently tablets from other findspots have ended up in these collections. The main text is written in Babylonian script and in the Neo-Babylonian variant of Akkadian. Such a tablet could have been written in Babylonia and intended to be sent to the Assyrian king. It could also have been written by a Babylonian living in Nineveh. The colophon, however, is written in Assyrian script, which was not used after the end of the Assyrian empire. Also, the dating by eponyms is characteristic of Assyria. It is therefore very likely that the tablet was finished and found in Nineveh. The eponym, Bel-šaduʾa, had the office in 650 BC.

# 2 Transliteration[2]

## 2.1 Obverse

(1) [DIŠ] ⸢*i*⸣-*na* $^{\text{itu}}$Š[U UD-15-KAM $^{\text{d}}$UTU *i-na šu-ut* $^{\text{d}}$*En-líl i-na* MURU]B$_4^?$ $^{\text{mul}}$AL-LUL GUB-*az ù* $^{\text{d}}$*Sin*

(2) [*i*]-*na šu-ut* ⸢$^{\text{d}}$⸣[*É-a i-na* ....] ⸢x⸣ $^{\text{mul}}$SUḪUR-MÁŠ$^{\text{ku}}_6$ GUB-*ma* 8 DANNA $u_4$-*mu* 4 DANNA GE$_6$

---

(3) UD-15-KAM $^{\text{d}}$UTU 1 UŠ 20 NINDA *šá* $u_4$-*mu* GIN-*ak-m*[*a*] ⸢2/3⸣ DANNA *qaq-qa-ru* 10 UŠ $u_4$-*mu ik-te-ri*

(4) *šá-ni-ti* UD-15-KAM 2/3 DANNA 50 NINDA LAL-*ṭi* GIN-*ak šá-lul-tú* UD-15-KAM 18 UŠ 20 NINDA GIN-*ak*

(5) 3 UD-15-KAM$^{\text{me}}$ GIN-*ak-ma* 1 DANNA $u_4$-*mu i-ker-ri šá* 3 UD-15-KAM$^{\text{meš}}$ 1 2/3 DANNA ⸢7⸣ [UŠ 30 NINDA] ⸢*a*⸣-*lak šá* $^{\text{d}}$UTU

(6) 12-*ú šá* $u_4$-*mu* 15 UD$^{\text{meš}}$ GIN-*ak* a ⸢x x⸣ [x x x (x)] ⸢x *i*$^?$ x -*ru*$^?$⸣-*ú* 12-*ú* ⸢10$^?$⸣ [x x x] LAL-*ṭi*

---

1 Pingree and Reiner 1974/1977.

2 [....] stands for a break of unspecified length; where the size of a break can be estimated, x stands for one missing sign.

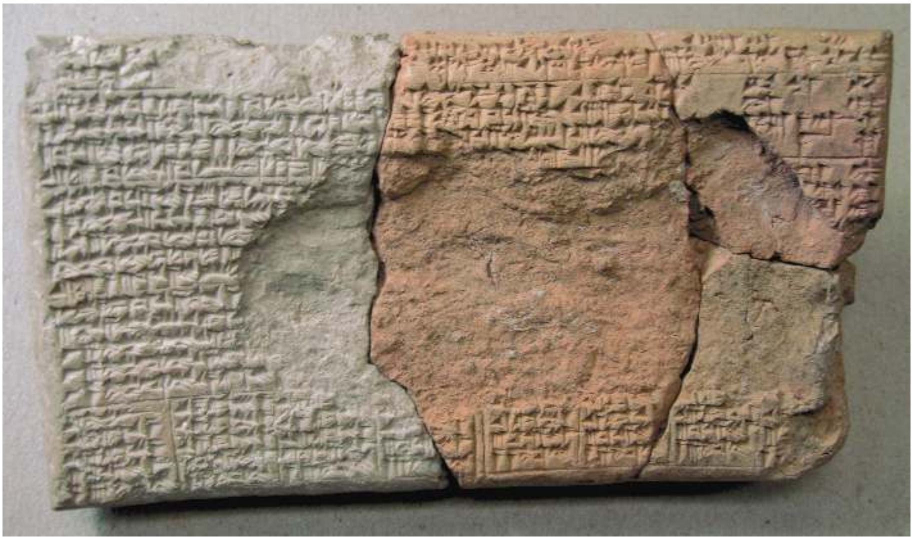

**Fig. 1** K. 2077 + 3771 + 11044 + BM 54619 Obverse.

**Fig. 2** K. 2077 + 3771 + 11044 + BM 54619 Obverse, left side.

(7) *ul-tu* UD-15-KAM *šá* $^{\text{itu}}$ŠU EN UD-⸢30⸣-[KAM *šá* $^{\text{itu}}$IZI 1 DANNA *u*$_4$*-mu* LUGUD-DA-*ma a-na* 7 DANNA *u*$_4$*-mu*] GUR-*ár*

(8) *šá* 7 DANNA 12-*šú* 17 UŠ 30 NINDA ⸢x⸣ [.... 1 DANNA *u*$_4$*-mu* LUG]UD-DA-*ma*

(9) *a-na* 6 DANNA *u*$_4$*-mu* GUR-*ár* ⸢*šá*⸣ [....] $^{\text{itu}}$APIN

(10) 1 DANNA *u*$_4$*-mu* LUGUD-DA-*ma a*-[*na* 5 DANNA *u*$_4$*-mu* GUR-*ár* ....] ⸢x x⸣ [x]

(11) 12-*ú*$^?$ *šá* 3 UD-15-KAM$^{\text{meš}}$ ⸢x⸣ [....]

---

(12) DIŠ *ina* $^{\text{itu}}$AB UD-15-KAM 4 [DANNA *u*$_4$*-mu* 8 DANNA GE$_6$ ....]

(13) 3 UD-15-KAM$^{\text{meš}}$ 10 UŠ ⸢x⸣ [....]

(14) *ina* 5 DANNA *u*$_4$*-mu* 12 U[Š 30 NINDA ....]

(15) *ina* 6 DANNA *u*$_4$*-mu* 15 UŠ [....]

(16) *ina* ⸢7⸣ DANNA *u*$_4$*-mu* 17 UŠ 30 NIN[DA ....]

| | | | | |
|---|---|---|---|---|
| (17) | 2/3 DANNA | 17 UŠ 30 NINDA | 15 UŠ | [12 UŠ 30 NINDA |
| (18) | 19 UŠ 10 NINDA | 16 UŠ 40 NINDA | 14 UŠ ⸢10⸣ NINDA | 1[1 UŠ 40 NI]NDA |
| (19) | 18 UŠ 20 NINDA | 15 UŠ 20[3] NINDA | 13 UŠ 20 NINDA | 10 [UŠ] ⸢50 NINDA⸣ |
| (20) | 3 UD-15-KAM$^{\text{me}}$ | 3 UD-15-KAM$^{\text{me}}$ | 3 UD-15-KAM$^{\text{me}}$ | 3 UD-15-KAM$^{\text{me}}$ |
| (21) | *šá* 8 KAS *u*$_4$*-mu* | *šá* 7 KAS *u*$_4$*-mu* | *šá* 6 KAS *u*$_4$*-mu* | *šá* 5 KAS *u*$_4$*-mu* |

*(lines 17 to 21 continued)*

| | | | | |
|---|---|---|---|---|
| (17) | 10 UŠ | 12 UŠ 30 NINDA | 15 UŠ | 17 UŠ 30 NINDA] |
| (18) | ⸢10 UŠ 50 NINDA⸣ | ⸢13 UŠ 20⸣[NINDA] | ⸢15 UŠ⸣[50 NINDA] | 18 UŠ [20 NINDA] |
| (19) | 11 UŠ 40 NINDA | 14 UŠ 10 NINDA | 16 UŠ 40 NINDA | 19 UŠ [10 NINDA] |
| (20) | 3 UD-15]-KAM$^{\text{me}}$ | 3 UD-15-KAM$^{\text{me}}$ | 3 UD-15-KAM$^{\text{me}}$ | 3 U[D-15-KAM$^{\text{me}}$] |
| (21) | *šá* 4 KAS *u*$_4$*-mu* | *šá* 5 KAS *u*$_4$*-mu* | *šá* 6 KAS *u*$_4$*-mu* | *šá* 7 KAS *u*$_4$*-mu* |

3 (Obv. 19) The same error occurs in rev. right col. 20 in the same number.

## 2.2 Reverse

### 2.2.1 *Right column*

(1) *an-nu-ú tal-lak-tú šá* $^{d}$UTU TA KASKAL$^{II}$ *šu-ut* $^{d}$*En-l*[*íl*]

(2) EN KASKAL$^{II}$ *šu-ut* $^{d}$*É-a* TA KASKAL$^{II}$ *šu-ut* $^{d}$*É-a*

(3) EN KASKAL$^{II}$ *šu-ut* $^{d}$*En-líl* TA $^{d}$UTU-È EN $^{d}$UTU-ŠÚ-A

(4) TA $^{d}$UTU-ŠÚ-A EN $^{d}$UTU-È 12 DANNA *qaq-qar mi-šiḫ-ti a-šar-ri*

(5) *ki-ṣip-ta-šú šá-lim-ti áš-ṭur qaq-qar ul ma-al-la a-ḫa-meš šú-ú*

(6) *ut-ru ù muṭ-ṭi-e li-ik-ṣi-pu-ma liq-bu-nim-ma*

(7) *ina pi-i lu-še-eš-mi* LUGAL *i-de ki-i dib-bi an-nu-tì*

(8) *ina ṭup-pi la šaṭ-ru ù ina pi-i* UN$^{me}$ *la ba-šu-ú*

(9) *ina ṭup-pi* $^{lú}$ŠAMAN-LÁ *ul i-šem-mi-i ú-*⸢x⸣ [x x]

(10) $^{lú}$SAG LUGAL *ḫat-tu-ú* ⸢*lu*⸣*-kal-li-mu* ⸢x⸣ [....]

(11) *mi-šiḫ-ti* KI$^{meš}$ *ù bi-rit* ⸢x⸣ [....]

(12) *an-na-a-ti ina pi-i lu-šá-*[....]

(13) *tal-lak-ti* $^{d}$*Sin* $^{d}$UTU $^{d}$UDU-IDIM$^{meš}$ [....]

(14) *di-ri u na-dan* GISKIM *ina lìb-bi in-na*[*m-m*]*a-ru*

| | | | | |
|---|---|---|---|---|
| (15) | 2/3 DANNA | 15 UŠ | 10 UŠ | 15 UŠ |
| (16) | 19 UŠ 10 NINDA | 14 UŠ 10 NINDA | 10 UŠ 50 NINDA | 15 UŠ 50 NINDA |
| (17) | 18 UŠ 20 NINDA | 13 UŠ 20 NINDA | 11 UŠ 40 NINDA | 16 UŠ 40 NINDA |
| (18) | 17 UŠ 30 NINDA | 12 UŠ 30 NINDA [ | 12] UŠ 30 NINDA | 17 UŠ 30 NINDA |
| (19) | 16 UŠ 40 NINDA | 11 UŠ 40 NINDA [ | 13 U]Š 20 NINDA | 18 UŠ 20 NINDA |
| (20) | 15 UŠ 20[4] NINDA | 10 UŠ 50 [NINDA] [ | 14 U]Š 10 NINDA | 19 UŠ 10 NINDA |

(21) *tal-lak-tú šá* $^{d}$UTU [x] *di-ib-bu*

### 2.2.2 *Left column*

| | | |
|---|---|---|
| (1) | 16 UŠ | 8 UŠ |
| (2) | 15 UŠ 20 NINDA | 8 UŠ 40 NINDA |
| (3) | 14 UŠ 40 NINDA | 9 UŠ 20 NINDA |
| (4) | 14 UŠ | 10 UŠ |
| (5) | 13 UŠ 20 NINDA | 10 UŠ 40 NINDA |
| (6) | 12 UŠ 40 NINDA | 11 UŠ 20 NINDA |
| (7) | 12 UŠ | 12 UŠ |
| (8) | 11 UŠ 20 NINDA | 12 UŠ 40 NINDA |
| (9) | 10 UŠ 40 NINDA | 13 UŠ 20 NINDA |
| (10) | 10 UŠ | 14 UŠ |
| (11) | 9 UŠ 20 NINDA | 14 UŠ 40 NINDA |
| (12) | 8 UŠ 40 NINDA | 15 [UŠ 20 NINDA] |

(13) *an-nu-ú šá* $^{d}$*Sin* DIB-D[IB x x]

(14) EN-NUN *u šit-ti lu-*[x x]

(15) *lu-še-eš-*[*mi*]

### 2.2.3 *Upper edge (in Neo-Assyrian script)*

(1) [.... $^{I}$x]-$^{d}$Gu-la $^{lú}$A-ZU

(2) [$^{itu}$x U]D-1[5]-KAM [*li*]*m-mu* $^{Id}$EN-KUR-*u-a*

4 (Rev. right col. 20) The same error occurs in obv. 19 in the same number.

5 (Upper edge, 2) Or: [UD x] + 2.

**Fig. 3** K. 2077 + 3771 + 11044 + BM 54619 Reverse.

**Fig. 4** K. 2077 + 3771 + 11044 + BM 54619 Reverse, left side.

## 3 Translation

### 3.1 Obverse

(1) [¶] In month IV, [on the 15th day .... the sun] stands in [the (stars) of Enlil in the midd]le$^{?}$ of Cancer, and the (full) moon

(2) stands in the (stars) of [Ea in ....] Capricorn, and there are 8 *bēru* day, 4 *bēru* night.

---

(3) (In) 15 days, the sun goes 1 UŠ 20 NINDA (= 1° 20′) per day, and 2/3 *bēru* (= 20°) is the *qaqqaru*. 10 UŠ the day became shorter.

(4) The second 15-day (period), it goes 2/3 *bēru* less 50 NINDA (= 19° 10′). The third 15-day (period), it goes 18 UŠ 20 NINDA.

(5) 3 15-day (periods) it goes, and the day becomes shorter (by) 1 *bēru*. Of 3 15-day (periods), 1 2/3 *bēru* 7 [UŠ 30 NINDA] (= 57° 30′) is the going of the sun.

(6) One-twelfth of a day it goes (in) 15 days .... [....] .... one-twelfth [....] becomes less.

---

(7) From the 15th day of month IV until the 3[0th day of month V the day becomes shorter (by) 1 *bēru*, and the day] returns [to 7 *bēru*.]

(8) Of 7 *bēru*, one-twelfth (is) 17 UŠ 30 NINDA [.... the day] becomes shorter [(by) 1 *bēru*,] and

(9) the day returns to 6 *bēru* .... [....] month VIII

(10) the day becomes shorter (by) 1 *bēru*, and [the day returns to 5 *bēru* ....] .... [....]

(11) One-twelfth$^{?}$ of 3 15-day (periods) .... [....]

---

(12) ¶ In month X, the 15th day, 4 [*bēru* day, 8 *bēru* night ....]

(13) 3 15-day (periods) 10 UŠ .... [....]

(14) At 5 *bēru* day, 12 UŠ [30 NINDA ....]

(15) At 6 *bēru* day, 15 UŠ [....]

(16) At 7 *bēru* day, 17 UŠ 30 NINDA [....]

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| (17) | 20° | 17°30′ | 15° | 12°30′ | 10° | 12°30′ | 15° | 17°30 |
| (18) | 19°10′ | 16°40′ | 14°10′ | 11°40′ | 10°50′ | 13°20′ | 15°50′ | 18°20′ |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| (19) | 18°20′ | 15°50′! | 13°20′ | 10°50′ | 11°40′ | 14°10′ | 16°40′ | 19°10′ |
| (20) | 3 15-days | 3 15-days | 3 15-days | 3 15-days | 3 15-days | 3 15-days | 3 15-days | 3 15-days |
| (21) | of 8 *bēru* day | of 7 *bēru* day | of 6 *bēru* day | of 5 *bēru* day | of 4 *bēru* day | of 5 *bēru* day | of 6 *bēru* day | of 7 *bēru* day |

## 3.2 Reverse

### *3.2.1 Right column*

(1) This is the course of the sun from the path of Enlil

(2) to the path of Ea, from the path of Ea

(3) to the path of Enlil. From sunrise to sunset,

(4) from sunset to sunrise 12 *bēru qaqqaru* is the measurement of the places[?].

(5) I wrote down its complete computation. The *qaqqaru* is not equal to each other.

(6) Let them compute the excess and the deficiencies, and let them tell me, and

(7) I will let it be heard. The king knows that these words

(8) are not written on a tablet and do not exist in the mouth of people;

(9) the apprentice scribe does not hear (them) from a tablet .... [....]

(10) I will show (it[?]) the Hittite[?] *ša rēš šarri*-official [....]

(11) the measurement of the places[?] and the interval [....]

(12) these I will [....] by mouth[?] [....]

(13) the course of moon, sun, planets [....]

(14) intercalations and giving of signs will be seen in it.

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| (15) | 20° | | 15° | | 10° | | 15° | |
| (16) | 19° | 10′ | 14° | 10′ | 10° | 50′ | 15° | 50′ |
| (17) | 18° | 20′ | 13° | 20′ | 11° | 40′ | 16° | 40′ |
| (18) | 17° | 30′ | 12° | 30′ | 12° | 30′ | 17° | 30′ |

| | | | | |
|---|---|---|---|---|
| (19) | 16° 40′ | 11° 40′ | 13° 20′ | 18° 20′ |
| (20) | 15° 50′! | 10° 50′ | 14° 10′ | 19° 10′ |

(21) The course which the Sun [...]....

### 3.2.2 *Left column*

| | | |
|---|---|---|
| (1) | 16° | 8° |
| (2) | 15° 20′ | 8° 40′ |
| (3) | 14° 40′ | 9° 20′ |
| (4) | 14° | 10° |
| (5) | 13° 20′ | 10° 40′ |
| (6) | 12° 40′ | 11° 20′ |
| (7) | 12° | 12° |
| (8) | 11° 20′ | 12° 40′ |
| (9) | 10° 40′ | 13° 20′ |
| (10) | 10° | 14° |
| (11) | 9° 20′ | 14° 40′ |
| (12) | 8° 40′ | 15° 20′ |

(13) This (is) what the moon pa[sses?]

(14) Watch and sleep? let me [....]

(15) I will let [hear?.]

### 3.2.3 *Upper edge (across both columns)*

(1) [....]-Gula, the scribe.

(2) [Month ....,] 1st? day, eponym Bel-šaduʾa.

## 4 Philological notes

### 4.1 Obverse

(1)
(2) } restored in analogy to statements in $^{mul}$Apin.

(3) UD-15-KAM is to be read *šapattu*, as can be seen from the fem. adjectives in line 4.

(6) seems to say that the sun moves 1/12th of a day in 15 days.

### 4.2 Reverse right column

(4) *a-šar-ri* seems to be an unknown word. Its measurement (*mišiḫtu*) is 12 *bēr qaqqaru*, i.e. a full circle – which is approximate for the distance from one sunrise to the next. *mišiḫtu* occurs again several lines later, followed by KI$^{meš}$. Since KI$^{meš}$ can be read *ašrī*, I propose to see in *a-šar-ri* an unconventional writing for *ašrī* (a similarly strange writing is found in *ma-al-la* for *mala* in line 5). KI is the usual term for 'place, position' in astronomical texts; it can even be translated as 'longitude' in a technical context. While *qaqqaru* is the more frequent reading of KI in late texts, *ašru* is not excluded here.

(8) *la ba-šu-ú*: *ba* here is clearly different from *ma* (e.g., in line 6) and *bašû* makes sense: the theory proposed by the writer cannot be found anywhere else.

(10) like Reiner and Pingree, I cannot explain the presence of a Hittite official here. PA-*tu-ú* yields no better meaning.

(21) This line is still in Babylonian script (see the sign *šá*), and it begins flush with the table preceding it. It is therefore not to be connected to the following two lines on the edge which are in Assyrian script (see the sign LÚ). [x]-*di-ib-bu* could be a verb in a relative clause: which the sun .... Unfortunately, I cannot find a meaningful restoration.

### 4.3 Reverse left column

(13)
(14) } maybe 'watch' is here in contrast to 'sleep' because knowing how long one has to wait for the moon means knowing when one can go to sleep.

### 4.4 Upper edge

A.ZU can be a logogram for 'scribe'; on the other hand, names containing Gula are likely to be those of physicians so that the more common meaning 'physician' may be intended here. Since the colophon is in Neo-Assyrian script, it may not contain the name of the writer of the main text but rather its owner. The name Arad-Gula in ABL 1109 r. 6, mentioned by Reiner, is to be read Arda-Mullissi according to collation in SAA 10, 113 r. 5.

## 5 Discussion

As was recognized by Pingree, the numbers in the tables at the end of obverse and reverse would be the lengths of seasonal hours measured in UŠ. This implies that UŠ is intended to be a measure of time. If we had only the table, this would be a convincing explanation, especially since there is another text apparently giving seasonal hours, the Ivory Prism BM 123340 in the British Museum.[6] However, there are problems with the statements in the second section of the obverse (which were not known to Pingree). Here the sun is explicitly said to move (GIN-*ak* etc.). The section starts with the daily movement of the sun at the time of the summer solstice, which is given as 1 UŠ 20 NINDA. Line 3 further states that the amount by which the sun moves in the 15-day period following the summer solstice is 2/3 *bēr qaqqaru*. *qaqqaru* cannot be explained as a time measurement because it would be superfluous. Also, all other passages with *bēr qaqqaru* in other texts refer to distances, not to time spans.[7] The following lines make a clear distinction between time and distance, in spite of using the same units UŠ and NINDA (UŠ and *bēru* are originally length measures which are also used for time).

2/3 *bēru* time is equivalent to 80 of our minutes, which is the length of one seasonal hour at summer solstice, under the assumption of a ratio of 2 : 1 of the longest to the shortest daylight. But 2/3 *bēru* is also, according to our text, the distance traveled by the sun in the 15 days after summer solstice. In line 6, this numerical equivalence is even explicitly stated: one-twelfth of a day (the sun) goes (in) a 15-day period. Unfortunately, the rest of the line is too broken to be understood. In line 8 one-twelfth of the duration of daylight, 7 *bēru*, is again taken to be the distance traveled by the sun; here too the rest of the line is unfortunately missing. It should however be remembered that at the beginning of the description of the sun's course in line 3 (see above) a daily movement of 1 UŠ 20 NINDA, which is obviously one-fifteenth of 2/3 *bēru*, is attributed to the sun. This is not related to a time interval.

6 Hunger and Pingree 1999, 112–115 (with earlier literature).

7 CAD, Vol. 13, Q, 117a–119a, s.v. *qaqqaru* A mng. 3; CAD, Vol. 2, B, 209b–210a, s.v. *bēru* A s. mng. 1b.

The movement ('going') of the sun cannot be an amount of time; it has to move a certain distance. One could assume that the distance is meant which the sun moves in one-twelfth of a day (i.e. one seasonal hour). But this would be just a part of the daily movement parallel to the equator.

In the second section of the text, the movements of the sun in each of the three 15-day periods are added up to its movement in 45 days.

Following this pattern one can add all the numbers in the table, and one arrives at 360 UŠ, or one full circle. In the right column of the reverse the 'measurement' of the daily movement of the sun from sunrise to sunrise is given as 12 *bēr qaqqaru* or 360°. *qaqqaru* here is clearly a distance. This supports the interpretation of *qaqqaru* in obv. 3 that the numbers of the table are not seasonal hours but distances which the sun is assumed to move, arranged by 15-day periods.

If the text had wanted to describe seasonal hours, there would have been no need to explain the sun's movement in distance. A table similar to those in Enūma Anu Enlil XIV would have sufficed. But in this text the decrease in the duration of daylight is taken as justification for the decreasing distance which the sun supposedly moves in a 15-day period.

The first lines of the reverse can be seen as a description of the path of the sun both in the ecliptic (from the path of Enlil to the path of Ea and back) and parallel to the equator (from sunrise to sunset to sunrise). Both are circles (approximately) and therefore 360° or 12 *bēru* in length.

Under the assumptions that

1. the course of the sun in one year equals 12 *bēru*
2. the ratio of longest to shortest daylight is 2 : 1
3. the velocity of the sun varies proportionally to the duration of daylight

then the distances which the sun travels in 15-day periods must be those given in the table.

Whether this was the reasoning of the author can however be doubted.

If one expresses the velocity of the sun in a varying time unit, in our text in one-twelfth of daylight, then the distance traveled by the sun will necessarily vary with the duration of daylight. Due to the relation between the units, the numbers for one-twelfth of daylight and for the sun's progress in 15 days are the same. This need not be deliberate; the duration of daylight is given at 15-day intervals already in $^{\text{mul}}$Apin and EAE XIV. However, one-twelfth of daylight, i.e. one seasonal hour, is unusual for Mesopotamia. The question is whether it really is intended as a unit for time measurement. The text considers the result of dividing the duration of daylight by 12 as the 'going of the sun', i.e. as a distance.

One can object to this mixing of time and space measurement. I do however draw attention to the easy interchanging of months and zodiacal signs in (admittedly later) astronomical texts.

Due to damage on the obverse of the tablet, the reasoning of the author (if anything of this kind was written there) cannot be reconstructed. In my interpretation, he assumed that the sun moves in the ecliptic twice as fast at summer solstice than at winter solstice. This may seem unbelievable; but the assumption of a ratio of 2 : 1 between longest and shortest daylight is also far off the real values.

It is not surprising that the author insists that his knowledge is found nowhere else (rev. 7–9). Unfortunately, the applications of his theory for the calendar (intercalations) and for omens, to which he refers in rev. 10–14, are not clear because of breaks in the tablet.

### 5.1 Reverse left column

This contains another table,[8] this time varying between a maximum of 16 UŠ and a minimum of 8 UŠ. There are twice 12 lines, so that the difference from line to line is 2/3 UŠ or 40 NINDA. As mentioned in the subscript, this table refers to the moon. The values correspond to tables in $^{\text{mul}}$Apin (II ii 43–iii 12)[9] or tablet XIV of Enūma Anu Enlil (table D)[10] giving the interval from sunset to moonset at new moon, and from sunset to moonrise at full moon, respectively, for every month of a schematic year.

8 Already mentioned in Hunger and Pingree 1999, 115–116.

9 Hunger and Pingree 1989, 101–103.

10 Al-Rawi and George 1991/1992, 58–59.

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Elisabeth Rinner

# Ancient Greek Sundials and the Theory of Conic Sections Reconsidered


## Summary

In this article, a new aspect of a possible connection between the development of ancient Greek and Roman sundials and the history of conic sections is analyzed: On conical sundials, a conic section occurs at the edge between the plane top surface and a conical surface which is most commonly used in sundials as the shape of the dial face. Based on an analysis of 3D models of conical sundials, this paper argues that the curve is not the result of a method of shaping the conical surface, but rather the basis to do so. A method is given by which the curve can be drawn approximately by connecting points. The latter can be found using a geometrical construction. This procedure suggests that craftsmen who built sundials had basic knowledge of the geometry of cones and conic sections.


Keywords: Ancient sundial; conic section; theory of conic sections; manufacturing; history of mathematics; history of technology; 3D model.

Im Fokus steht ein neuer Aspekt der möglichen Verbindung zwischen der Entwicklung antiker griechischer und römischer Sonnenuhren und der Geschichte der Kegelschnitte: Bei konischen Sonnenuhren tritt an der Kante zwischen der ebenen Oberseite und einer kegelförmigen Fläche, der häufigsten Variante der Schattenfläche, ein Kegelschnitt auf. Basierend auf einer Analyse von 3D-Modellen konischer Sonnenuhren wird dafür argumentiert, dass die Kurve nicht das Resultat einer Methode der Ausführung der Fläche ist, sondern ihre Grundlage. Eine Methode zum approximativen Zeichnen der Kurve wird angegeben, bei der geometrisch konstruierte Punkte verbunden werden. Die Prozedur legt nahe, dass Handwerker der Sonnenuhrenherstellung Grundkenntnisse der Geometrie des Kegels und der Kegelschnitte besaßen.

Keywords: Antike Sonnenuhr; Kegelschnitt; Theorie der Kegelschnitte; Fertigungsprozess; Mathematikgeschichte; Technikgeschichte; 3D-Modell.

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## 1 Introduction

It is neither new nor ill-founded to think about the connection between the origin of ancient Greek sundials and the history of the theory of conic sections.[1] Besides the quite obvious presence of conic sections as the lines of the daily motion of the shadow of the tip of a gnomon on a plane surface that are usually represented on planar sundials for some special days in the year, such lines appear in a completely different context in one other type of ancient Greek sundials: For the largest group of sundials, the shadow receiving surface is part of the surface of a right cone whose axis directs towards the poles. Since all those sundials have a plane top surface that is parallel to the horizon, the intersection of those two surfaces is a non-circular conic section. But has this curve been recognized as such? This analysis aims to reconstruct the role of this conic section in the geometry of conic sundials and its relation to Greek theories of conic sections. Contrary to earlier approaches, this analysis is based on an evaluation of the material evidence.

## 2 The geometry of conical sundials

With the exception of a small group from the islands of Rhodes and Kos, and a handful of other objects, almost all sundials with conical shadow receiving surface share the same design. They consist of a stone block whose south facing front face is divided into two parts. Whereas the upper part is always a plane surface that is inclined relative to the plane top side, the lower part can have different layouts. In many cases there is another inclined plane surface which intersects the upper plane in a straight line parallel to the other intersections of the top, back, and bottom planes. Often, the planar part of the lower section of the front face is decorated with two lion paws on its left and right sides or stylized feet-like elements on the same position as in a sundial from Delos (Fig. 1).

1 For example, in Neugebauer 1948 and Neugebauer 1975, 857, Otto Neugebauer suggests that the early theory of conic section originated from the theory of sundials.

**Fig. 1** View on the south and west (left), and on the top side (right) of a conical sundial from Delos. Archaeological Museum Delos, Inv. B3652 (11023).

The stone block is intersected by a right cone that stands orthogonal on the upper part of the front face. On its surface, eleven hour lines – and usually three lines that show the daily motion of the sun for some days of the year – are marked. This grid of lines makes it possible to determine the time that is indicated by the shadow of the tip of the gnomon that is situated in the top surface. In order to show the right time, the upper part of the front face and the planes of the day curves have to be parallel to the equatorial plane.

Conical sundials make up the largest group (about 35%) among the preserved sundials of the Greek type.[2] The oldest sundials with a design as described above date to the beginning of the 2nd century BCE. Some conical sundials with different corpus forms are even slightly older.[3] Conical sundials therefore belong to the earliest sundials that have come down to us.

An evaluation of 3D models of sundials shows that the conical surfaces deviate only little from right cones and are indeed orthogonal to the inclined upper part of the front face.[4] Due to the relative positions of cone and block, the edge between the conical surface and the planar top surface is a conic section. On the preserved objects we can observe three different types of curves: ellipses, parabolas, and hyperbolas (Fig. 2).

An analysis of a group of conical sundials from the island of Delos has shown that there exists a single principle that can explain some key dimensions of those objects.

2 This value is based on information on the preserved objects given in reports on sundials and catalogs such as Gibbs 1976 and Schaldach 2006.

3 For example, a sundial found at Herakleia with two conical dialfaces at the south and the north facing sides dates to the 3rd century BCE (see Berlin Sundial Collaboration 2014a and Berlin Sundial Collaboration 2014b).

4 These properties of the conical sundial type have been suggested by earlier scholars such as Sharon L. Gibbs (Gibbs 1976) but have never been shown before. In many cases the analysis of the geometry of sundials is based on the assumption of these properties.

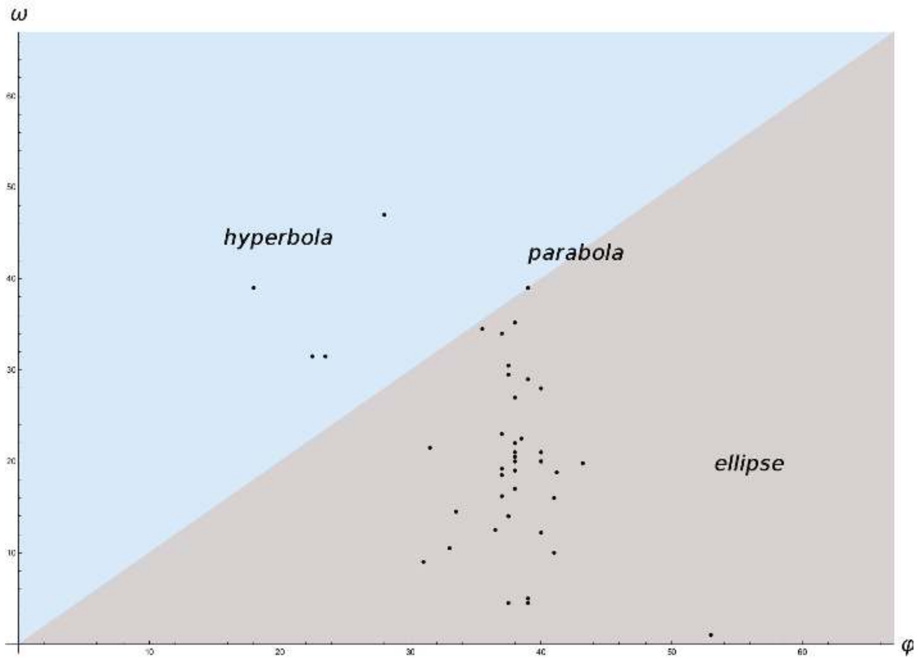


**Fig. 2** Type of conic sections at the intersection of the cone with the planar top surface. Depending on the inclination of the front face towards the vertical direction (φ) and the angle at the top opening (ω), the conic section is an ellipse, parabola or a hyperbola. The information used for the diagram is the result of a survey of literature on ancient sundials such as Gibbs 1976 and Schaldach 2006 as well as reports on ancient sundials.

Starting with the geographical latitude of the place given by the ratio of the length of a gnomon $g_0$ to the length $s_0$ of its equinoctial shadow the lengths of a set of edges can be derived by easy calculations (Fig. 3):

| | | number of modular units |
|---|---|---|
| $r$ | radius | $s_0$ |
| $g$ | | $g_0$ |
| $s$ | | $s_0$ |
| $w$ | width | $2 \cdot s_0 + 1$ |
| $h$ | height | $2 \cdot s_0 + 1$ |
| $d$ | depth | $s_0 + 3$ |
| $l$ | depth of cone opening | $s_0 + 1$ |

Besides one missing parameter and a decision about the design of the base part of the sundial, the shape of the sundial including its cone is determined uniquely by this principle.

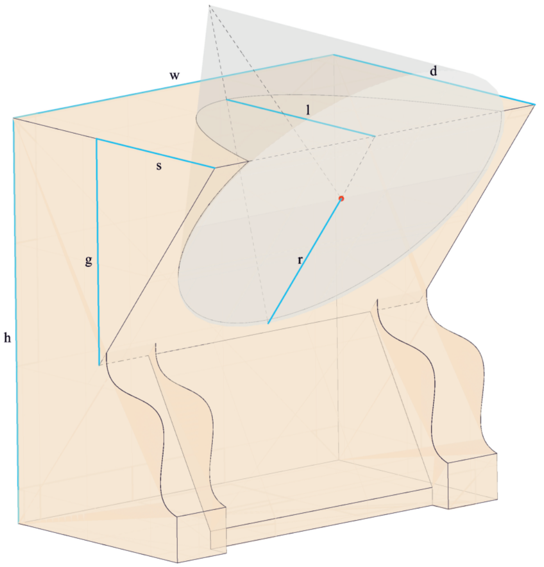


**Fig. 3** Dimensions of conical sundials that are determined by a constructional principle found for a group of sundials from Delos. *w*: width; *h*: height; *d*: depth; *r*: radius of the opening circle; *g* and *s*: sides of the orthogonal triangle given by the inclined front side; *l*: distance of the point of the conic section that is most distant to the front edge to this edge.

The principle leads – at least for the shaping of the corpus – to an easy to follow procedure that enables the stonecutter to build a sundial for a given geographical latitude without knowledge of astronomy and geography, or a deep understanding of their geometry. Traces of the use of this procedure to determine the position of the upper inclined front side can be identified on several preserved sundials from Delos and elsewhere.

## 3 Shaping the conical surface

By the principle derived from the Delian sundials, the shape of the cone is almost[5] given. We still have to identify the craftsman's method for creating the conical surface on the object. Since a large part of the circular base of the cone is given, in a first attempt one might consider a method based on one possible definition of cones: The surface of a cone is generated by the rotation of a straight line fixed at a point (the vertex of the cone) around the circle at its base. Since the cone should be a right cone, the vertex lies

5 The missing parameter determines the position of the center of the opening circle of the sundial. Due to the geometrical properties and the condition to meet the restrictions coming from the obliquity of the ecliptic, the possible variation in its position is very small.

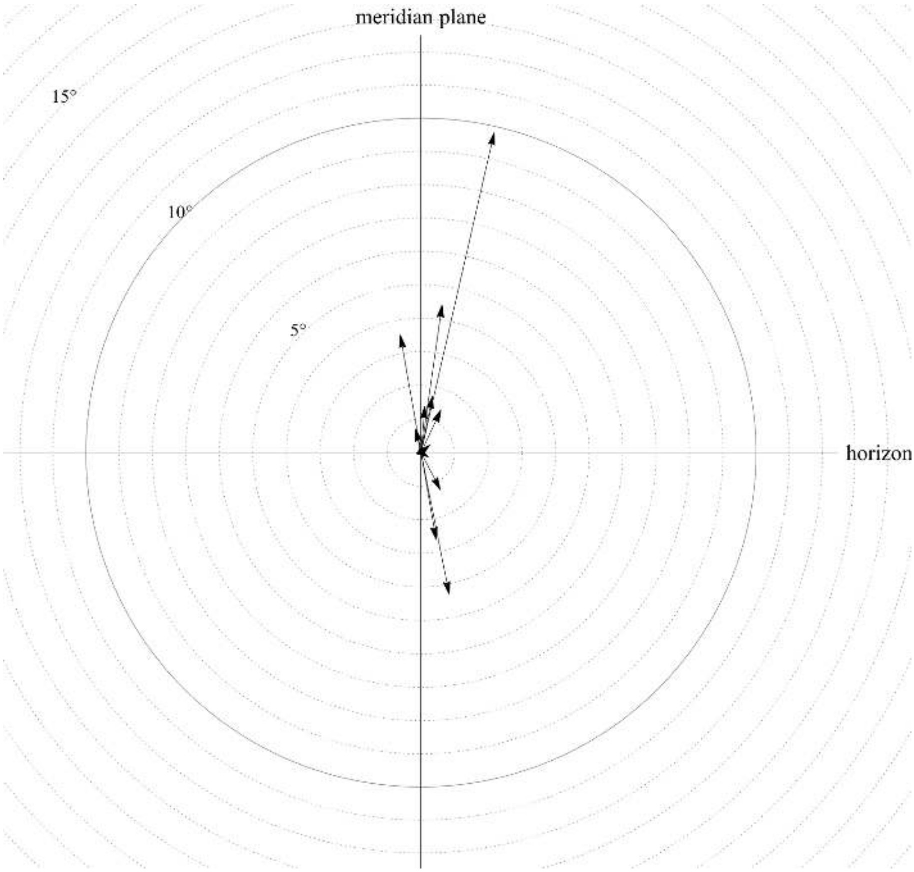


**Fig. 4** Deviations of the cone axes towards the pole direction for well preserved conical sundials, based on the existing 3D models. The distances to the center indicates the deviation in the inclination, the directions of the arrows show their directions relative to the horizon and meridian line.

on the line orthogonal to its base circle standing on its middle point. The shaping of the conical shadow receiving surface then can be controlled by the use of a ruler that is fixed on the vertex of the cone. The position of the vertex can be constructed on the basis of the dimensions given by the construction principle.

As a result of this method one would expect deviations in the fixed point of the ruler from the vertex in any direction. This would lead to deviations of the angles of the cone's axes to the upper part of the corresponding front planes from a right angle in any direction. But this is not in accordance with the material evidence: In most cases, the direction of the cone axis lies within the meridian plane (Fig. 4). So what we seek is a method that can explain this very specific error.

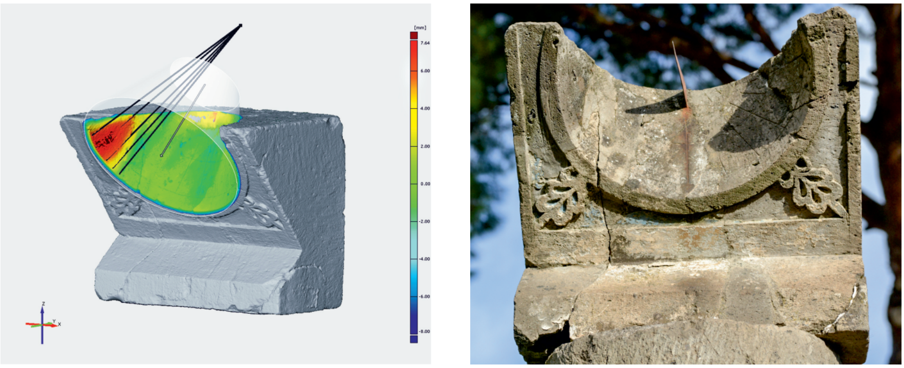

Fig. 5 Left: deviation of the shadow receiving surface towards an exact right cone in a conical sundial from the Villa San Marco, Castellamare di Stabiae, Italy, based on its 3D model (Berlin Sundial Collaboration 2015a). Right: photo of the conical sundials from Villa San Marco, Castellammare di Stabiae, Italy.

A different approach is to use both the circular line on the upper part of the front side and the line of the conic section that results from the intersection of the cone with the top plane. Again, the removal of the material can be controlled by a ruler. The surface is reached, if all material which lies between any two points of the two given lines is removed. Due to the convexity of cones no part of those lines falls outside the cone. Since the convex hull of the two curves contains the section of the cone's surface we know that the method leads to a removal of all redundant material. This method is more robust regarding deviations of the cone's axis to the west and east than to directions lying in the meridian plane. By this, we can explain the characteristics of the errors as shown in Fig. 4.

Deviations of the resulting cone then can be caused by drawing incorrect conic sections on the top surface, errors in the usage of the dimension of the distance of the deepest point of the cone to the front edge $l$, erroneous inclinations of the upper part of the front side towards the top surface, errors in the circle on the front side, or by stopping the process before the conic surface is reached.

The effect of those errors can be observed in the preserved sundials. For example, on a sundial from the Villa San Marco at Castellamare di Stabiae, the western part of the conical surface does not coincide with the circular line that is engraved on the front side (Fig. 5 right). In this area, the surface of the object deviates from the exact conic surface (Fig. 5 left). Going to the top surface, this deviation becomes smaller.

This situation can be explained by the use of the method based on the conic section on the top surface, if the process has been stopped before the conic surface has been reached. Of course it could be generated by an erroneous usage of the first method. But in this case the vertex had to be moving, the circle on the front side has not been met,

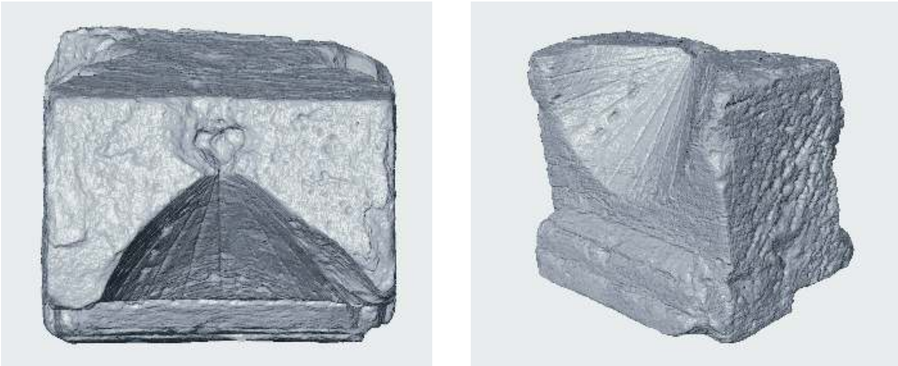

**Fig. 6** View on the top plane (left) and the south eastern edge (right) of the sundial from Naga, based on its 3D model (TrigonArt, Bauer Praus GbR 2015 [2013]).

and the procedure stopped when – by chance – the correct conic section at the top plane has been reached. This is not plausible.

In the case of the Naga sundial[6] the curve at the top plane deviates from a conic section (Fig. 6 left). Nevertheless the surface is generated by an (erroneous) sequence of straight connections of points lying on both defining curves. In the relevant part of the surface the direction of these lines is the same as in the markings that come from the carving of the surface and are preserved at the western part of the surface (Fig. 6 right). Again, we see the result of the second method, based on erroneous starting conditions.

Since the method based on the conic section can explain the very specific errors observed on the material evidence, it has to be considered as the one that has been used to build those sundials.

## 4 Greek mathematics and the method of shaping the cone

The first Greek mathematical texts dealing with the geometry of cones – especially right cones – significantly predate the first preserved sundials. About the time of the earliest preserved conical sundials Apollonius of Perga states two propositions at the very beginning of his *Conics*:

> Prop. 1: The straight lines drawn from the vertex of the conic surface to points on the surface are on that surface.

6 Kroeper and Krzyzaniak 1998.

> Prop. 2: If on either one of the two vertically opposite surfaces two points are taken, and the straight line joining the points, when produced, does not pass through the vertex, then it will fall within the surface, and produced it will fall outside.[7]

As a consequence, all straight lines connecting the circle and the conic section as in the situation of the second method will fall inside or on the conical surface. So, using two very elementary propositions it can be proved that the method meets the demand.[8] Moreover, this illustrates that at least within the context of Greek geometry of the time people were aware of this central aspect of the reconstructed method. This shows that this part of the method is historically adequate.

In the light of the theory of conic sections as presented by Apollonius, the curve at the top plane of a conical sundial is a conic section. Depending on its properties one would have to call it an ellipse, parabola or hyperbola.

But since the top plane is not orthogonal to any of the straight lines of the conical surface, according to the reconstruction of the older theory of conic sections the curve is not considered as a representative of one of the three types of conic sections – section of an acute-angled cone, section of a right-angled cone, and section of an obtuse-angled cone – that are analyzed and used in the geometry based on this theory. Nevertheless, some statements suggest a broader use of those terms for all curves that can be generated by an orthogonal intersection of a plane with a cone,[9] but the terms themselves are still used for example by Archimedes in his *On Conoids and Spheroids* shortly before the time of Apollonius' *Conics*.

Both the first conical sundials and the change in the concept fall into the same time. So what we know is that some people were aware of those curves and their properties, but the lines might not have been named as conic sections. Again, this shows that the usage of those sections of planes and cones is in accordance with what we know about the history of mathematics.

7 Translation from Taliaferro and Fried 2013.

8 Since a large section of the base circle and its intersection with the part of a conic section at the endpoints of those lines is given, the generation of the surface follows.

9 This interpretation has been suggested by Heath 1921, 439. One example is found in the introduction of Euclid's *phaenomena* (Berggren and Thomas 1996).

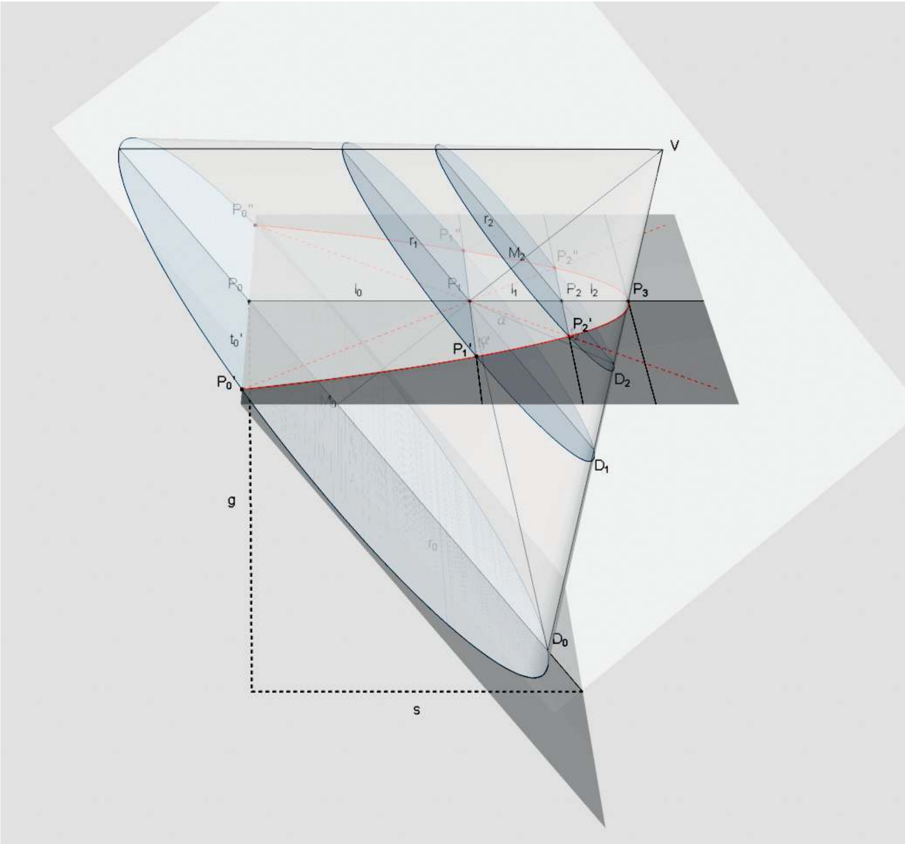


**Fig. 7** Geometry of a situation in a conical sundial that can be used as a basis for the construction of 7 supporting points.

## 5 Methods of drawing the conic section on the top surface

Regardless, whether one calls the edge of the conical shadow receiving surface with the top plane of the sundial a conic sections or not, in order to shape the surface in the identified way one needs a method to find that curve.

Since in most but not all cases the conic section is an ellipse, the method has either to be case sensitive on the type of conic section, or it has to provide the result independent of the type. Due to the spatial limitations on the top surface of the sundial there might be some additional restrictions to the method – unless the conic section is transferred to the object from a separate diagram.

One possibility to do so is based on the following properties of the geometry of a conical sundial (Fig. 7). In a sundial with a right cone that is orthogonal to the upper

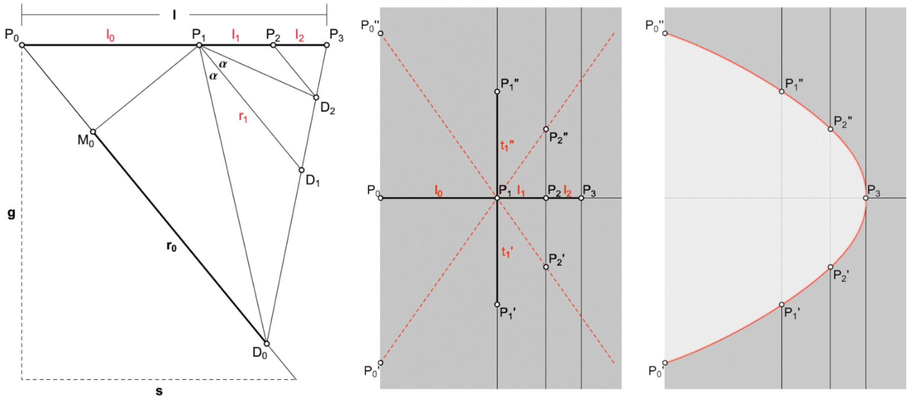


**Fig. 8** Analemma-like diagram (left), construction on the top plane (middle), and connection of the supporting points (right) of a method for drawing an arbitrary conic section on a conical sundial.

part of the front face, each plane parallel to the upper part of the front face intersects the cone in a circle. Let two such circles be given. The first should contain the intersection point $P_1$ of the cone's axis with the top plane,[10] the second circle should be taken such that the angles $D_0P_1D_1$ and $D_1P_1D_2$ are equal. Since in the first plane the middle point $P_1$ of the intersection circle lies on the top plane, the distances $P_1P_1'$, $P_1P_1''$, and $P_1D_1$ are all equal to the radius $r_1$ of this circle. In the given situation, the points $P_0'$, $P_1$, and $P_2''$, as well as $P_0''$, $P_1$, and $P_2'$, lie on a straight line. This is similar to the situation of the rising and setting of the sun on the solstices and the equinoxes that is considered in the analemma diagram. All geometrical properties can be shown by the use of very basic knowledge of the geometry of right triangles and proportions.

For a sundial for which the inclination of the upper part of the front face, the position and radius of the opening circle, and the inclination of the meridian line on the conical surface are given, one can construct a diagram similar to the analemma diagram (Fig. 8 left). In this situation, the positions of $P_1$ and $D_1$ can be derived from the given properties. $D_2$ and $P_2$ then can be constructed such that the angles $D_0P_1D_1$ and $D_1P_1D_2$ are equal. From this diagram one can measure the distances $l_0$, $l_1$, $l_2$, and the radius $r_1$.[11]

10 This point is usually suggested as the supposed position of the tip of the gnomon. Since in many objects significant deviations between this point and the plane of the equinoctial day curve can be observed, there might be another type with deviating gnomon point positions.

11 'Meridian line' relates to both the intersection of the meridian plane with the conical surface (as in this context) and the intersection of this plane with the planar top surface.

Using this information together with the geometrical properties stated above gives the possibility to construct seven points of the conic section (Fig. 8 middle):

- Points $P_0'$ and $P_0''$ are given by the intersection of the opening circle of the cone on the upper part of the front face and the edge between the upper front and top plane
- Points $P_1'$ and $P_1''$ lie on a line which is parallel to the front edge with the distance $l_0$ as constructed in the diagram in Fig. 8 left. Since as in the 3D diagram both points and point $D_1$ lie on the same circle with middle point $P_1$ the distances of points $P_1'$ and $P_1''$ to the meridian line on the top plane are equal to the radius $r_1$ as constructed in diagram in Fig. 8 left.
- Points $P_2'$ and $P_2''$ lie on straight lines through point $P_0''$ ($P_0'$) and point $P_1$. Their distance to the front edge is given in the left diagram (distance $l_0 + l_1$).
- Point $P_3$ lies on the meridian line on the top plane with distance $l$ as in the left diagram.

This construction works independently of the type of conic section of the curve. By this, one does not need to know the type of the curve for finding positions of some of its points or even have a concept of conic sections.

In a last step, the intersection line of cone and plane can be found – at least in a close approximation – by drawing a smooth curve connecting those points (Fig. 8 right).

A very late witness for drawing a conic section by connecting a number of given points is C. Ptolemy in his *Geography*. At one point,[12] he reminds the reader to care about the correct shape of the ellipses in the depiction of the globe within a ringed sphere/astrolabe.

In prop. 25 of the IVth book of his *Conics*, Apollonius shows that

> A section of a cone does not cut a section of a cone or circumference of a circle at more than four points.[13]

According to this, 5 given points suffice to determine the conic section uniquely – as long as the interpolated curve is a conic section. So, the construction provides two additional points. This makes it easier to find the right location of the curve.

The distribution of the given points is a source of errors in the shape of the curve. Since there are only few points in the middle, there is not much guidance in this part. This lack of guidance cannot prevent errors as for example in the Naga sundial: the shape of the conic section is too cuspid in its middle (Fig. 6 left).

12 C. Ptolemy, *Geography*, Book 7, ch. 6.

13 Translation from Taliaferro and Fried 2013.

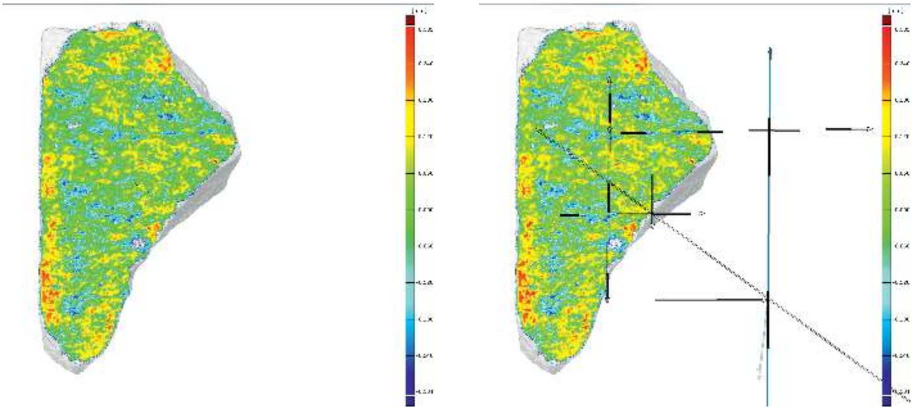

Fig. 9 Constructional lines (right) and their positions (left) on a sundial from Delos (Archaeological Museum, Delos, Inv. B4669) in the 3D model of the object (Berlin Sundial Collaboration 2015b).

Again, for this method we have a similar situation as for the shaping of the conical surface: on one hand, there is a procedure that uses easy geometric constructions to provide everything that is needed to find the correct conic section. On the other hand, there is mathematical knowledge that can prove that the procedure leads to a good result – and that this was known.

There is at least one ancient sundial that shows a very specific set of constructional lines on the top plane that can be part of the construction of those points. Unfortunately, only a fragment of the western part of the upper half of the sundial is preserved. The dimensions of this sundial can be reconstructed according to the principle of the Delos sundials.

All constructional lines are parallel to the intersection of the top and front planes (Fig. 9). One goes through the point most distant to the front edge. The two others are close to the markings of the equinoctial and winter solstitial plane (Fig. 10). The lines are intersected by two other lines that are both orthogonal to the former lines. Two of the intersection points lie very exactly on the now damaged conic section. The line through the intersection point next to the winter solstitial plane and the intersection of the first line with the meridian line on the top plane meets the eastern intersection point of the cone with the front edge and can be seen as constructional line in the back part of the top plane (Fig. 9 left).

A similar situation can be observed on other sundials. What is special in this case is that the foremost constructional line does not coincide exactly with the intersection line of the equatorial plane with the top plane. Otherwise, the lines could also be used to find the location of those planes.

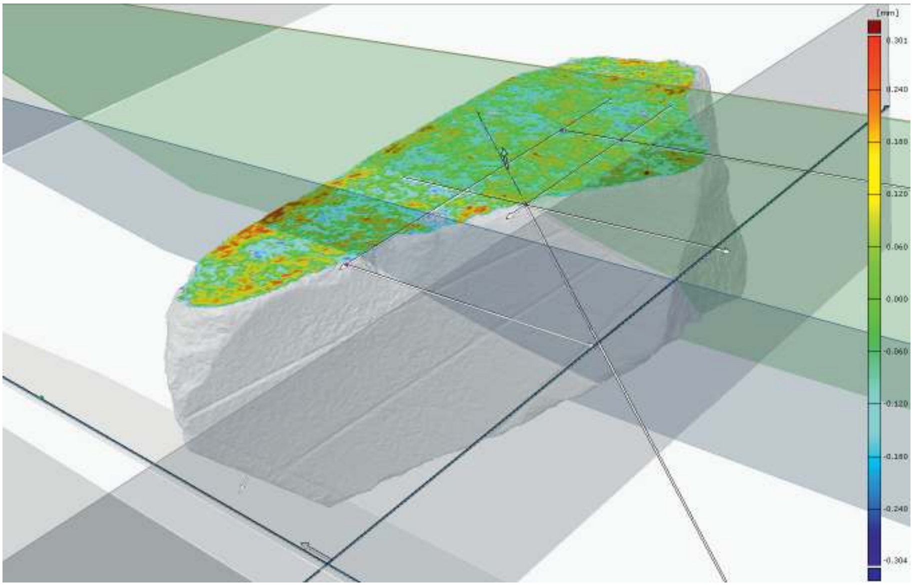

**Fig. 10** Constructional lines and their positions relative to the equinoctial and solstitial plane on a sundial from Delos (Archaeological Museum, Delos, Inv. B4669) in the 3D model of the object (Berlin Sundial Collaboration 2015b).

## 6 Conical sundials and the theory of conic sections

All in all, the conic section that occurs at the intersection of the planar top surface and the conical shadow receiving surface of a conical sundial is not only the result of the geometric configuration of conical sundials. It is crucial in the process of their making. By this, in addition to their contribution to the functionality in other types of sundials, conic sections are of great importance for the design.

The usage of conical shadow receiving surfaces can be traced back into the time of Apollonius of Perga and to a change in the theoretical concepts on conic sections. Whereas the curves that occur in the conical sundials tend to belong to the Apollonian 'universum' of conic sections, the mathematics used in the method for shaping the cone are not specific for this author. The generation of both the conic section and the conic surface can be justified with geometrical properties of cones. Even the number of points that are needed to determine the conic section uniquely or at least this question – according to a common interpretation of Apollonius' own words[14] – goes back to Conon.

14 See also Fried's introduction to Apollonius, *Conics*, Book IV, in: Taliaferro and Fried 2013, 269.

An influence of the methods of constructing conical sundials to the development of the theory of conic sections has not be found.

Mathematics are not only the means by which the correctness of the outcome of a method for building a sundial can be justified. Advanced geometry is also part of the procedure to cut the stone:

*construction of the supporting points of the conic section:* needs a geometrical construction similar to the analemma diagram that has to be transferred onto the stone;

*drawing of the conic section:* requires knowledge of the shapes of conic sections;

*shaping of the conical surface:* requires knowledge of the shapes of cones.

This suggests that elementary knowledge on cones and conic sections was part of the background of craftsmen who built sundials in ancient Greece.

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**Berlin Sundial Collaboration 2015e**
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## Illustration credits

**1** Berlin Sundial Collaboration 2015c (left) and Berlin Sundial Collaboration 2015d (right).
**2** Diagram by Elisabeth Rinner, information resulting from Gibbs 1976, Schaldach 2006, and others.
**3–4** Elisabeth Rinner.
**5** Left: Elisabeth Rinner, based on the 3D model of the object (Berlin Sundial Collaboration 2015a). Right: Photo from Berlin Sundial Collaboration 2015e.
**6** Elisabeth Rinner, based on the 3D model of the object (TrigonArt, Bauer Praus GbR 2015 [2013]).
**7–8** Elisabeth Rinner.
**9–10** Elisabeth Rinner, based on the 3D model of the object (Berlin Sundial Collaboration 2015b).

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Alexander R. Jones

# The Roofed Spherical Sundial and the Greek Geometry of Curves

## Summary

Greco-Roman sundials existed in a great variety of forms, but in most of the common types the curves traced through the day by the Sun's projection at the various stages of the year were circles, straight lines, or conic sections, that is, the kinds of line most commonly investigated in Greek geometry. The variety known as roofed spherical sundials has day curves of a more complicated character; nevertheless, the mathematicians of the time could have investigated their properties by means of trigonometrical and projective resources attested in texts such as Ptolemy's *Almagest* and Pappus's *Collection*.

Keywords: Sundials; geometry; Cetius Faventinus; Vitruvius; Pappus of Alexandria.

Griechisch-römische Sonnenuhren existierten in großer Formenvielfalt, aber bei den gängigsten Typen sind die Kurven, denen die Sonnenprojektion über den Tag und in den verschiedenen Jahresabschnitten folgt, Kreise, gerade Linien oder Kegelschnitte – also die Art von Linien, die am häufigsten in der griechischen Geometrie untersucht wurden. Bei Sonnenuhren mit Lochgnomon und halbkugelförmiger Schattenfläche (*roofed spherical sundials*) treten jedoch kompliziertere Kurven als Tageslinien auf. Nichtsdestotrotz hätten die damaligen Mathematiker deren Eigenschaften mit trigonometrischen und projektiven Mitteln untersuchen können, die in Texten wie dem *Almagest* von Ptolemaios und den *Mathematischen Sammlungen* von Pappos belegt sind.

Keywords: Sonnenuhren; Geometrie; Cetius Faventinus; Vitruvius; Pappos von Alexandria.

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Greco-Roman sundials were products of astronomy, mathematics, and craft. The underlying astronomical theory is that, from a terrestrial perspective, the Sun's movement during the course of a day and night can be idealized, with negligible inaccuracy, as uniform motion along a declination circle of the celestial sphere, i.e. a circle parallel to the celestial equator that is partly above and partly below the observer's horizon. The 'seasonal hours' of the day, which were the seasonally varying time units used in daily life, were defined astronomically as the intervals during which the Sun traverses equal twelfths of the arc of the declination circle above the horizon. The sundial displays the linear projection of the Sun's instantaneous position on the celestial sphere, through a fixed vertex point, upon an immobile surface.[1] This surface is inscribed with a grid formed by two sets of lines: projections of a subset of the declination circles corresponding to key stages of the solar year, called 'day curves', and loci of the projections of the points on all the declination circles corresponding to the endpoints of the seasonal hour arcs, called 'hour curves'. Hence the position of the Sun's projection relative to the grid lines shows not only the current seasonal hour of the day but also the current stage of the year.

The surfaces chosen for sundials were those of simple geometrical forms: planes, spheres, cones, and cylinders; and normally the vertex was the tip of a gnomon so that the projection of the Sun's position was displayed as the tip of the gnomon's shadow. The quintessential Greco-Roman sundial type from a cosmological point of view had a concave spherical surface and a gnomon whose tip was at the center of the sphere, so that the surface is an inverted but geometrically undistorted image of part of the celestial sphere, and the day curves are parallel circular arcs (Fig. 1). Another common type that preserved the day curves as parallel circular arcs had a concave right conical surface whose axis was polar, that is, perpendicular to the plane of the equator, and whose gnomon tip was on the axis. A comparatively rare limiting case of this type flattened the cone into a planar surface parallel to the equator; such equatorial sundials had to consist of a slab with two inscribed faces and two gnomons since the Sun shines on each face of the slab for only half the year.[2] In another, likewise rare, limiting case, the conical

1 In this paper I am not concerned with portable sundials, which for the most part worked on different principles from fixed-position sundials.

2 Six examples are currently known (Herrmann, Sipsi, and Schaldach 2015). Notable among them are the fragments of an exceptionally early – second half of the fourth century BC? – equatorial sundial excavated at Oropos (Archaeological Museum of Oropos, East Attika, inv. A 392, formerly Piraeus, Archeological Museum inv. 235, see Schaldach 2004 and Schaldach 2006, 116–121) and a well preserved one of unknown provenance and date (British Museum 1884,0615.1 = Gibbs 5022G, intended latitude estimated by Gibbs as 32° and by me as 33°, incorrectly identified by Winter 2013, 597 as a vertical sundial).

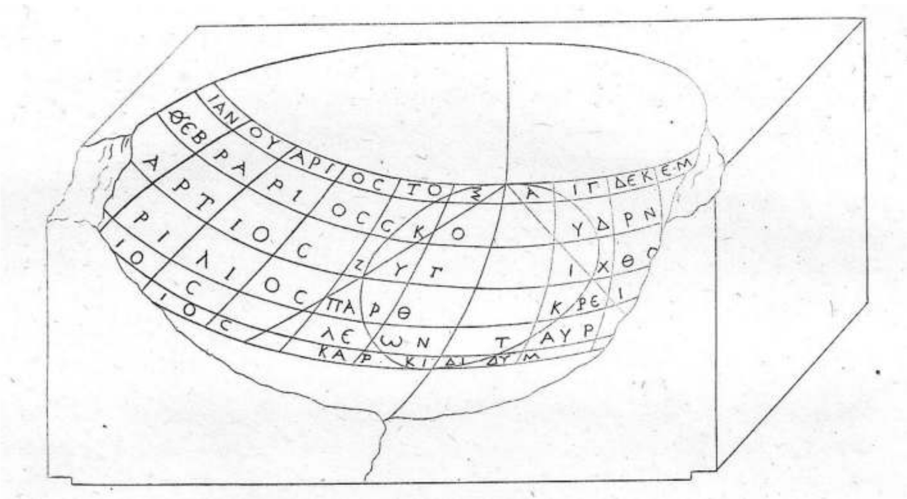

**Fig. 1** Spherical sundial, Vatican, Musei Vaticani inv. 2439 = Gibbs 1068G, found before 1820 on the Esquiline, Rome, likely first century AD and certainly after 8 BC; drawing from Guattani 1811, 102. Gibbs (1976, 184) estimates the latitude for which the sundial was made as 42°, appropriate for Rome (actual latitude 41° 54′). The sundial bowl would have faced south, and the marble block out of which it was sculpted would have been rectangular except for the south face, which would probably have sloped forward from bottom to top so that the upper rim of the bowl could accommodate the projections of the entire arcs of the eastern and western horizons over which sunrises and sunsets take place through the year. The lost gnomon would have been mounted vertically from the middle of the bottom front edge, roughly where the present broken edge shows a notch. The grid is exceptionally elaborate and carefully executed, with labeling inscriptions in Greek. The arcs running from left to right are the day curves correspond to the dates of the Sun's entry into the zodiacal signs, including the winter solstice (top), equinoxes (middle), and summer solstice (bottom). Eleven hour curves separating the twelve hours of day cross the day curves. The circle is an image of the ecliptic divided into twelve equal sectors, used to locate the day curves for the zodiacal sign entries between the solstices and equinoxes, while the two oblique lines indicate the lengthening of days through the course of the year relative to the winter equinox.

surface was stretched out into a concave cylindrical surface with a polar axis.[3] All the foregoing types can be grouped in a general category of polar-axial sundials.

Since the surface generated by the straight lines passing through a fixed vertex and through all points of a declination circle is a right cone, a Greek geometer would immediately have recognized that the projections of declination circles on planar surfaces are

3 A remarkable example of this type, consisting of a cylindrical hole perforating a slab in the plane of the equator, was excavated at Aï Khanoum (No. 2 in Veuve 1982; see also Savoie 2007); it was constructed for latitude 37°, which is approximately correct for Aï Khanoum, except that the hour curves would best fit a latitude of about 25°. The other two polar cylindrical sundials known to me, resembling conventional spherical or conical sundials, are Gibbs 6002G (found at Cumpăna, Rumania, Constanta Archeological Museum inv. 1657) and Gibbs 1053G (uncertain provenance and date, in archeological storage at Thessaloniki, classified by Gibbs as spherical but see Schaldach 2006, 140).

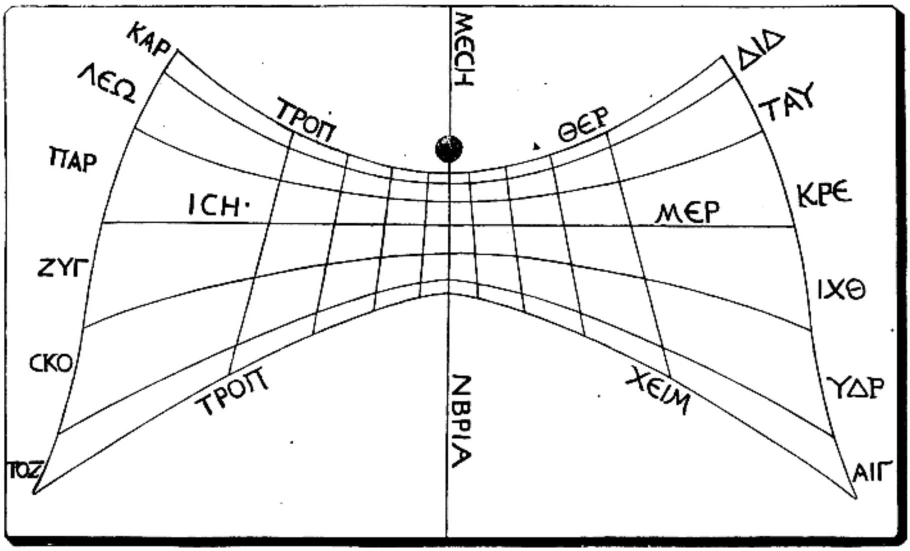


**Fig. 2** Horizontal sundial, Naples, Museo Archeologico Nazionale inv. 3075 = Gibbs 4007, found in 1865 at Pompeii, probably first century AD and certainly not later than AD 79; drawing from Museo archeologico nazionale di Napoli 1867, 16. Only the hole for the vertical gnomon survives; the slab would have been oriented with the top edge (as shown) facing south. The hyperbolic day curves correspond to the dates of the Sun's entry in the zodiacal signs, including the summer solstice (top), equinoxes (straight line at middle), and winter solstice (bottom). labeled with abbreviated names in Greek. The hour curves have been drawn pointwise, and exhibit unexplained sinuosities; in most horizontal sundials the hour curves are drawn as straight lines. Basing the estimate on measurements along the meridian hour curve from the photograph, the sundial was constructed for approximate latitude 41°, appropriate for Pompeii (actual latitude 40° 45′).

conic sections.[4] Aside from the equatorial type that we have just described, the surfaces of ancient planar sundials were either parallel to the horizon or perpendicular to it and facing any horizontal direction.[5] Assuming a terrestrial location between the Tropic of Cancer and the Arctic Circle, the day curves of a horizontal or vertical sundial will always be hyperbolas except for the equinoctial curve, which must be a straight line since the tip of the gnomon lies in the plane of the celestial equator.[6] Although no ancient discussion of the day curves of planar sundials as conic sections survives, there is no doubt that their properties were well within the grasp of Hellenistic geometers; and in

4 Neugebauer (1948) went so far as to suggest that the Greek study of conic sections originated in sundial theory, though he conceded that this hypothesis was difficult to reconcile with the specific orientations of cone and plane by which the curves were generated in the period before Apollonius's *Conics*.

5 In practice one finds vertical dials built to face the four cardinal directions as well as the four directions at 45° from them; the octagonal Tower of the Winds at Athens has sundials facing all eight directions.

6 The conventional axiom of Greek cosmology that 'the Earth has the ratio of a point to the cosmos' implies that the tip of a gnomon is, for all observational purposes, at the center of the celestial sphere.

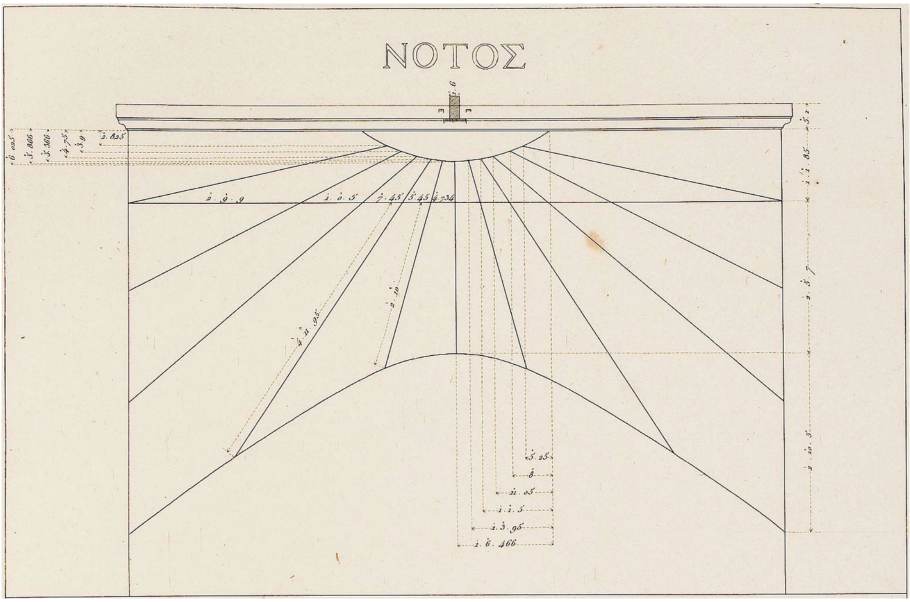


**Fig. 3** Vertical sundial on the south face of the Tower of the Winds = Gibbs 5001, Athens, c. 100 BC; drawing from Stuart and Revett 1825, pl. xix. The hyperbolic day curves correspond to the winter solstice (top), equinoxes (horizontal straight line at middle), and summer solstice (at bottom); they are executed with astonishing accuracy, and were probably based on measurements made on the walls of the Tower after it had been erected (Schaldach 2006, 68–81 and 169–181). The hour curves are inscribed as straight lines. The gnomons on the present-day monument are inaccurate modern restorations.

fact some of the day curves on the best executed planar sundials have the appearance of being the products of theoretical construction calibrated by empirical data (examples Figs. 2–3).

The hour curves, by contrast, would have been beyond the resources of Greek mathematics to handle except in an approximative pointwise manner. If the time units employed had been equinoctial hours (equal twenty-fourths of a day and night, counted from noon or midnight), the hour curves in any polar-axial sundial would have divided all the day curves in similar arcs of 15°, and hence they would have been easy geometrical objects to handle: great circle arcs on spherical sundials, and straight lines on the other types, all lying in planes passing through the polar axis. These same planes that correspond to the equinoctial hours would project on a planar sundial as straight lines, albeit no longer equally spaced. Use of seasonal hours, however, results, for all the sundial types, in hour curves that have rather messy analytical representations that do not lend themselves to geometrical construction in the Greek manner.

Of course *any* shape of surface could be used for a sundial if one does not require the day curves to be circles, straight lines, or conic sections. In practice the designers of Greco-Roman sundials exercised this freedom only in limited ways. One recurring, though not very common, type employed a concave or convex cylindrical surface with a vertical axis.[7] In such sundials, the day curve for the equinoxes is an arc of an ellipse, but the other day curves would not have been tractable by ancient mathematical methods.

The type of Greco-Roman sundial usually designated in English as 'roofed spherical sundial' (the French name *cadran à œilleton* is better) stands out as by far the most popular of the designs whose day curves are not straight lines, circles, or conic sections. According to the latest published information, about thirty-two examples of this type are known, either as existing at present or having existed since the 16th century.[8] This amounts to something between one-fifteenth and one-twentieth of the currently known Greco-Roman sundials, a fraction comparable to that accounted for by horizontal sundials.[9] The great majority are from Italy and fully a third from Aquileia, which thus appears to have been a center of their production in the imperial period.[10] The earliest, however, appears to be the south face of an elaborate late Hellenistic multiple sundial excavated in 1905 in the sanctuary of Posidon and Amphitrite on Tinos, which bears inscriptions associating it with Andronikos Kyrrhestes (c. 100 BC?), the architect of the Tower of the Winds in Athens.[11]

Vitruvius writes tersely of a type of sundial named "hemispherical" (*hemicyclium*),[12] "hollowed out of a rectangular block and undercut in accordance with the latitude".[13]

7 Several examples of concave vertical cylindrical sundial surfaces are elements of rather baroque Roman-period multiple sundials (Gibbs 7004–7007), probably all from Italy. An apparently self-standing one is Gibbs 6001, from Volubilis. The cistern-annex on the south side of the Tower of the Winds bears a convex vertical cylindrical sundial, a type otherwise known only from miniature portable sundials.

8 Gibbs (1976, 195–218) lists twenty-three while Winter (2013, 59) lists twenty-eight, among which 'Durostorum 1' (Silistra, Archeological Museum inv. 517) and 'Serdica' (Sofia) are misidentified, whereas one should add Winter's 'Leptis Magna 3' (a photo of which appears on the book's cover), 'Salona' (Split, Archeological Museum, incorrectly classified by Winter 2013, 539 as a conventional spherical sundial, but see Gibbs 1976, 210, No. 2016G), as well as the following four sundials that are entirely missing from Winter's book: Gibbs 2014G (Trieste, Civic Museum of History and Art), 2021 (Vatican Museum inv. 53875 = PN 5), 2023G (Berlin, Antikensammlung SK1049), and a sundial from Villa B at Oplontis published (without inventory number) in Catamo et al. 2000, 217–218. Bonnin (2012, 22) speaks of thirty-three known roofed spherical sundials without providing a list. Hannah and Magli (2011) have proposed that the Pantheon was a kind of monumental roofed spherical sundial.

9 Gibbs inventories 276 sundials, Winter roughly 400, while the forthcoming catalog by Jérôme Bonnin will list at least 563 (http://bsa.biblio.univ-lille3.fr/blog/2012/09/horologia-romana, visited on 17/7/2017).

10 Schaldach 1997, 35–36; Winter 2013, 60.

11 Tinos, Archeological Museum inv. A 139 = Gibbs 7001G. Whether the Tinos sundial was constructed by Andronikos or merely honors his memory is disputable. On the vexed problem of dating Andronikos and the Tower of the Winds see Schaldach 2006, 61–63.

12 The Greek word ἡμικύκλιον can mean a hemisphere as well as (more commonly) a semicircle.

13 Vitruvius, *De Architectura* Book 9, 8, translated from Rose 1899, 233.

A fuller description provided by the third-century architectural writer Cetius Faventinus removes any doubt that this type is our roofed spherical sundial:[14]

> Let the clock [*horologium*] that is called *hemicyclion* be formed in a similar manner from a stone or a marble block having its four sides broader at the top and narrower at the bottom, so that it has its sides wider behind and on the sides, but let the front lean forward somewhat and make a greater shadow. On the underside of this front let a circumference [*rotunditas*] be drawn with a compass, and let this be hollowed out inwards and make the shape of a hemisphere. In this cavity let there be three circles [*circuli*], one close to the top of the clock, the second through the middle of the cavity, and let the third be marked close to the edge. Next from the smaller circle to the greater seasonal[?] circle[15] [*circulum horalem*] let eleven straight [!] lines be drawn at equal spacing, which are to indicate the hours. Through the middle of the hemisphere, above the smaller circle, let there be a smooth plate of more delicate thickness, so that with a circular finger-size hole having been opened up [*aperta rotunditate digitali*] the ray of the sun, passing within more easily, may indicate the hours through the numbers of the lines. Then at the season of winter it will provide the numbers of the hours through the smaller circle, and in the season of summer it will step through the intervals of the greater circle.[16]

As Cetius writes, the operating surface of a roofed spherical sundial (examples Figs. 4, 5, 6, and 7) is a concave hemisphere that is oriented facing southwards and slightly downwards so that the body of the sundial overhangs the surface, hence the modern designation 'roofed'.[17] At the highest point of the hemisphere is an orifice covered by

14 Cetius Faventinus 310.13–311.2, translated from Rose 1899, 302–303.

15 It is not clear what Cetius intends by *horalis*, a very rare word that one would expect to mean 'pertaining to hours'. The "circle" in question is a day-curve, not an hour-curve.

16 Following the passage translated here, Cetius speaks of two vertical sundial faces oriented eastwards and westwards, but (contrary to the interpretation in Schaldach 1997, 37–38) this must refer to the other type of sundial that he earlier described, the *pelicinum*, which comprised a pair of vertical sundials facing southeast and southwest and joined at the meridian hour line; the sentences in question are likely displaced. For the correct identification of the *pelicinum* see Traversari 1989 and Bonnin 2015, 30–32; incorrect identifications abound. Bonnin (2015, 29–30) doubts whether Cetius is correct in applying the name *hemicyclium* to the roofed vertical type, and demonstrates the existence of a rare roofed conical type, which will not be discussed in the present article.

17 Discussing the Berlin sundial, Staatliche Museen zu Berlin, Antikensammlung inv. SK1049 = Gibbs 2023G, Woepcke (1848 [1842], 38–39) proposed that the sundial would have been mounted lying on the face that we would call its back, and with the face that we would call its top facing south, with disastrous results for his analysis of it. The lion's feet should have made the correct orientation obvious. The mistake, and Woepcke's consequent identification of the type with Vitruvius's *antiboreum* (*De Architectura* Book 9, 8), persist even in fairly recent works on ancient sundials, e.g. Rohr 1970, 14; this despite the fact that other publications starting with Kenner 1880 had shown roofed spherical sundials

**Fig. 4** Roofed spherical sundial, Museo Arqueológico Nacional, Madrid, inv. 33185 = Gibbs 2020, excavated at Baelo Claudia, 1st century AD. This is the general design that Cetius Faventinus knew, a rectangular block with a forwards-sloping south face, and most of the extant roofed sundials follow it. The hemispherical sundial surface is approximately tangent to the top face of the block. The original eyehole would have been a perforation in a metal plate mounted covering the large hole at the top; the plate now occupying this position is a modern restoration. The loop-shaped curves are the day curves for the winter solstice (smallest), equinoxes, and summer solstice (largest, close to the rim of the bowl). Measurement of the inclination of the equinoctial day curve from the digital model shows that the sundial was constructed for a latitude of approximately 48° 30′, whereas the latitude of Baelo Claudia is near 36°.

a plate perforated in an eyelet, through which sunlight penetrates from above. A small spot of light thus falls on the surface at the point that is the projection of the Sun's position on the celestial sphere through the eyelet, which thus functions as a gnomon in reverse.[18]

Situating the vertex of projection on the spherical surface instead of at its center results in a complete change in the geometry of the Sun's projected paths compared to a conventional spherical sundial. At both sunrise and sunset the Sun's projection coincides with the eyelet (treating the eyelet as a geometrical point), so that each day curve is a closed loop. Since the eyelet lies in the plane of the celestial equator and the intersection of any plane with a sphere is a circle, the equinoctial day curve is a complete circle; but, notwithstanding what Cetius writes, this is not true of the day curves for the solstices or for any of the other day curves; in fact, unlike the days curves of polar and planar sundials, those of the roofed spherical sundial do not even lie in a single plane. For the

in their proper orientation, the correctness of which was decisively established by the mathematical analysis in Drecker 1925, 25–34. See Schaldach 2016 for further references.

18 If the eyelet is circular and the edge around it is thin, the projected spot of light will be circular no matter where it falls on the spherical shell. In principle this is a better way of marking the Sun's position than a gnomon shadow because the center of the spot can be easily judged by eye.

**Fig. 5** View from the west side of a three-dimensional digital model of the bowl and front and top faces of the Baelo Claudia roofed sundial, with other faces cut away. A recessed area of the top face accommodates a metal plate perforated with the eyehole.

**Fig. 6** Roofed spherical sundial, Louvre inv. ME1178, acquired 1999, reportedly found at a Roman Villa in Carthage, likely first century AD and certainly after 8 BC. The day curves correspond to the entries of the Sun into the zodiacal signs, and are labeled in Greek. The hour curves are executed with greater theoretical accuracy than those of the Berlin sundial. A plate perforated with the eyehole would have been mounted over the present hole at the top. Savoie and Lehoucq (2001) determined the latitude for which the sundial was constructed to be approximately 41°, much too far north for Carthage (latitude 36° 51′), more nearly appropriate for Rome.

**Fig. 7** View from the west side of a digital model of the bowl of the Louvre sundial. (Model reconstructed by photogrammetry.)

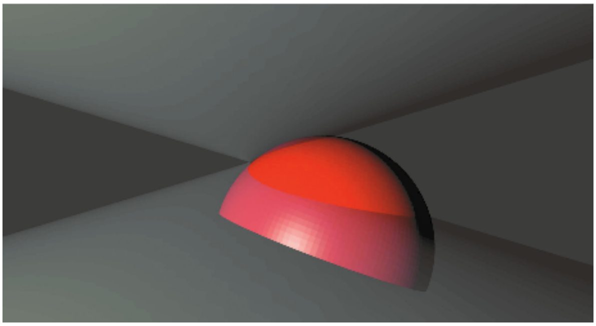

**Fig. 8** Double-napped cone corresponding to solar declinations $\pm\delta$ intersecting sundial bowl, with its vertex at the eyehole point. The figure is oriented so that horizontal lines are parallel to the equator.

general case of a declination circle for declination $\delta$, the day curve is part of the intersection of the spherical surface with a double-napped right cone of aperture $(180^\circ - 2\delta)$ whose vertex lies on the surface of the sphere and whose axis is perpendicular to the plane of the equator (Fig. 8). The portion of this intersection lying on the northern nap is the day curve for declination $+\delta$, and the portion on the southern nap is the curve for $-\delta$ (Fig. 9). Since Greek geometry only dealt with single-napped cones, an ancient geometer would have regarded the day curve as a complete line of intersection of a cone and a sphere, though for purposes of mathematical analysis it would have been useful to work with the curves for equal positive and negative declinations simultaneously.

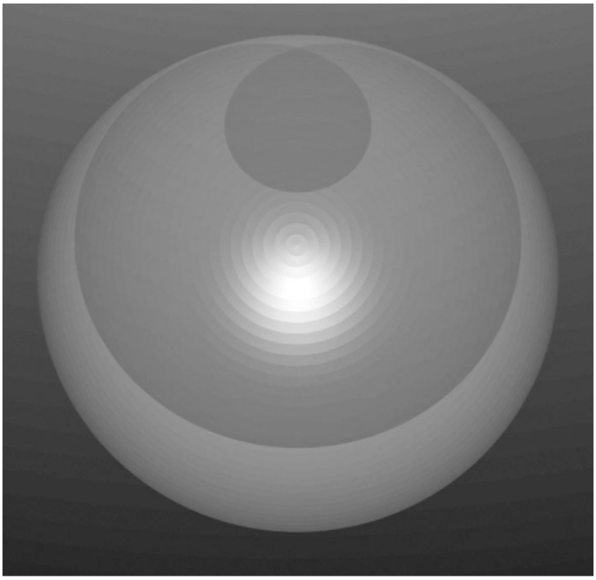

**Fig. 9** Curves of intersection of the cone of Fig. 8 with the sundial bowl, viewed from directly in front of the bowl. The outline of the paler gray region is the day curve for $+\delta$, and that of the darker region is the day curve for $-\delta$.

According to Pappus, Greek mathematicians recognized three classes of lines as tractable mathematical objects: straight lines and circles, which could by hypothesis be invoked in a given plane without having to justify their generation; conic sections, which could be generated by the intersection of the plane with simple three-dimensional surfaces (cones and cylinders); and miscellaneous other curves, which could be generated either by imagined 'mechanical' contrivances or by geometrical constructions involving the intersections of three-dimensional surfaces.[19] He delineates a hierarchy of geometrical problems, according to which a problem that can be solved using just straight lines and circles is called 'planar' (ἐπίπεδον) and *should* only be solved using these 'planar' objects, while a problem that is not planar but that can be solved by introducing one or more conic sections is 'solid' (στερεόν), and one that can be solved using another variety of curved line is 'curvilinear' (γραμμικόν). Pappus attributes to the geometers a strict view that it was "no small fault" when a problem was solved by curves that are not proper to its classification, which would mean that special curves should only be invoked when a problem cannot be solved using just straight lines, circles, and conics, in practice special curves were sometimes applied to 'solid' problems, perhaps because it was easier to devise an apparatus for drawing them.

An ancient mathematician would easily have seen that the day curves of any sundial are the intersections of cones defined as above with the sundial surfaces. What is less obvious is whether a mathematician would have been capable of discovering and

19 Pappus, *Collection* Book 4, cited after Hultsch 1876–1878, Vol. 1, 270–272.

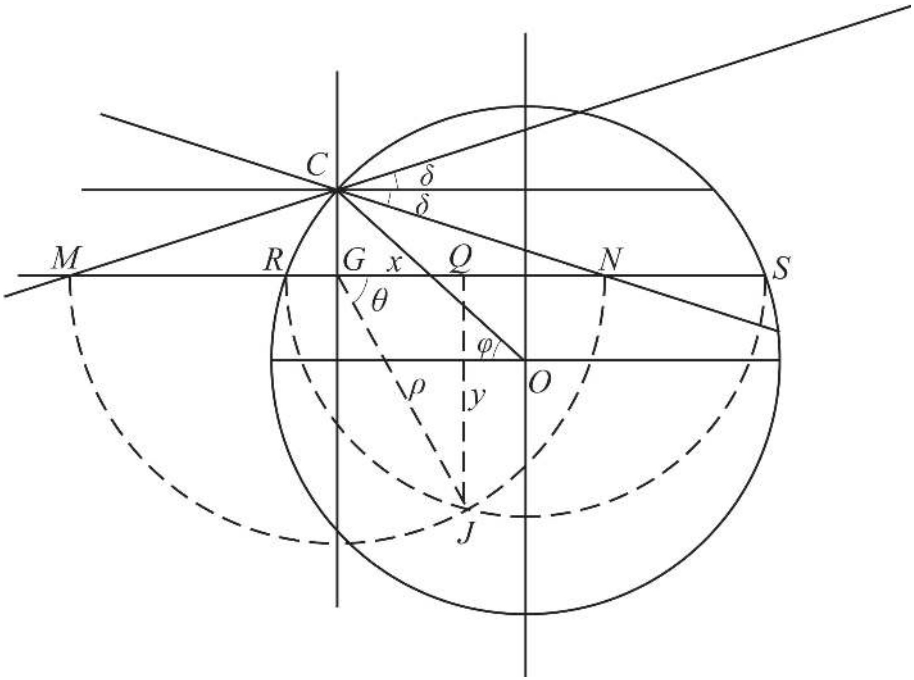


**Fig. 10** Conditions determining the coordinates $x$, $y$ (i.e. $GQ$, $QJ$ as defined in the text) of a point $J$ lying on the day curve for declination $+\delta$ (adapted from Drecker 1925, Plate 4, Fig. 43). Solid lines are in the meridian plane through the center $O$ of the sphere of the sundial bowl, broken lines in an arbitrary plane of section parallel to the equator and passing through $RS$. The diagram is oriented so that lines parallel to the equator are horizontal. $C$ is the eyehole point; the semicircle on diameter $MN$ is the section of the declination cone cut by the arbitrary plane, and the semicircle on diameter $RS$ is the section of the sphere cut by the plane.

demonstrating properties of the day curves in a roofed spherical sundial. Curves in three dimensions were certainly objects of study; examples include the helix, the hippopede of Eudoxus, and the intersection of a torus and a cylinder employed by Archytas in his construction of the two mean proportionals. Pappus, *Collection* 4 contains a discussion of a technique of generating surfaces ('cylindroids') as the loci of straight lines perpendicular to a given plane and passing through points of a given curve;[20] by taking the intersection of such a cylindroid with a planar or curved surface, one could obtain new and possibly more mathematically tractable curves as a form of projection of the original curves. The fact that certain 'mechanically' generated curves such as the quadratrix could also be related to intersections of surfaces was a matter of interest.

Expressed in suitable orthogonal coordinates $x$, $y$, and $z$, the intersection of a sphere with a cone would be the solution of a pair of quadratic equations. Drecker demon-

20 Hultsch 1876–1878, Vol. 1, 258–264.

strated that if we set the origin at the eyehole point, the $x$-axis oriented south-north in the plane of the equator, and the $y$-axis oriented east-west in the plane of the equator, then (Fig. 10):[21]

$$\left(x^2 + y^2 - 2r\cos(\varphi)\cos^2(\delta)\,x\right)^2 = r^2\sin^2(\varphi)\sin^2(2\delta)\left(x^2 + y^2\right) \tag{1}$$

where $r$ is the diameter of the sphere and $\varphi$ is the terrestrial latitude for which the sundial is constructed. Disregarding $z$, this quartic equation describes the day curve projected orthogonally into the plane of the equator (Fig. 11).[22] As Drecker remarks, it is the equation of a limaçon of (Étienne) Pascal. A characteristic property of the limaçon becomes apparent if we express the equation in polar form:

$$\rho = 2r\cos(\varphi)\cos^2(\delta)\cos(\theta) \pm r\sin(\varphi)\sin(2\delta) \tag{2}$$

Since

$$\rho = 2r\cos(\varphi)\cos^2(\delta)\cos(\theta) \tag{3}$$

is the equation of a circle passing through the origin, the limaçon is the locus of points at a constant distance from the circle as measured along any straight line through the origin. Hence the limaçon is also known as the conchoid of a circle, an analogue of the conchoid of Nicomedes which is the locus of points at a constant distance from a straight line as measured along any straight line that passes through an origin not lying on the given straight line.

The conchoid of Nicomedes was a 'mechanical' curve (in principle drawable by means of a special compass) introduced in the Hellenistic period as a way of allowing certain so-called *neusis* constructions, which are constructions that can be reduced to the postulates of *Elements* Book 1 only in special conditions. A *neusis* is the construction of a straight line passing through a given polar point, such that the part of the line cut off between two intersections with given straight lines or circular arcs has a given length. When at least one of the given lines is a straight line, the *neusis* can be performed by drawing the conchoid of Nicomedes generated from the given polar point and the given straight line, and then finding the intersections of the conchoid with the other given line. Certain geometrical problems (for example in Archimedes *On Spirals*) could be reduced to *neuses* in which one of the bounding lines was a circle and the polar point

21 Drecker 1925, 26–27.

22 Drecker also shows that the orthogonal projection of the day curves for ±δ in the meridian plane is a parabola. I doubt that this would have been realized in antiquity; the body of the sundial obstructs this perspective from view.

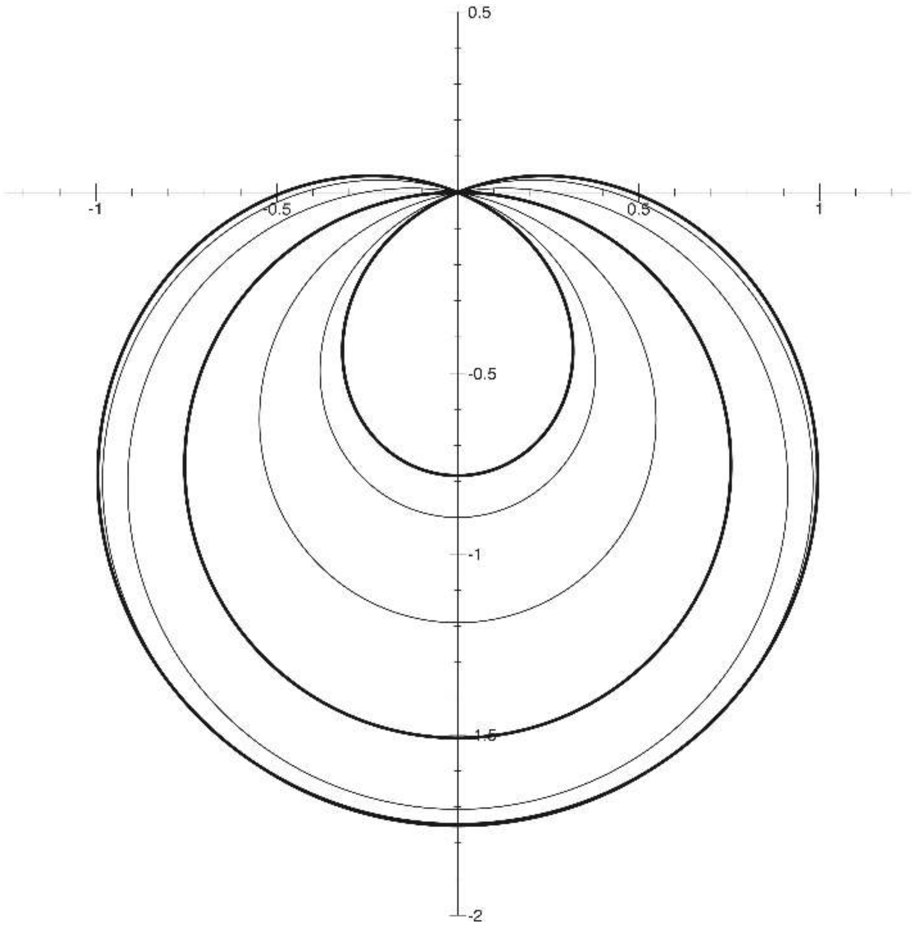


**Fig. 11** Orthogonal projection into an equatorial plane of day curves for $\varphi = 41°$, declinations corresponding to the Sun's entry into the zodiacal signs. Thicker curves are for the winter solstice (innermost), equinoxes, and summer solstice (outermost). Cf. Fig. 6 right.

lies on the same circle, and there is good reason to believe that the limaçon of Pascal was known in antiquity as a resource for resolving such *neuses*.[23]

The fact that the day curves of a roofed spherical sundial are projections of limaçons on the spherical surface is mathematically appealing, and a geometer familiar with the planar curves might have suspected it simply from the look of the day curves on an empirically constructed roofed spherical sundial. But could the geometer have *proved* it? Drecker's analytical approach to the problem does not translate well into a synthetic form that one could imagine being discovered in antiquity. However, a deduction of the

23 Knorr 1986, 222 and 258.

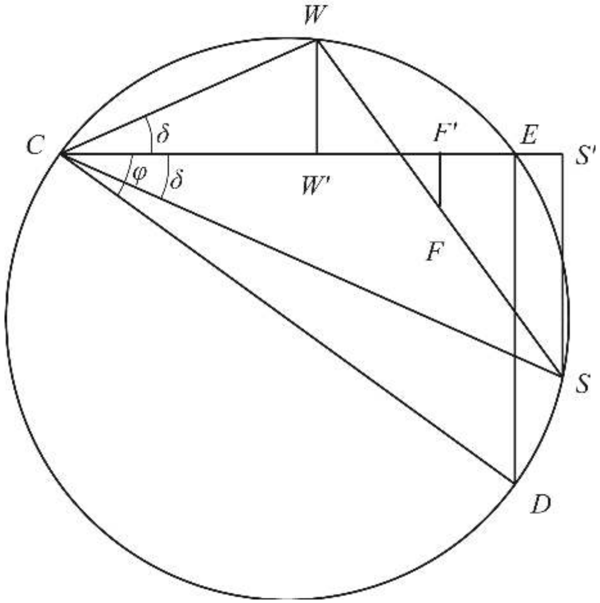


**Fig. 12** Meridian section of sundial sphere.

limaçon would have been within reach of someone equipped with the basic theorems underlying the planar trigonometry of Book 1 of Ptolemy's *Almagest*. In the following conjectural reconstruction I employ for the sake of clarity modern trigonometric functions instead of Ptolemy's chord function.

Fig. 12 shows the cross-section in the plane of the meridian of the complete sphere to which the sundial's bowl belongs, oriented so that the intersection of the meridian and equatorial planes is horizontal in the diagram. $C$ is the eyehole point, $E$ is the projection of the Sun at noon on an equinox, and $W$ and $S$ are respectively the projections of the Sun at noon on dates when the Sun's declination is $-\delta$ and $+\delta$. $CD$, the diameter of the sundial sphere passing through $C$, is perpendicular to the horizon of the locality for which the sundial has been constructed. Hence

$$CE = 2r\cos(\varphi) \tag{4}$$

$$CW = 2r\cos(\varphi + \delta) = 2r\left[\cos(\varphi)\cos(\delta) - \sin(\varphi)\ \sin(\delta)\right] \tag{5}$$

$$CS = 2r\cos(\varphi - \delta) = 2r\left[\cos(\varphi)\cos(\delta) + \sin(\varphi)\ \sin(\delta)\right] \tag{6}$$

Let $W'$ and $S'$ be the orthogonal projections of $W$ and $S$ in the plane parallel to the equator that contains $C$ and $E$, and let $F'$ be their midpoint. Then

$$CF' = \cos^2(\delta)\, CE \tag{7}$$

which, we note, does not depend on $\varphi$. Moreover,

$$F'W' = F'S' = 2r\sin(\delta)\cos(\delta)\sin(\varphi) \tag{8}$$

We now consider (Fig. 13) a different cross-section of the sphere in an arbitrary plane containing $C$ and perpendicular to the equator. Points $E''$, $W''$, and $S''$ are projections of the Sun at certain times of day (not necessarily the same times) on an equinox and on dates when the Sun's declination is respectively $-\delta$ and $+\delta$ as before. Let $D''C$ be the diameter of the circle of the cross-section, and let $\varphi''$ be angle $D''EC''$. By the same argument used to find $CF$, we have

$$CF''' = \cos^2(\delta) CE'' \tag{9}$$

Hence $CF'''/CE''$ is constant and equal to $CF'/CE$, so that $F'''$ lies on the circle with diameter $CF'$ in the equatorial plane through $C$. Again,

$$F'''\,W''' = F'''\,S''' = 2r''\sin(\delta)\cos(\delta)\sin(\varphi'') \tag{10}$$

But

$$\sin(\varphi'') = \frac{E''D''}{2r''} = \frac{ED}{2r''} = \left(\frac{r}{r''}\right)\sin(\varphi) \tag{11}$$

So

$$F'''\,W''' = F'''\,S''' = 2r\sin(\delta)\cos(\delta)\sin(\varphi) = F'\,W' = F'\,S' \tag{12}$$

which establishes that $W'''$ and $S'''$ lie on the two branches of a limaçon generated by the equatorial circle on diameter $CF'$ with $C$ as pole.

The day curves projected into the equatorial plane exhibit an asymmetry that has an analogue in planar sundials. As we have seen, the day curves corresponding to solar declinations of $+\delta$ and $-\delta$ are the intersections of the sundial surface with the two naps of a single double-napped cone. Hence from the modern perspective the two hyperbolic day curves for equal but opposite declinations on a planar sundial are the two branches of a single hyperbola (a Greek geometer would have called them a pair of 'opposite' hyperbolas); but the straight line that is the day curve for the equinoxes is not equidistant

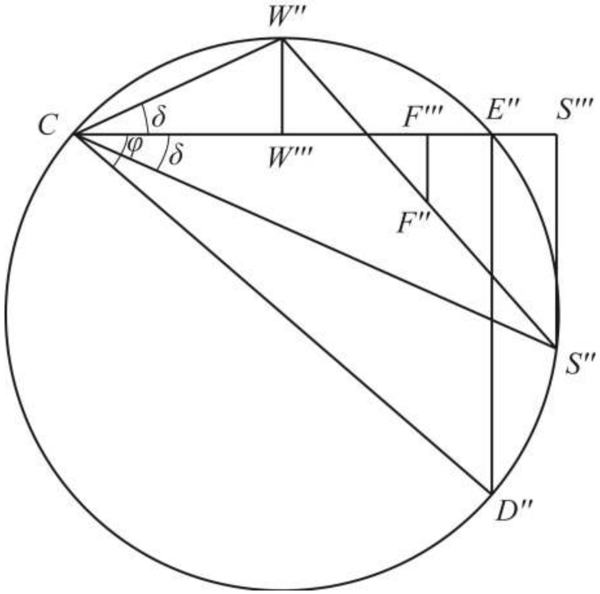


**Fig. 13** Section of sundial sphere in arbitrary plane through eyehole point *C* and perpendicular to the equator.

from the branches – this is very obvious in horizontal and south-facing vertical sundials. Similarly in the roofed spherical sundial, since the diameter of the generating circle of the limaçon projected in the plane of the equator for declination $\pm\delta$ is not $CE$ but $CF'$, which is progressively smaller than $CE$ as $\delta$ increases, the projections of the day curves for positive declinations are crowded closer together than those for negative declinations. However, on the spherical surface itself the arcs separating the day curves for $\pm\delta$ from the equinoctial day circle, measured along the circular section through $C$ and perpendicular to the equator as in Fig. 13, are equal to each other and subtend the same central angles ($2\delta$) as their counterparts in the plane of the meridian.[24] Since it is easy to draw a series of these circular sections on the spherical bowl – they are the circles through $C$ whose centers lie on the great circle of the sphere parallel to the equator – this property would make it easy to accurately construct the day curves pointwise. Moreover, a viewer standing reasonably close to the sundial will see a clearer separation between the day curves near that of the summer solstice than one gets in the equatorial projection.

24 Gibbs (1976, 98–99, note 10) incorrectly states that the arcs separating the day curves for $\pm\delta$ from the equinoctial curve are constant as measured along great circles through $C$.

Vitruvius ascribes the *hemicyclium* or roofed spherical sundial to Berossus the Chaldean, the Babylonian scholar who reportedly resided in Kos in the third century BC. We may reasonably be skeptical about this attribution.[25] But it is interesting to observe the company Berossus keeps in Vitruvius's list of inventors of sundial types, among whom we find Eudoxus, Aristarchus of Samos, Apollonius, and Dionysodorus, all of whom were distinguished mathematicians or mathematical astronomers. Whatever the specific accuracy of these credits, Vitruvius leaves us in no doubt that sundial design was regarded as field appropriate for a mathematician, and that the great variety of sundial types was a manifestation of scientific creativity. Many of the known examples of roofed spherical sundials were prestige objects exhibiting a high level of ornamental as well as geometrical skill in their sculpture. The comparative popularity of the type likely resulted in part from certain practical advantages. Unlike vertical sundials, they yielded an easy reading of the hour at all seasons and all times of day; while, unlike conventional spherical or conical sundials, they were well suited to mounting at eye height or above. But beyond this, the unobvious beauty of the inscribed curves of a well-executed roofed spherical sundial would have pleased the mind as well as the eye of the connoisseur.

25 See Steele 2013 for a judicious consideration of the astronomical and astrological testimonia concerning Berossus, concluding that some of the reports may be genuine but that Berossus had little or no connection with genuine Babylonian astronomy. The alleged invention of the sundial is mentioned on pp. 118–119.

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# INTERCONNECTIONS IN ANCIENT SCIENCE

Jens Høyrup

# Practitioners – School Teachers – 'Mathematicians': The Divisions of Pre-Modern Mathematics and Its Actors


## Summary

The paper starts by looking at how 'practical' and 'theoretical' mathematics and their relation have been understood from the Greeks to Christian Wolff and by historians of mathematics from Montucla to recent days. Drawing on earlier work of mine, and on the giants on whose shoulders I (try to) stand, I then suggest a categorization of the mathematical knowledge types a historian has to deal with: the 'sub-scientific' type, carried by practitioners taught in an apprenticeship network; the 'scholasticized' type, taught supposedly for practice but in a 'scribal' school by masters whose own genuine practice is that of teaching; and the 'scientific' or theory-oriented type. In the end, the utility of this categorization is tried out on two specific cases.


Keywords: Knowledge types in mathematics; educational types; Old Babylonian 'algebra'; abbacus mathematics; historiography of mathematics.

In diesem Beitrag wird zunächst untersucht, wie ‚praktische' und ‚theoretische' Mathematik und ihre Verbindung aufgefasst wurden, von den antiken Griechen bis zu Christian Wolff und von Mathematikhistorikern von Montucla bis heute. Ausgehend von früheren Arbeiten von mir und von Riesen, auf deren Schultern ich stehe (oder zu stehen versuche), schlage ich anschließend eine Kategorisierung mathematischer Wissenstypen vor, mit denen sich ein Historiker auseinandersetzen muss. Den ‚sub-wissenschaftlichen' Typ verkörpern Praktiker, die als Lehrlinge von eigentlichen Praktikern unterrichtet wurden; der ‚Schulungs-Typ' wurde wohl auch für die Praxis gelehrt, allerdings vermutlich in ‚Schreiberschulen' von Meistern, deren eigene ‚Praxis' ausschließlich in der Lehre bestand; schließlich der ‚wissenschaftliche' theorie-orientierte Typ. Anhand von zwei Beispielen wird diese Einteilung am Ende überprüft.

Keywords: Wissenstypen in der Mathematik; Ausbildungstypen; altbabylonische ‚Algebra'; Abakus Mathematik; Historiographie der Mathematik.

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I suppose I have known Lis longer than any other contributor to this volume, years before any of us knew we would end up as historians – namely since she started university in 1964. By then, or at least a couple of years later, she intended to study physics, as I actually did. But even that was in the future – potential and never actualized future in Lis's case, as we know. Actually, physics was not what we spoke about by then; during her first year in mathematics, I was her instructor of algebra, so it was boolean logic, linear algebra and groups. Even though Lis was always sitting as far back as possible, I remember *where* she was sitting, close to the window. Already in first-year algebra, she was of course impressively bright. So, when we later ended up in our present-day pigeon-holes, we were in no doubt about each other.

Having never worked on astronomy (except when review editors have sent me something on the topic, not knowing that 'Babylonian mathematics' and 'Babylonian astronomy' are not only different topics but also on the whole as far in time from each other as Charlemagne from Churchill), I shall not contribute anything within Lis's own field. Instead I shall present a bird-eye's view of something I know better.

However, before approaching that subjectmatter, let me offer a personal note: I believe – but obviously cannot be sure about a matter of this kind – that my interest in practitioners' knowledge as an autonomous body goes back to the three years I taught physics at an engineering school some forty-five years ago, having thus moved away from the environment where I had met Lis. Among other things I remember one episode which to me has always illustrated the relationship between theoretician's knowledge and what is often (too often, I would argue) supposed to be its 'application'. Two colleagues – say, B and H – planned and held a course in electrodynamics for students of constructional engineering. H had been trained as an engineer himself, while B was a nuclear physicist. They were very good friends, and agreed upon most of what one can agree upon in this world. None the less, H one day complained to me that "B removes a Maxwell equation a year, but nothing changes!" Evidently, merely simplification of high theory was not what was needed in order to bridge the gap between the theoretician's and the engineering organization of knowledge.

## 1 Proto-historiography

Herodotos, followed by numerous other ancient Greek writers until Proclos, maintained that geometry began as (Egyptian) practice, and was later transformed into (Greek) theory; nothing was said by them about theory becoming in its turn the guide for the corresponding practice, although Hero and a few others tried to accomplish something like that (with modest impact outside the realm of war machines).

The standard view of the High Middle Ages – the epoch where the Latin Middle Ages had developed a scientific culture enabling them to form an opinion of their own in the matter and not just repeat what had already been repetition with Isidore – was not very different. In the introduction to the 'Adelard III' version of the *Elements*[1] we read that in the case of geometry, as in that of any other skill (*facultas*), *usus* not only preceded theory (*artificium*) but also continues to exist as the *exercitatio* of the skill; the main difference with respect to Antiquity is that the writer – himself certainly an *artifex* – demonstrates to have some interest in the practical *exercitatio*, as could reasonably be expected from someone who had Hugues de Saint-Victor in his intellectual luggage.

Though knowing *the field* of mathematics, perhaps both as theory/*artificium* and as a tool for practice / an *exercitatio*, neither Antiquity nor the medieval epoch was familiar with the figure of the *mathematician* in our sense of the word. At first, a μαθηματικός was a member of a branch of the Pythagorean movement; later in Antiquity, the *mathematicus* would mostly be an astrologer of the 'Chaldean' type; the teacher of the mathematical Liberal Arts – the closest we may perhaps come to a *professional* mathematician – would mostly be designated a *geometer*, while a μαθηματικός in the teacher's garb might teach any liberal or philosophical art. Aristotle does speak about the person who is engaged in mathematical argumentation as a μαθηματικός, but this is a personification of his ideal of epistemological autonomy of the various fields of knowledge, still no professional role. The Latin Middle Ages often did try to distinguish between the *matematicus*, that is, the astrologer, and the *mathematicus*, the one who practiced mathematics; but it would be difficult to find a person who primarily identified himself as a *mathematicus*.

Attitudes begin to change in the Renaissance. In a lecture on the mathematical sciences held in Padua by Regiomontanus in 1463/64 (the introduction to a series of lectures on al-Farghānī, printed by Schöner in 1537),[2] everything is seen in a (social as well as metatheoretical) top-down perspective. Mathematics is essentially *theory*, deriving its deserved high prestige, on one hand from its roots in classical Antiquity, on the other from its utility for philosophy and from its civic utility (which consists in procuring courtly pleasure). Much lower merit is ascribed to the applications[3] taught in the abbacus school (commercial computation, area calculation, etc.), and next to none to its use in material production. Whether these low-ranking applications are presumed to derive from theory is not clear.

Regiomontanus was ahead of his times, not only in the sense that he was a better mathematician than any contemporary in the Latin world but also in his attitudes to the character and role of mathematics (attitudes that he could only develop because of his mathematical insights and aptitudes); but a writer who is 'ahead of his times' is still

1 Ed. Busard 2001, 31–32.

2 Facsimile in Schmeidler 1972.

3 An inadequate term, since it presupposes that theory is 'applied to' (put upon) some practice; I use it for lack of a comprehensible one-word alternative.

bound to his times in many ways. A more mature expression of the conception of the relation between theory and practice that ripened during the later Renaissance is found in Vesalius's introduction to his *De humani corporis fabrica*.[4]

Vesalius, of course, discusses the medical art, not mathematics. This art, in his opinion, had been almost destroyed by the fact that responsibility for exerting it had been parceled out into three shares: that of the physician, the one who knows the principles of the art but does not know how to use a knife – or does not dare to lest his social standing might suffer; that of the pharmacist, who at least works under the guidance of the physician (that is, under the control of the medical faculty of universities); and that of the barber or surgeon, ignorant of everything according to Vesalius and therefore unable to do *adequately* that which in fact he does: use his hands. The art can only be restored to its former splendor if the three shares are once again united, *and 'the hand' brought under the control of the theoretically schooled physician*. In other words: practice – even the dirty practice of cutting and bloodletting – has to become applied science.

Vesalius, as is well known, inaugurated a period of rapidly progressing insights in anatomy. Medicine understood as the art of healing did not keep up with this progress in theory, but Vesalius had some sound justification for his claim. Slightly later we see a similar but stronger claim being made for mathematics by Petrus Ramus. Ramus, as is equally well known, wanted to avoid Euclid's 'Platonic error', the teaching of theory for theory's sake; but his alternative was an edition of the *Elements* where the proofs had been replaced by explanations of the utility of the single theorem. Theory should thus, as also requested by Vesalius, reform its mind and discard the mistaken fear of practical utility and dirty hands; but (reformed) theory should govern. In the historical introduction to Ramus's *Scholae mathematicae*[5] this view reveals its purely ideological character in the claim that the three famous great discoveries – the magnetic compass, gunpowder, and printing – were made in Germany because the mathematician Heinrich von Hessen had been forced to leave Paris in the 1380s and go to Vienna, thus inaugurating the blossoming of German mathematics; Ramus also wonders[6] that applied mathematics flourishes more in Italy than elsewhere in spite of the modest number of university chairs in mathematics, ignoring the existence of the abbacus school institution (*deliberately* ignoring it for sure, just as he deliberately ignores Stiefel from whom he copies wholesale though at the modest level he understands – probably indirectly but from authors like Jacques Peletier who do tell their debt to Stiefel).

In the sixteenth century, the 'mathematician' became a recognized social role, not least for those 'higher mathematical practitioners' who moved around the Italian courts;[7] Baldi's majestic *Vite de' matematici* illustrate the development.

4 Vesalius 1543, $2^r$–$2^v$.

5 Ramus 1569, 64–65.

6 Ramus 1569, 107.

7 Biagioli 1989.

What is most charitably characterized as Ramus's pipedream gradually materialized as reality over the next couple of centuries – first by the efforts of *Rechenmeister* like Tartaglia and Faulhaber to appropriate whatever Euclidean and Archimedean knowledge they might need (for their practice or for their social standing), afterwards in the interplay between these creators of new branches of mixed mathematics and mathematicians with scientific training and engaged in developing useful knowledge, for instance at the request of the Académie des Sciences. In his *Mathematisches Lexikon* from 1716, Christian Wolff recognizes that "*mathesis practica*, die ausübende Mathematick" as a category does not coincide with "*mathesis impura sive mixta*, die angebrachte Mathematick" – the latter being the application of mathematical understanding to "human life and nature", whether for the purpose of doing something *or* for obtaining theoretical insight.[8] He adds, however, that

> It is true that performing [*ausübende*] mathematics can be learned without reasoning mathematics; but then one remains blind in all affairs, achieves nothing with suitable precision and in the best way, at times it may occur that one does not find one's way at all. Not to mention that it is easy to forget what one has learned, and that that which one has forgotten is not so easily retrieved, because everything depends only on memory. Therefore all master builders, engineers, calculators, artists and artisans who make use of ruler and compass should have learned sufficient reasons for their doings from theory: this would produce great utility for the human race. Since, the more perfect the theory, the more correct will also every performance be.[9]

After the creation of the École Polytechnique and its nineteenth-century emulations there was no longer any need to repeat this protestation. For pragmatic reasons, Wolff's distinction between the 'practical' and the 'mixed' could be discarded – as it was already discarded in the names given by Gergonne and Crelle to their journals, respectively *Annales de mathématiques pures et appliquées* and *Journal für reine und angewandte Mathematik*.

## 2 Historiography

Modern historiography of mathematics begins, we might say, with the generations from Montucla and Cossali to Libri and Nesselmann. These were still close to the victory of the 'Vesalian' subordination of practice under reformed theory, and furthermore

8 Wolff 1716. On pp. 866–867 Wolff observes that "everything in mathematics beyond arithmetic, geometry and algebra [which constitute his 'pure mathematics'] belongs to accommodated mathematics". As everywhere in the following where no other translator is identified, the translation is mine.

9 Wolff 1716, 867.

brought up mathematically before the triumphs of the 'new' pure mathematics inaugurated by Cauchy, Abel, etc. Finally, they were hungry for sources of any kind. No wonder hence that their attitudes would still have some of their roots in the situation delineated by Wolff. Montucla, when telling in his second edition[10] about Ottoman, Arabic, Persian, and Indian mathematics, actually applies what in one of the current meanings of that word can be characterized as an *ethnomathematical* perspective, describing (briefly) teaching practices as well as the uses of mathematics and computation in general social life.[11]

However, the interest in practical mathematics did not die with their generations. When dealing with pre-Modern mathematics, historians like Boncompagni, D. E. Smith, Tropfke, Karpinski, and Vogel would still pay much attention to sources that had their roots in practice. At least as a rule, they abstained from using the term 'mathematicians' about the originators of what several of them termed 'school mathematics' or 'elementary mathematics'. Given the sources they relied on,[12] neither designation was mistaken; but they express a belief in the unity of the mathematical genres that agrees with Wolff's ideal (and with the perspective of their own times) but not – as I shall argue – with the social reality of pre-Modern mathematics.

Around 1930, the perspective changed.[13] History of mathematics came to be understood as the history of the *mathematics of mathematicians*, and mathematicians tended to be defined in post-Cauchy-Abel terms. In part that was a consequence of the disappearing interest in European medieval mathematics, on which next to nothing was published between 1920 and 1948.[14] But this explanation from the object of the historian is partial at best: in the 1920s and the early 1930s, the appearance of two good editions of the Rhind Mathematical Papyrus and the publication of the Moscow Mathematical Papyrus spurred some further publication activity; from the late 1920s onwards, the Babylonian mathematical texts were cracked and published, which had a great impact, not least through the acceptance of Neugebauer's thesis about the descent of Greek 'geometric algebra' from Babylonian 'algebra'. However, in the perspective of the epoch, even

10 Montucla and Lalande 1799–1802, in particular Vol. I, 397–402, but also elsewhere.

11 Cf. D'Ambrosio 1987; Mesquita, Restivo, and D'Ambrosio 2011.

12 Namely, manuscripts and printed works. Montucla, when making his proto-ethnomathematics, had relied on ethnographic informants (diplomats and other travelers), and elsewhere uses his direct acquaintance with practitioners to supplement what he can document from written sources. But the historians of mathematics of the following 150 years, like other historians from von Ranke's century, relied on *documents*.

13 Given, for instance, that Vogel lived and worked until 1985 it goes by itself that this statement is an extremely rough approximation to *wie es eigentlich gewesen*, permissible only in the context of an introductory discussion.

14 Most of the few publications that did appear are from Karpinski's hands. If these are excluded, the general absence of interest in medieval Latin and vernacular European mathematics becomes even more striking.

Babylonian mathematics came to be understood as the product of 'Babylonian mathematicians'.[15] Moreover, even the historiography of Early Modern mathematics tended to turn away from the applications of mathematical theory and to concentrate on 'real' mathematics.

## 3 Missed opportunities

Two events should be mentioned at this point, not because they affected the historiography of mathematics but rather because it might seem strange that they did not.

The first is the renowned intervention of Soviet scholars at the London Congress of the History of Science in 1931.[16] Within the historiography of science, Boris Hessen's paper on "The Social and Economic Roots of Newton's *Principia*" was indubitably the one that had the strongest impact. By way of J. D. Bernal's reception and ensuing successful campaign for the implementation of *science policy*, Bukharin's paper on "Theory and Practice from the Standpoint of Dialectical Materialism" and M. Rubinstein's presentation of the "Relations of Science, Technology, and Economics under Capitalism and in the Soviet Union" were probably those that were most consequential.

Hessen's paper was written under conditions which his audience did not know about, and carried a subtle message that it missed.[17] Bukharin shared Hessen's fate not only in life (both fell victims to Stalin's purges in 1938) but also as regards his London paper. As observed by I. Bernard Cohen, "Bukharin's piece remains impressive today [c. 1989] to a degree that Hessen's is not".[18] But that went largely unnoticed in 1931.

Bukharin discusses the relation between theory and practice both from an epistemological and from a sociological point of view. On the first account he emphasized that knowledge comes not from pure observation but from intervention in the world – which may not go beyond what he cites from Marx, Engels, and Lenin though certainly beyond what his audience knew about what these authors had said, and which in any case had to wait for Mary Hesse and Thomas Kuhn before it was accepted outside Marxist circles. On the second account – the one that is relevant for our present purpose – he emphasized the complexity and historically conditioned mutability of the relation between knowledge and practice, as well as the changing ways in which different types of knowledge are distributed between carriers with different social roles.

15 I do not remember Neugebauer to have employed the expression; in Neugebauer 1934, 125 n. 1 he rejects the notion of 'mathematicians' very explicitly with reference to Egypt; but it was used by Thureau-Dangin – e.g., Thureau-Dangin 1938, xxxviii – and afterwards by various other authors, although most would speak simply of 'the Babylonians'.

16 The Soviet contributions, printed already at the moment, were reprinted in 1971 as *Science at the Cross Roads* (Bukharin et al. 1971).

17 See Graham 1993, 143–151.

18 Graham 1993, 141.

As alluded to, Bukharin's subtleties proved too subtle for the Western audience, and had no impact.[19] Even Joseph Needham, who later was to make the non-trivial interplay between 'clerks and craftsmen' a favorite theme of his, only saw Bukharin's paper as "in its way a classical statement of the Marxist position".[20] Needham instead received his impulse from the second of the above-mentioned events: Zilsel's paper on "The Sociological Roots of Science"[21] (as well as other papers by the same author).

Recent work on Zilsel's *Nachlaß*[22] shows that this and other papers of his from the same period belong within a larger metatheoretical project that never materialized as such. As it stands and on its own, the paper argues that the discussion about *the* root of the new science of the late sixteenth and the seventeenth century – whether scholastic thought, Humanism, or the knowledge of engineers like Leonardo da Vinci – is mistaken, since it was *the interplay* between natural philosophers belonging to the scholastic tradition, trained Humanists, and 'higher artisans' that made possible the breakthrough.

Needham was not the only historian *of science* to be impressed by Zilsel's paper, which (like Hessen's article) indeed called forth a number of other publications either taking up the thesis or explicitly arguing against it. Strangely, however, no historian of mathematics seems to have addressed the questions whether Zilsel's thesis might apply *mutatis mutandis* to the revolution in early Modern mathematics.[23] Initially this non-reaction was perhaps not so strange – at the time, and for long, historians of mathematics saw in the most important group of 'higher artisans' of relevance for the question (the Italian abbacus masters) nothing but not very competent *vulgarisateurs* of Leonardo Fibonacci (if they happened to know at all about their existence); ascribing to such people a stimulating influence was more than could be expected from historians concentrating on the mathematics of (great) mathematicians.[24]

19 They may also have been too subtle for his fellow-countrymen, but until Bukharin's rehabilitation in 1988 these had other reasons not to get too close. For decades, the points of view expressed by the Soviet delegation at the London Congress could only be discussed in the Soviet Union as filtered through Bernal's not very sophisticated reception.

20 Bukharin et al. 1971, ix.

21 Zilsel 1942.

22 Raven and Krohn 2000.

23 At least not before I organized an international workshop on the theme "'Higher artisans', Humanism and the University Tradition. The Zilsel thesis reconsidered in relation to the Renaissance transformation of mathematics" in 1998 – but even then it did not really happen (Paolo Rossi, who would probably have understood, was forced to cancel his participation). In consequence, I had to take up the theme on my own in Høyrup 2011.

24 Karpinski's closing commentary to Jacopo da Firenze's abbacus treatise, though preceding Zilsel's paper, is characteristic of the attitude that prevailed afterwards (Karpinski 1929, 177): "[the early fourteenth-century] treatise by Jacob of Florence, like the similar [late fifteenth-century] arithmetic of Calandri, marks little advance on the arithmetic and algebra of Leonard of Pisa. The work indicates the type of problems which continued current in Italy during the thirteenth to the fifteenth and even sixteenth centuries, stimulating abler students than this Jacob to researches which bore fruit in the sixteenth century in the achievements of Scipione del Ferro, Ferrari, Tartaglia, Cardan and Bombelli." As we see, Fibonacci, Jacopo, Calandri, and Bombelli are supposed to belong on *the same* branch, although part of it has undergone some degeneration.

No 'event' but a process has been the increasing awareness within the history of technology that pre-nineteenth-century technical knowledge, including knowledge leading to technical innovation, cannot be adequately described as 'applied science'. Even this process has left fewer traces in the historiography of mathematics than it should perhaps have done.

## 4 'Popular' or 'sub-scientific'

In spite of the invitations of Bukharin and Zilsel it thus remained common, to the extent the mathematics of medieval and other pre-Modern practitioners was at all taken into account and seen as a different body than that of the 'scientific' traditions, to characterize it as 'popular' or 'folk'. I still did so myself in in my contribution to the Sarton Centennial Conference[25] when discussing the roots for those aspects of the Islamic mathematical corpus which lexicographers like al-Nadīm do not trace to the Greeks but treat as anonymous traditions or fail to mention.

But evidently neither the use of the 'Hindu numerals' nor trigonometry were known at the time by 'people' in general; these kinds of supposedly 'popular' knowledge were carried by narrow social groups and thus certainly constituted *specialists' knowledge*. In consequence I began speaking of these sources and the traditions to which they belonged as 'sub-scientific', first in passing,[26] then more analytically.[27] On occasion of Bukharin's centennial I elaborated this discussion,[28] emphasizing the oral cultural type of the carrying environment and pointing (i) to the function of (what has come to be misnamed) 'recreational problems'[29] as 'neck riddles' that display appurtenance to a particular craft carrying a particular body of know-how, (ii) to the possibility to use these problems (as eventually adopted into cultures leaving written sources, thereby becoming properly 'recreational') as index fossils allowing us to trace an oral culture that in the nature of things is not directly documented in writing.[30]

In this paper I still used the term 'sub-scientific' about scribal as well as non-literate practitioners' mathematics, singling out the former type as nothing but a sub-category. Schools – even pre-Modern schools teaching practical mathematics – certainly vary in

25 Høyrup 1984.
26 Høyrup 1986.
27 Høyrup 1987.
28 Published as Høyrup 1990b.
29 More precisely: the problems become 'recreational' when adopted into literate culture; the term is only a misnomer in relation to their original function.
30 Also in the later 1980s, David King investigated the astronomy of Islamic legal scholars and pointed out that it was distinct from the astronomy of mathematicians. He used the term 'folk astronomy' but left no doubt that it was the astronomy of the 'craft' of legal scholars. See the papers contained in King 1993.

character, and can be argued to constitute a pluri-dimensional continuum merging gradually into oral apprenticeship teaching on one side; but it is also difficult, even in several pre-Modern settings, to make a totally clean cut between schools teaching for practice and schools teaching 'scientific' mathematics.[31] I would therefore now distinguish between the *sub-scientific* knowledge type, carried by practitioners taught in an apprenticeship network; the *'scholasticized'* or *scribal* practitioners' knowledge type, communicated in a school by masters whose own genuine practice is that of teaching, not the practical use of the knowledge they teach; and the *'scientific'* or *theory-oriented* type, the one to which historians of mathematics have dedicated most of their efforts – keeping in mind that these are fuzzy categories understood through ideal types functioning as navigational guides rather than classificatory boxes.[32]

## 5 Applications of the categorization

Networks of categories constitute an instance of formal knowledge (albeit of the most primitive kind). Their utility thus depends on their ability to create order in the tangle of real-world phenomena – those from which they were derived in the first instance through a process of abstraction (that should be the easier but still the obvious first test) as well as others that did not intervene when they were constructed (not necessarily quite as easy). I shall look at one instance of each kind.

When speaking for the first time of a 'sub-scientific tradition' in 1986 I referred to the tradition that linked Old Babylonian 'algebra' to the area riddles in Abū Bakr's *Liber mensurationum*. Some years later,[33] I also voiced a suspicion that the problem BM 13901 #23 (dealing with a square, for which the sum of *the four* sides and the area is given) was "a surveyors' recreational problem, maybe from a tradition that was older than – perhaps even a source for – Old Babylonian scribal school 'algebra'"; I also observed the family resemblance of the configuration used in the solution with one of al-Khwārizmī's proofs. However, by then I had to leave both matters there.

Over the following years, being alerted to the stylistic peculiarities that might characterize fresh borrowings from an oral tradition as well as to those that should correspond to transmission within a stable school environment (and being in general stimulated to be sensitive to stylistic detail and not only to so-called 'mathematical substance')

31 See, for late Greco-Roman Antiquity, Cuomo 2000.

32 Cf. Høyrup 1997. It might be useful to distinguish a fourth type, the 'deuteronomic' teaching of theory petrified into and taught in school as a dignified tradition – the shape in which most of the students taught scientific mathematics have encountered their Euclid since two thousand years; cf. Netz 1998. But since my topic is the relation between mathematical practice and mathematical theory I shall not pursue this theme at present.

33 Høyrup 1990a, 275.

I was able (that is at least my own opinion) to put on a firmer footing than done before the claim that Old Babylonian 'algebra' and Euclidean 'geometric algebra' (both 'so-called') were connected, and to demonstrate also that the geometric riddles of Arabic *misāḥa* treatises as well as al-Khwārizmī's geometric proofs for the basic *al-jabr* procedures belonged within the same network. Moreover I could argue (still of course in my own opinion) that the Old Babylonian 'algebraic' school discipline built upon original borrowings from the neck riddles of a lay surveyors' environment, and that this environment and its riddles, not the tradition of scholar-scribes, was responsible for the transmission of the inspiration to later times.

Since I have described this analysis and its outcome at length elsewhere,[34] I shall not go into further detail, but turn instead to a historical phase which I started looking seriously at some fifteen years ago: the Italian abbacus school of the late Middle Ages and the Renaissance, and its relation to Leonardo Fibonacci.

Karpinski, who was one of the first to describe the stylistic peculiarities of an abbacus treatise (Jacopo da Firenze's above-mentioned *Tractatus algorismi* from 1307, in Tuscan in spite of the Latin title, and written in Montpellier), though quite aware of its deviations from what can be read in Fibonacci's *Liber abbaci*, still appraised its contents as if it was only a station on the road from Fibonacci to Scipione del Ferro (see note 24). At the moment little systematic work had been done on abbacus material,[35] but things did not change even when Gino Arrighi and his pupils had published an appreciable number of manuscripts. Wholly in Karpinski's vein, Kurt Vogel stated that Cowley's description of the Columbia ms X 511 A1 3 had been important because it "filled a lacuna between Leonardo da Pisa's *Liber abbaci* and Luca Pacioli's *Summa*".[36] Even sharper are the formulations of those who have worked most intensely on the material – thus Warren Van Egmond, according to whom all abbacus writings "can be regarded as [...] direct descendants of Leonardo's book";[37] and Raffaella Franci and Laura Toti Rigatelli, according to whom "the abacus schools had risen to vulgarize, among the merchants, Leonardo's mathematical works".[38] More recently, Elisabetta Ulivi – probably the scholar who has worked most in depth on the social history of the abbacus environment – has expressed the view that the abbacus treatises "were written in the vernaculars of the various regions, often in Tuscan vernacular, taking as their models the two important works of Leonardo Pisano, the *Liber abaci* and the *Practica geometriae*".[39]

34 Most extensively in Høyrup 2001 and Høyrup 2002, 362–417.

35 Karpinski (1910/1911) describes another abbacus algebra, and Cowley (1923) analyzes a whole treatise. During the nineteenth century a number of excerpts had been published by Libri, Boncompagni, and others, but no coherent descriptions of whole treatises (nor *a fortiori* of the category as such) had appeared.

36 Vogel 1977, 3.

37 Van Egmond 1980, 7.

38 Franci and Toti Rigatelli 1985, 28.

39 Ulivi 2002, 10. Similarly in more recent publications.

All of these, I would claim, have fallen victims to the 'syndrome of The Great Book', the conviction that every intellectual current has to descend from a *Great Book* that is *known to us* at least by name and fame – the same conviction that made those who objected to Neugebauer's proposed transmission observe that no Greek would have bothered to read the Babylonian clay tablets, and induced many of those who have discussed the possible borrowing of Indian material into Arabic algebra to believe that inspiration had to come from the writings of an Āryabhaṭa or a Brahmagupta.

Already Karpinski had noticed that Jacopo's algebra has no problems in common with the *Liber abbaci*. Reading of the whole treatise shows it to have no single problem, algebraic or otherwise, in common with the Great Book, but to contain on the other hand numerous problems belonging to classes that are also present in that Book.[40] Some of these belong to the cluster of problems that are found in ancient and medieval sources "from Ireland to India", as Stith Thompson says about the 'European folktale'[41] – and even in the Chinese *Nine Chapters*. This cluster of problems usually going together was apparently carried by the community of merchants traveling along the Silk Road[42] and adopted as 'recreational problems' by the literate in many places; it is thus a good example of a body of sub-scientific knowledge influencing school knowledge systems in many places and an illustration of the principle that it is impossible to trace the 'source' for a particular trick or problem in a situation where "the ground was wet everywhere".[43]

Other problem types are shared with Fibonacci but not diffused within the larger area (or diffused within a different larger area that may coincide with the Arabic network of sea trade from the Indian Ocean to the Mediterranean). Moreover, Jacopo employs a range of set phrases ("et così se fanno tucte le simile ragioni", "se ci fosse data alcuna ragione", etc.) that also turn up copiously in other abbacus writings as well as in similar writings from the Provençal-Catalan and the Castilian area[44] – and also, but on so rare occasions that they seem to represent slips, in Fibonacci's text.

A slightly earlier Umbrian abbacus treatise (Florence, Riccardiana ms. 2404, from c. 1290)[45] claims in its title to be "according to the opinion" of Fibonacci. Analysis of the text shows this claim to be misleading.[46] Everything basic in the treatise is as different from what we find in the *Liber abbaci* as is Jacopo's *Tractatus* (and characterized by the presence of the same set phrases); but the writer borrows a number of sophisticated problems from Fibonacci, often demonstrably without understanding even as

40 Cf. Høyrup 2007, which contains an edition and English translation of the work.

41 Thompson 1946, 13.

42 Some of the traveling problems deal precisely with bits of this web of caravan and sea routes extending from China to Cadiz, and no other network (however open-ended) existed that ranged so widely.

43 Høyrup 1987, 290.

44 See Sesiano 1984, a description of the Pamiers algorism; Malet 1998, an edition of Francesc Santcliment's *Summa de l'art d'aritmetica*; and Caunedo del Potro and Córdoba de la Llave 2000, an edition of the Castilian *Arte del alguarismo*.

45 Arrighi 1989.

46 Høyrup 2005.

much as the notation of his source. Obviously, Fibonacci had already become a kind of culture hero (modern historians are not the first to fall victims to the syndrome of The Great Book), and the borrowings serve as embellishment beyond the ordinary teaching matters.

From combination of these pieces of evidence it becomes obvious that Jacopo's as well as the Umbrian treatise refer to an environment spread out in all probability over much of the Romance-speaking Mediterranean region, already in possession of elementary vernacular literacy and probably based in some kind of school teaching similar to the Italian abbacus school but with at most tenuous ties to the world of university scholars. It also becomes clear that already Fibonacci had drawn part of his inspiration for the *Liber abbaci* from this environment, whose existence thus antedates 1200 (or at the very least 1228).

Analysis of Jacopo's algebra chapter and comparison with Arabic algebraic writings suggests that it is ultimately drawn from another level of Arabic algebra than that of the Great Books of al-Khwārizmī, Abū Kāmil, Ibn al-Bannāʾ, etc. It seems likely – but for the time being cannot be conclusively established – that the just-mentioned school environment was not restricted to the Romance-speaking area but also reached into (and probably came from) a similar environment in the Arabic Mediterranean teaching *muʿāmalāt*-mathematics (even Arabic merchants must have learned their mathematics somewhere, including the use of the rule of three to which already al-Khwārizmī had dedicated the "Chapter on *muʿāmalāt*" of his *Algebra*.[47] That school in Bejaïa in which Fibonacci tells to have spent "some days" learning the *studium abbaci*[48] is likely to have been such a school (the alternative, a mosque school, is not plausible).[49]

Though in all probability a descendant of a school environment that had inspired both Fibonacci and Jacopo, the mature Italian abbacus school of the fourteenth and fifteenth century developed characteristics that are not likely to have been present before 1310 – characteristics that appear to have depended on the market competition between abbacus masters for jobs and pupils. Both the Umbrian abbacus and Jacopo's treatise make mathematical mistakes from time to time – but they abstain from mathematical fraud. Already within the first two decades after Jacopo's writing of (what is in all probability) the first Italian vernacular algebra, on the other hand, abbacus treatises

47 Ed. Rosen 1831, Arabic 48.

48 Ed. Boncompagni 1857, 1.

49 Some of the formulations in Jacopo's discussion of metrologies are strikingly similar to what we find in Aḥmad ibn Thabāt's *Reckoners' Wealth* from c. 1200 (*Ġunyat al-ḥussāb*, ed. Rebstock 1993), which however both surpasses what it would be reasonable to teach to practical reckoners (e.g., Euclidean geometric definitions) and offers too little training for these; but ibn Thabāt was a scholar who taught law as well as *ḥadīth* and *ʿilm al-ḥisāb* at the Niẓāmīya madrasah (Rebstock 1993, x), and thus wrote a scholarly book about practitioners' mathematics, no textbook for the training of merchant youth. Apart from his own intellectual pleasure, he may have been motivated by what (for instance) a judge had to understand about all domains of practical computation.

begin to present blatantly false rules for irreducible equations of the third and fourth degree – not easily unmasked by competitors, however, because the examples are always chosen so as to lead to 'solutions' containing radicals. Only at a moment when abbacus-trained writers like Luca Pacioli began moving on the interface between the Humanist-courtly and the scholastic-scholarly areas[50] was the fraud exposed – and only then was there space for del Ferro's genuine solution to contribute to the revolution in mathematics (in good agreement with the Zilsel thesis, we might say).

Italian abbacus mathematics is thus not to be understood as an activity bridging one Great Book (the *Liber abbaci*) and another one (e.g., Cardano's *Ars magna*) but as a distinct undertaking, carried neither by scholarly mathematicians nor by a purely oral culture, yet having most of its ultimate roots in an environment close to the latter type, and giving eventually important stimuli to the further development of scientific mathematics. I shall permit myself to claim that the categorization suggested above is fruitful in opening our eyes to evidence in the sources that has so far been overlooked, and thus allows us to attain better understanding of the real historical process. At the same time the example demonstrates that a seemingly simple category ('schools') covers phenomena of widely different character, held together mainly by being neither orally based nor 'scientific' in ambition.[51]

50 That Luca moved in this zone is quite obvious, e.g., both from his preface to the *De divina proportione* (ed. trans. Winterberg 1889, 17–35) and from his publication of the Campanus version of the *Elements*.

51 This point could be sharpened if the abbacus school were contrasted, e.g., with the Old Babylonian scribe school, which eliminated mathematical fraud (namely, mock solutions) from its sub-scientific heritage. Analysis of what happens to a specific problem type, e.g., the 'hundred fowls,' might highlight the difference between the genuinely sub-scientific and the abbacus-school style.

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Wayne Horowitz and John M. Steele

# A Mysterious Circular Tablet with Numbers and Stars


## Summary

In this paper we publish a unique circular cuneiform tablet which is divided into twelve sectors each of which contains numbers and star names. Our analysis of the text suggests that it contains astrological material related to the so-called *Kalendertext* scheme.


Keywords: Circular tablets; astrology; star names; Mesopotamia; Sumerian.

In diesem Beitrag publizieren wir eine einzigartige kreisförmige Keilschrifttafel, die in zwölf Sektoren eingeteilt ist, von denen jeder Zahlen und Sternnamen enthält. Die Textanalyse legt nahe, dass es sich um astrologisches Material handelt, das mit dem sogenannten Kalendertext-Schema in Verbindung steht.

Keywords: Kreisförmige Tontafeln; Astrologie; Sternnamen; Mesopotamien; sumerisch.

BM 47762 is published by permission of the Trustees of the British Museum.

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## 1 Introduction

BM 47762 preserves approximately one fourth of a round tablet with numerous numerals and a small number of star-names. The tablet attracted the attention of Wayne Horowitz during his search for exemplars of Astrolabes, including those in planisphere form of the type CT 33 11–12.[1] Horowitz quickly realized that the tablet had nothing to do with Astrolabes and passed the text on to John Steele for further investigation. In the following, we present a preliminary study of the text in the hope that others will be able to offer a fuller explanation of the mathematical and astronomical/astrological schemes at play on this unique member of the cuneiform corpus.

Fig. 1 BM 47762 obverse.


Fig. 2 BM 47762 reverse.

1 For other circular astronomical texts, see Horowitz and Al-Rawi 2001.

## 2 The tablet

The circular tablet fragment BM 47762 = 81-11-03, 467 (Figs. 1 and 2), most likely from Babylon, presents a set of numbers and star-names which place this text in the realm of astronomy or astrology, and so the area of expertise of our friend and colleague Lis Brack-Bernsen. What is almost certainly the obverse is divided into sectors, each containing several lines of cuneiform text (the reverse is uninscribed except for unreadable traces of a few wedges towards the center of the tablet). The surviving piece gives at least part of four out of an original 12 sectors. A small piece of the outer edge of the tablet survives in the third section from the left, with part of a circular incision that apparently served as an outer border of the text when complete. Given the format of the tablet, sectors in the shape of 'pie-slices', the available space in each line diminishes as one moves towards the center of the circle. This enables us to determine how many lines are missing from each of the other less well preserved sectors. The actual center of the circle is missing, but it is likely that the very center of the circle was left vacant. In fact, if the tablet was more complete we would probably find the impression of a compass point at the center given that the arc of the surviving piece of the border at the edge of the tablet is so well drawn. This is also the case for the dividing lines between the sectors, suggesting work with an ancient 'compass and ruler'. In contrast, the scribe struggles somewhat with the problem of rendering cuneiform in his circular format leaving some signs squeezed, or misshapen. As on all previously known circular astronomical/astrological tablets the text is meant to be read from outside to inside. The obverse reads as follows:

| | Sector IX | Sector X | Sector XI | Sector XII |
|---|---|---|---|---|
| (1) | [x x x x] | [x x x x] | GU.LA 1 ⸢28⸣ | [x x x x] |
| (2) | [x x x x] | [x x x x] | 2 28 11 7 | 3 [x x x] |
| (3) | [x x x x] | [x]⸢7 10 14⸣ | 2 7 11 14 | 3 7 1[2 x] |
| (4) | [x x x]⸢21⸣ | 1 14 10 21 | 2 14 11 21 | 3 14 1[2 x] |
| (5) | [x x x 2]⸢8⸣ | 1 21 10 28 | 2 21 11 28 | 3 21 12 [x] |
| (6) | [x x x x] | 10 21 SAG.DU RÍN | 11 21 ⟨ŠÚ⟩ SAG.DU | 12 21 SA[G.DU] |
| (6a)[2] | | | GÍR.TAB | ŠUL.[PA.È?[3]] |

2 (Transliteration, L. 6a) This line exists in Sector XI–XII (see the commentary).

3 (Translit., L. 6a, Sector XII) We expect a reference to Sagittarius here. Most likely the scribe intended

| | | | | |
|---|---|---|---|---|
| (7) | [x x x] | ˹2˺ 8 ÙZ | 28 Á BIR | 28 ŠUL.[PA.È] |
| (8) | [x x x] | [7 IG]I GIŠ.DA | 7 IGI BIR | 7 IGI [x (x)] |
| (9) | [x x] | [14 S]AG | ˹1˺4 [SAG] | 14 SAG |
| (10) | [x x] | [x x] | x BI$^{?}$[4] | [x x] |

## 3 Commentary

The text has a rigid format with each sector containing ten entries. Each entry is written on a separate line except in the case of line 6 where in sectors XI and XII the scribe ran out of room and so continued onto an indented line 6a. In the following, we treat lines 6 and 6a together. The structure of the text allows most of the four partially preserved sectors to be restored with certainty. Below we give a translation of the text including restored text.

| | Sector IX | Sector X | Sector XI | Sector XII |
|---|---|---|---|---|
| (1) | [Sagittarius 11 28] | [Capricorn 12 28] | Aquarius 1 28 | [Pisces 2 28] |
| (2) | [12 28 9 7] | [1 28 10 7] | 2 28 11 7 | 3 [28 12 7] |
| (3) | [12 7 9 14] | [1] 7 10 14 | 2 7 11 14 | 3 7 1[2 14] |
| (4) | [12 14 9] 21 | 1 14 10 21 | 2 14 11 21 | 3 14 1[2 21] |
| (5) | [12 21 9 2]8 | 1 21 10 28 | 2 21 11 28 | 3 21 12 [28] |
| (6), (6a) | [9 21 beginning of Virgo] | 10 21 beginning of Libra | 11 21 beginning of Scorpio | 12 21 begin[ning] of Sagitt[arius$^{!}$] |
| (7) | [28 […]] | 28 The She Goat | 28 […] The Kidney | 28 Šulpae |
| (8) | [7 in front of […]] | [7 in front of] Jaw of the Bull | 7 in front of The Kidney | 7 [in front of […]] |

to write PA.BIL.SAG but instead wrote ŠUL.PA.È, which also contains the PA sign and which appears in the following line. An error of this kind suggests that the scribe was copying another tablet.

4 (Translit., L. 10, Sector XI) BI or a more complex sign which ends with a BI-type element.

| | | | | |
|---|---|---|---|---|
| (9) | [14 beginning] | [14 be]ginning | 14 beginning | 14 [beginning] |
| (10) | […] | […] | […] […] | […] |

The preserved entry in line 1 of Sector XI indicates that each of the twelve sectors concerned one sign of the zodiac and demonstrates that the preserved portion of the tablet contains the final four sectors. Each entry in lines 2–5 contains a series of four numbers in the following sequence, with variables $a$ and $b$ as the first and third numeral in each line:

| | | | |
|---|---|---|---|
| $a$ | 28 | $b$ | 7 |
| $a$ | 7 | $b$ | 14 |
| $a$ | 14 | $b$ | 21 |
| $a$ | 21 | $b$ | 28 |

In each case, the variables $a$ and $b$ are each less than or equal to 12, and both $a$ and $b$ increase by 1 (moduli 12) from one sector to the next. Furthermore, $b$ is always equal to the number of the zodiacal sign (where Aries = 1, Taurus = 2, etc., up to Pisces = 12). The use of numbers to refer to signs of the zodiac (and months in the ideals 360-day calendar) is found in astrological texts which use the so-called *Dodecatemoria* and *Kalendertext* schemes.[5] Entries from these schemes are usually written out in the form of four numbers (for example, 1 13 1 1) where the first and second numbers, and the third and fourth numbers, are to be understood as either a position given with a sign of the zodiac, and degree or a date in the schematic 360-day calendar given with the month and day. This parallel suggests that in our text, the four numbers in each of lines 2–5 are also to be understood as two pairs of either month and day, or zodiacal sign and degree. The agreement between the number $b$ and the sign of the zodiac found in line 1 seems to confirm this interpretation of these numbers. The relation between the two pairs of numbers in each entry does not follow either the *Dodecatemoria* or *Kalendertext* schemes, however. In those schemes, the first pair of numbers should increase by 13 degrees or 277 degrees respectively for each increase of 1 degree in the second pair of numbers, which results in a mapping of every position in the second pair onto a distinct position in the first pair. The position generated by either scheme does not agree with what we find here. Nevertheless, there does appear to be a connection between the numbers in lines 3–5 and the *Kalendertext* scheme: if we take both $a$ and $b$ to be zodiacal signs, then the number of degrees between the first pair of numbers and the second pair of numbers is equal to 277, which is the characteristic number of the *Kalendertext* scheme. However, the entry in line 2 does not follow this same rule. It seems likely, therefore, that the text

5 For an overview of these schemes, see Brack-Bernsen and Steele 2004.

contains some type of astrological material which is related to the *Kalendertext* scheme in some way, but which also exploits the pattern of numbers 7-14-21-28, which perhaps corresponds to the phases of the moon.

Line 6 begins with the number of the zodiacal sign for the sector followed by the number 21 and the statement 'beginning of' (literally 'head of') another zodiacal sign which is nine signs further on from the sign of the sector. An interval of nine signs may again relate to the *Kalendertext* scheme as $277 = 9$ signs $+ 7$ degrees.

Lines 7–9 (and probably line 10) each begin with a number from the 7-14-21-28 sequence followed by a star name preceded in line 8 by the term IGI, meaning either 'in front of' or 'visible', and in line 9 by the term SAG, meaning 'at the beginning of'. The stars given in line 7 are all known to be used as substitute names for planets in certain omen texts: The She Goat for Venus, The Kidney for Mercury, and Šulpae for Jupiter.[6] We think it likely, therefore, that line 7 should be interpreted as referring to the planets. Beyond that, however, we do not understand the relationships between the numbers, the star names, and the terms IGI and SAG in these lines.

This is as far as we can go in understanding our text.[7] We present this text to Lis in the hope that she will enjoy our exposition of this 'mathemagical' text and be able to build upon our analysis to provide a fuller explanation of the text and its place in the cuneiform astronomical-astrological corpus.

6 Reiner 2004.

7 At the Regensburg IV conference in Berlin, Jeanette Fincke presented a second fragment of this text and proposed an alternative interpretation of its contents. We refer the reader to her forthcoming study for further details.

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**1–2** Courtesy of the Trustees of the British Museum.

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Francesca Rochberg

# An Ancient Celestial Empire of Benevolent Knowledge


## Summary

This paper deals with the concept of anomaly in cuneiform knowledge, looking mainly at the principles of divinatory texts concerning norms and anomalies in ominous signs. I consider here one way in which the system of divinatory knowledge was consistent with early Babylonian approaches to knowledge of the heavens.


Keywords: Anomaly; sign; monster; norm; ideal.

Das Konzept der Anomalie in keilschriftlichem Wissen wird in diesem Beitrag behandelt. Untersucht werden in erster Linie die Prinzipien jener divinatorischen Texte, die sich mit Normen und Anomalien in unheilverkündenden Zeichen befassen. Auf diesem Weg prüfe ich, inwiefern das System divinatorischen Wissens mit frühen babylonischen Herangehensweisen an astronomisches Wissen übereinstimmte.

Keywords: Anomalie; Zeichen; Monster; Norm; Ideal.

This essay represents part of a chapter from my book *Before Nature: Cuneiform Knowledge and the History of Science* (Chicago: University of Chicago Press, 2016).

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It is a distinct honor to contribute to a volume for Lis Brack-Bernsen, who has advanced the understanding of Babylonian astronomy with her ground-breaking methodology to explicate how and why the Babylonians observed the lunar horizon phenomena around opposition that we call the Lunar Four. Focusing upon the use of those observational data to construct the period of the moon in velocity and in latitude, she showed that col. Φ of the Babylonian System A lunar ephemeris, the column that advances by the Saros period (223 months) line-by-line, is an argument of lunar anomaly. This paper deals with anomaly from a quite different standpoint. Looking mainly at the principles of divinatory texts concerning norms and anomalies in ominous signs, I reflect here on one way in which the system of divinatory knowledge was consistent with early Babylonian approaches to knowledge of the heavens.

In the preface to *The Order of Things: An Archaeology of the Human Sciences*, Michel Foucault referred to the classification system of the fictive Chinese encyclopedia that Jorge Luis Borges entitled the "Celestial Empire of Benevolent Knowledge", where animals are classified as

> (a) belonging to the emperor, (b) embalmed, (c) tame, (d) sucking pigs, (e) sirens, (f) fabulous, (g) stray dogs, (h) included in the present classification, (i) frenzied, (j) innumerable, (k) drawn with a very fine camelhair brush, (l) et cetera, (m) having just broken the water pitcher, (n) that from a long way off look like flies.[1]

Foucault said,

> In the wonderment of this taxonomy, the thing we apprehend in one great leap, the thing that, by means of the fable, is demonstrated as the exotic charm of another system of thought, is the limitation of our own, the stark impossibility of thinking *that*. But what is it impossible to think, and what kind of impossibility are we faced with here?[2]

Zhang Longxi critiqued Foucault's analysis of Borges' fabulous taxonomy on the grounds that Foucault did not realize

> that the hilarious passage from that 'Chinese encyclopedia' may have been made up to represent a Western fantasy of the Other, and that the illogical way of sorting out animals in that passage can be as alien to the Chinese mind as it is to the Western. [...] In fact, the monstrous unreason and its alarming subversion of Western thinking, the unfamiliar and alien space of China as the image

1 Borges 1964, 103.

2 Foucault 1973, xv. Emphasis in the original.

> of the Other threatening to break up ordered surfaces and logical categories, all turn out to be, in the most literal sense, a Western fiction.[3]

Certainly one of the striking features of Borges' fictive list of categories is the way in which it seems to focus on something that will not submit to the order of nature, but rather has been given its own orientation to 'something else'. And therein lies its strangeness. On the other hand, the classifications inherent in Sumerian, Sumero-Akkadian, and Assyro-Babylonian lexical and divinatory texts are not fictive, but historically real. The criteria for classification and making connections between elements of various categories found in those texts, because they do not reduce to a desire to know and classify nature, can have a similar effect.

Unlike China, ancient Babylonia and Assyria have not played the role of the Other in the Western imagination so much as they have been conscripted into the role of precursors of Ourselves, of Western civilization. And yet when it comes to the analysis of cuneiform corpora of knowledge, where the intellectual history of the ancient Near East merges with the beginnings of Western science, we find ourselves confronted with classifications and categories, even phenomena, that sometimes confound our own sense of the order of nature. Still, as conceived in the cuneiform world, the overriding goal of the observation and interpretation of phenomena was to establish norms and anomalies by means of which to find the order of things.

## 1 Categories of signs

In various compendia of ominous signs, phenomena are organized into a great many categories. The organization of signs is sometimes such that a sign will be 'seen' that cannot occur in the world. Moreover, the consequent of a sign, including one that cannot occur, does not point to co-occurring events in the perceived world, but to associations based on a hermeneutic code. This makes for a complex set of references from which to reconstruct what it was that interested the scribes about the perceived world. The basic impetus for detailed and systematic observation of the world was divination from ominous signs.

James Allen pointed to the essential fact that

> Our term 'sign' comes, of course, straight from the Latin *signum*, which in turn renders the Greek σημεῖον, whose range of uses it tracks pretty closely. Not only the term, but the idea or complex of ideas for which it stands are an inheritance from Greco-Roman antiquity.[4]

3 Zhang Longxi 1998, 21.

4 Allen 2010, 29.

The persistent preoccupation with phenomena as signs continues on into later antique and mediaeval science as well, with descendants in Western European, Eastern Byzantine and Islamic traditions, not to mention Indian science. Peter Harrison, in a striking statement, said that

> For virtually the first fifteen hundred years of the common era the study of natural objects took place within the humanities, as part of an all-encompassing science of interpretation which sought to expound the meanings of words and things.[5]

Divination and astrology found a central place among various ancient and medieval cultures of knowledge, both from the point of view of prognostication as well as of the philosophy of inference-making from signs.

Ancient cuneiform knowledge of what Harrison referred to as "natural objects", roughly for the fifteen hundred years before the Common Era, constitutes very much the same thing that he identified for the first fifteen hundred years of the Common Era, i.e., it was "part of an all-encompassing science of interpretation which sought to expound the meanings of words and things." This is represented in the cuneiform tradition of knowledge in the overwhelming focus by the scribes on the systematic and interpretive science of divination from signs. The principal qualification must be in designating the objects of this knowledge not as 'natural objects', but as observed, imagined, and conceived objects in relation to physical as well as imagined things, and for the focus not on observation of the signs alone, but on their interpretation according to systematic codes embodied in textual compendia (some might argue it was really one code with particular variants for different domains of phenomena, say, exta, or births, or the stars and planets). Both the idea of a sign and the hermeneutics of the texts together constituted the science of signs in the culture of cuneiform knowledge.

In the West, the Hellenistic period saw a new focus on signs from astronomy to philosophy. Already in the 3rd century BCE Aratus began his poetic star catalog, the *Phaenomena* (lines 5–6), as Katharina Volk noted, by reference to Zeus as giver of "propitious signs to humans",[6] thus framing the composition in terms of signs. Volk further explained,

> In addition to announcing the poem's topic, this proem neatly states the *Phaenomena*'s conception of the world as a cosmos full of benevolent signs from an omnipresent god who has the welfare of human beings at heart. […] The idea of the sign is central to the *Phaenomena*, as is apparent from the fact that forms of the noun σῆμα (*sēma*, pl. *sēmata*) 'sign' appear 47 times in the course of the

5 Harrison 1998, 9.

6 Volk 2010, 200. Cf. also Netz 2009, 186–187.

> poem, those of the verb (ἐπι)σημαίνω 'to signal' 11 times. The [...] repetition of these and similar keywords [...] drives home the message that Aratus is *not interested in natural phenomena* (e.g., the constellations) as such, but only in as much as they are part of the cosmic system of signs that has its origin in the benevolence of Zeus.[7]

Despite the cultural track running from Rome to Greece and further on to the ancient Near East, many particulars of ancient Greek (or Roman, or medieval) ideas about signs differ from those of cuneiform texts. The ubiquity of the importance of signs throughout the cultural worlds of the ancient Near East and Mediterranean should not be mistaken for a thoroughgoing similarity or unity in how signs were understood from one cultural milieu to another.[8] What a sign was in the cuneiform context, even as it may well have changed over the millennia-long span of the tradition, reflects nonetheless within bounds an Assyro-Babylonian way of seeing the world where the portentous, the anomalous, and the prodigious differed, however subtly, from the preternatural, the monstrous, or miraculous in the worlds of later Hellenistic (Greek, Greco-Roman, Greco-Egyptian, Indian) diviners, (Platonist or Christian) theologians or Late Antique and medieval natural philosophers who were interested in these things.

In particular, once God and nature entered into the matrix of ideas that defined the world, signs would begin to signify specifically in terms of that matrix, in which sometimes there was an equivalence of God and nature, sometimes a tension between Divine will and the laws of nature. Consider the statement of Augustine, where God's will works within nature for His purposes:

> So, just as it was not impossible for God to set in being natures according to his will, so it is afterwards not impossible for him to change those natures which he has set in being, in whatever way he chooses. Hence the enormous crop of marvels, which we call 'monsters,' 'signs,' 'portents,' or 'prodigies.'[9]

The explanatory rhetoric of God and nature, or natures, is evidence of a new conceptual foundation for prognostication through signs, and for science, differentiating it from anything that developed in the Hellenic cultural realm, and certainly from that of the ancient Near East.

In the most general of terms, signs are communicative. They point to things beyond themselves, conveying information in a multiplicity of ways, as is readily seen in the fourteen meanings of 'sign' in the *Oxford English Dictionary*. Signs can be read and

7 Volk 2010, 200–201; my emphasis and ellipses.

8 Beerden (2013) looks at some of the differences in how divinatory signs functioned in Mesopotamia, Greece, and Rome, focusing on the textual (cuneiform) or non-textual (Greek) nature of divination, the institutional settings of the diviner.

9 Augustine, *City of God*, cited from Daston and Park 2011 [1998], 40.

understood, or variously interpreted. Signs can be linguistic and orthographic, and thus can themselves constitute a form of writing, literally (cuneiform or any other script) or figuratively (the liver, the stars and planets, or the Book of Nature). Like its English counterpart, Akkadian *ittu* had a range of meaning from 'mark, feature, characteristic', or even 'diagram', to 'omen', 'password, signal, notice, acknowledgment', and 'written proof'.[10] Signs entered into the Western cultural-historical discourse on various levels, including the linguistic, the theological, the philosophical, the divinatory and the medical diagnostic. The last two in this enumeration played a central part in the discourse of the Assyro-Babylonian scholars.

From these general statements, many distinctions are to be made among the forms, the functions, as well as the responses to ominous signs in cuneiform texts. In addition to the various kinds of signs, another important distinction can be made between signs that were seen and/or reported and those that are found as entries in written compendia, as in the series *Enūma Anu Enlil*,[11] *bārûtu*,[12] *Šumma izbu*, and others. The compendia served as vehicles for organizing the signs together with their portents in complex lists of antecedent-consequent statements, the conditional statements 'If P, then Q'. The phenomena are presented in a way that follows a fundamental method of interpretation, more or less employed in each series. This method has been variously referred to as a code,[13] or a hermeneutic strategy.[14]

The relationship between the antecedent and consequent clauses allowed the development of thinking about signs to encompass the observable, the possible, and the conceivable, including, within the category of conceivable signs, those that cannot occur in actuality. Actuality, as we might define it by what is permissible by nature, was not the focus of the scholarly imagination working within the sources in question here. The so-called impossible phenomena have been a puzzle to modern scholars for a long time. It has been offered that the invention of these impossibilities was to fill out and complete interpretive schemata. This is undoubtedly so, but nonetheless question-begging as to the nature of the framework in which the interpretive schemata had validity. Perhaps the reason for our puzzlement is that for too long we have failed to see how a notion of the order of nature was fundamentally absent from and irrelevant to cuneiform divination. The omen series explored the world in a different way.

10 See CAD, Vol. 7, I/J, 304b, s.v. *ittu*, meanings 1–4.

11 See Veldhuis 2010, 77–91.

12 On the difference between the omens from exta in the series versus those in the 'extispicy reports', see Heeßel 2012, 16–35, especially 33–35.

13 Koch-Westenholz 2000, 13; Brown 2000a, 106 and elsewhere throughout the text.

14 De Zorzi 2011.

## 2 Norms and anomalies

What were the characteristics that rendered phenomena ominous? Though many details of the appearances of stars, moon, sun, planets, animals, birds, and insects, human appearance and behavior, as well as sounds and light phenomena, and things seen in dreams, were ominous, not every single phenomenon was an omen. It is with respect to some conventionally established system of reference (the code or hermeneutic method) that something was interpretable as ominous, and even though many unreal and purely imagined phenomena were included in the schematic compilations of ominous phenomena, the system supported a notion of norms and a sense of normal and abnormal. Georges Canguilhem noted the ambiguity of the term *normal*:

> Sometimes it designates a fact that can be described through statistical sampling; it refers to the mean of measurements made of a trait displayed by a species and to the plurality of individuals displaying this trait – either in accordance with the mean or with certain divergences considered insignificant. And yet it also sometimes designates an ideal, a positive principle of evaluation, in the sense of a prototype or a perfect form.[15]

Canguilhem saw these two meanings as linked, therein finding the ambiguity of the term normal at the root of medical thinking about the pathological. In the realm of cyclical physical phenomena, such as those of the sun, moon, planets, and ecliptical star phases, the idea of a mean stemming from a measured standard is a related concept. I submit that the conceptual link Canguilhem drew attention to for the life sciences is also manifested in cuneiform sciences, from divination – which employed the sense of an ideal[16] – to astronomy, which began with the usage of the mean as an ideal, to a later approach that focused on anomaly as defined in relation to a numerical mean.

Referring to the turn of the nineteenth century anatomist and physiologist (and father of histology) Bachat, Canguilhem noted that

> in his *Recherches sur la vie et la mort* (1800), Bichat locates the distinctive characteristic of organisms in the instability of vital forces, in the irregularity of vital phenomena – in contrast to the uniformity of physical phenomena.[17]

Further, he defined Bachat's vitalism in his idea that "there is no pathological astronomy, dynamics, or hydraulics, because physical properties never diverge from their 'natural

15 Canguilhem 2008 [1965], 122.

16 David Brown first pinpointed the importance of the ideal as a norm in Brown 2000a, 113–122, and 125–126.

17 Canguilhem 2008 [1965], 122.

type."[18] The integrity of inanimate physical forms, therefore, did not permit the appearance of 'monstrosities' among such phenomena as, say, the moon and planets. Canguilhem stressed the distinction between living organisms' capacity for monstrosity and the fact that "there is no machine monster",[19] saying, "the distinction between the normal and the pathological holds for living beings alone."[20]

It seems relevant in this context to observe that across the various omen text categories a distinction between anomalous features of physical phenomena and monstrous features of births does not seem to be made. On the other hand, if we search for conceptions of the normal over a range of Akkadian divinatory texts, the same ambiguities as Canguilhem described for the concept may be found. That is, normal can be gauged in terms either of a 'mean of measurements' or an ideal, a 'positive principle of evaluation', where that ideal is determined by the divine scheme of things.

The adjective *kajamānu* (SAG.UŠ) 'normal' is found in omens of the *izbu* and *ālu* series, as well as in extispicy, as a description of, or a feature of a phenomenon. In addition to the passages cited in CAD s.v. *kajamānu* usage a 1′ and 2′, a number of additional passages from liver omens can be adduced, referring to the 'Presence' (*manzāzu*) of the liver, meaning the feature of the liver associated with the presence of a deity. For example, from early exemplars (2nd millennium Middle Babylonian and Middle Assyrian):

> If the normal Presence is there and a second one is placed on the left: The king will resettle his abandoned territory [...].[21]

And from another Middle Babylonian source:

> If the normal Presence is there and a second one descends to the River of the Pouch: The gods of your army will forsake it at its destination ([source] B adds: and it will be routed).[22]

We might cite, additionally, the statement from an extispicy ritual, "[f]or his well-being let there be a normal *naplastu*, let there be a normal *manzāz ilim*", referring to the Presence (of the god) on the liver.[23]

In light of Canguilhem's reference to the 'mean of measurements' being a defining basis for a conception of the 'normal', a passage from a late Uruk commentary may be

18 Canguilhem 2008 [1965], 122.

19 Canguilhem 2008 [1965], 90. From a completely different point of view, cf. Zakiya Hanafi's notion of mechanical monsters, in Hanafi 2000, 76–96.

20 Canguilhem 2008 [1965], 90.

21 Koch-Westenholz 2000, 72, text exemplars K 7, E 12, A 11′, and B 1′; my omission (truncating Koch's parenthesis). See also elsewhere in the Appendix to the Introduction.

22 Koch-Westenholz 2000, 94, line 31, text exemplars A r 8′ and B r 15′. Bracketed insertion ['source'] added by the author.

23 Starr 1983, 32, cited from Koch-Westenholz 2000, 52, note 139.

noted that explains 'the measurement of a normal Presence' as of three fingers length.[24] Ulla Koch cites another, Neo-Assyrian period, text that also describes norms in the features of the liver in terms of sizes:

> The Presence, the Path, the Pleasing Word, the Strength, the Palace Gate, the Well-being, the Gall Bladder, the Defeat of the Enemy Army, the Throne Base, the Finger, the Yoke and the Increment, the designs (subsections) of the Front of the Pouch are three fingerbreadths each measured in the 'large finger,' the finger of the diviner or the *asli*-measure. Seven Weapons, five holes, three Fissures you count as *niphu*s. The Foot is one fingerbreadth long, the Fissure is half a fingerbreadth long, the cleft is two fingerbreadths long, the *šīthu* is three fingerbreadths long, they affect the consecrated place. The circumference of the liver is one cubit 6 fingers, 14 fingers its diameter(?).[25]

As Koch-Westenholz noted,

> The liver may undergo morphological changes or changes due to diseases or parasites. Also external influences can cause changes in the appearance of the liver in the form of lesions and contusions, and different causes may have the same symptoms on the liver. All this was obviously irrelevant to the Babylonians; only the visible symptoms were of interest. They did note the healthy and normal appearance as a favorable sign.[26]

Despite the fact that the health and wholeness of the liver are regarded as of positive divinatory value, the emphasis on deriving positive and negative values for features of the exta overrides the value of the norm in a biological or anatomical sense. The evidence shows that from the seventh century to the later Babylonian Period the system was relatively unchanged, and did not reduce solely to a binary of normal and abnormal, but employed many schemes for determining positive and/or negative outcomes of a liver inspection. As Koch-Westenholz implied, the designation of what was normal did not relate to an investigation of the physical causality of malformation. The interpretive scheme did not function around the understanding of what makes for biological normality, but rather what could be observed of regularity and irregularity from a visual standpoint. Nor did it work in this way in the omens from the twenty-four tablet *Izbu* series, which itself seems to be based, by definition, in the *ab*normal.[27]

24 CAD, Vol. 8, K, 37b, s.v. *kajamānu*, usage a 2′, TCL 6 6 ii 3.

25 Koch-Westenholz 2000, 40: CT 20 44 i 52–58. Interrogation mark copied from Koch.

26 Koch-Westenholz 2000, 40–41.

27 Note that the D-stem adjective *uzzubu*, attested only in lexical texts, according to the CAD entry s.v. means 'freakish, anomalous, monstrous' (CAD, Vol. 20, U/W, 395b).

The omen series *Šumma Izbu* 'if a malformed birth' seems to be the right place to raise the question of whether the scribes thought in terms of 'monsters'. To put the notion of an *izbu* in the context of monsters requires reference to later history.[28] In later antiquity the understanding was that it was in the power of God to act within and against nature to produce any conceivable, or inconceivable, phenomenon so as to communicate with humankind. Indeed, by its etymology, a monster is something that 'warns' (Lat. *monere*) and is therefore a portent (Lat. *monstrum*).[29] Isidore of Seville, for example, said:

> Portents, according to Varro, are those things that appear to be produced against nature. But they are not against nature, since they happen by the will of God, since nature is the will of the Creator of every created thing. For this reason, pagans sometimes call God nature and sometimes, God. Therefore the portent does not happen against nature, but against that which is known as nature [*contra quam est nota natura*]. Portents and omens [*ostenta*], monsters and prodigies are so named because they appear to portend, foretell [*ostendere*], show [*monstrare*] and predict future things.... For God wishes to signify the future through faults in things that are born, as through dreams and oracles, by which he forewarns and signifies to peoples or individuals a misfortune to come.[30]

While Isidore's reasoning may not be totally incompatible with what can be reconstructed for Assyro-Babylonian thinking on the matter, his explanatory rhetoric is. On the grounds of the attributions in prayers to the unlimited power of the gods as well as from the omens themselves, the Assyro-Babylonian gods were viewed as producing any conceivable phenomenon to signal yet another event, but the key element of explanation, as Isidore related it, either that God acts against nature, or that God's will is tantamount to nature, departs from the framework within which omens would be understood by the cuneiform scholars.

An *izbu* is clearly a birth, and in the omen series, *izbu*s can be of animal (dog, pig, bull, cow, sheep, goat, donkey or horse) and human births.[31] *Izbu* is defined in a bilingual lexical commentary as a "prematurely born fetus that has not completed its months".[32] The description of human *izbu*s may be found in the first Tablet of the series, where a woman gives birth to newborns with various sorts of impairments (blindness,

28 For an outline of this history, see Hanafi 2000.

29 Hanafi 2000, 12–13.

30 Isidor of Seville, *Etymologiarum sive originum libri XX*, cited from Daston and Park 2011 [1998], 50. Ellipsis added by Daston and Park.

31 CAD, Vol. 7, I/J, 318a, s.v. *izbu*, usage b, K. 2315: 60ff., and for the reference to a malformed foal, see usage c 2′.

32 CAD, Vol. 7, I/J, 318a, s.v. *izbu*, lexical section.

*Izbu* I 60, and deafness, *Izbu* I 63)[33] or deformities (mental, as in a *lillu* 'fool',[34] physical, as in *akû* 'deformed',[35] and various kinds of conjoined twins). Included among the omens from human *izbu*s are descriptions such as 'if a woman gives birth to a lion/wolf/dog/pig/bull/elephant/ass/ram/cat/snake/tortoise/bird',[36] as well as '… to membrane',[37] or 'spittle'.[38] There are also omens for multiple births,[39] up to 'eight or nine', in the last omen of Tablet I: "If a woman gives birth to eight or nine (children): A usurper will attack; […] the land will become waste."[40]

Overall, including the *izbu*s from animal births, Nicla De Zorzi has shown how the conception of deformity manifested itself in the categories of (1) malformations resembling animal features, (2) absence of body parts, (3) deformed or incomplete body parts, (4) misplacement of body parts, and (5) presence of excess body parts.[41] Some of the *izbu*s are vividly imagined, as for example,

> If a woman gives birth, and (the child) is half a cubit tall, is bearded, can talk, walks around, and his teeth have already come in, he is called '*tigrilu*': Reign of Nergal; a fierce attack; there will be a mighty person in the land; pestilence; one street will be hostile to the other; one house will plunder the other.[42]

On the other hand, further evidence that *izbu*s were not conceived of as monsters is that breach birth,[43] and twins, both identical and fraternal,[44] are also found in the series, neither of which would classify as 'monsters' today for their irregularity.

Erle Leichty noted in his introduction to the series' *editio princeps* that

> the ancient Mesopotamians had no interest in the scientific study of anomalies to seek out their cause or cure. Their interest was centered on the apodosis, or prediction, and not the anomaly itself […] His major concern with the anomaly itself lay in description, and he classified anomalies only to enable himself to find them within the series in order to ascertain their significance.[45]

At the time of writing (1970), in order to qualify as science, the study of birth anomaly had to have explanatory and causal components as to why such malformations occurred biologically, or from the point of view of the genetics of the developing embryo. As

33 Leichty 1970, 37.

34 Note the late *Izbu* commentary to the term *lillu*, cited in Frahm 2011, 209: $^{lú}$lil : *sak-lu* "$^{lú}$lil (means) imbecile", BM 77808 rev. 1.

35 CAD, Vol. 1, A, Pt. 1, 284a, s.v. *akû* B, and note that the commentary text to *Izbu* explains *akû* as *enšu* 'weak', see CAD *akû* B lexical section.

36 *Izbu* I 5–23, see Leichty 1970, 32–34.

37 *Izbu* I 28 and 29, see Leichty 1970, 34.

38 *Izbu* I 71, see Leichty 1970, 38.

39 *Izbu* I 83–131, see Leichty 1970, 39–44.

40 *Izbu* I 131, see Leichty 1970, 44. Ellipsis copied from Leichty (damaged signs in the Akkadian).

41 De Zorzi 2011, 46.

42 *Izbu* I 82, see Leichty 1970, 39.

43 *Izbu* I 85, see Leichty 1970, 39.

44 *Izbu* I 83 (identical twin boys), 86 (fraternal twins), see Leichty 1970, 39.

45 Leichty 1970, 16.

a consequence of this approach, Leichty emphasized the systematic nature of the omen series' compilation of so many malformed births, even though they lacked the notion of physiological deformity having determinant causes. Despite his underlying sense of the non-scientific character of the anomalous birth omens, Leichty rightly observed the importance of the *izbu*'s description and in what sort of interpretation the entity was given, rather in line with Harrison's "science of interpretation which sought to expound the meanings of words and things". The cuneiform study of anomalies at the births of animals or humans was based on the same kinds of relationships between features of other phenomena construed as positive or negative in accordance with an idea of the norm. A binary interpretive system in which right has positive value and left negative, enabled an anomaly on the left side of an 'anomaly' to be positive. Thus:

> If a woman gives birth, and the right foot is twisted: That house will not prosper.

But:

> If a woman gives birth, and the left foot is twisted: That house will prosper.[46]

De Zorzi discussed the binary oppositions of above and below, front and back, inside and outside, large and small, right and left, male and female, dead and alive, as well as normal and abnormal in the context of the *Izbu* series. She said:

> The most common form of binary opposition in the protases is the opposition right/left. The corresponding apodoses fall into the opposing categories of favorable/unfavorable predictions, thus combining themselves with the protases to form pairs of omens based on as structure of symmetric oppositions. While this organizational principle is in evidence in all divinatory disciplines, in *Šumma Izbu* a malformation on the right side (normally the *pars familiaris*) is considered negative, a malformation on the left (normally the *pars hostilis*), positive. This is owed to the context of the observation: a malformation being *eo ipso* a negative sign, the normal meaning of the opposition right ('favorable') / left ('unfavorable') is inverted.[47]

The same interpretive reasoning is also found with respect to planetary phenomena in which the binary pair bright/dim is applied to planets taken to represent benefic (Venus and Jupiter) or malefic (Mars and Saturn) qualities. Brightness is usually a positive indication, and dimness a negative. The brightness of a malefic planet, either Saturn or Mars, is therefore judged to be negative, while its dimness is positive, and vice versa

46 *Izbu*, III 83 and 84, see Leichty 1970, 62.

47 De Zorzi 2011, 52–53. This was also noted in Jeyes 1991/1992, 35.

for the benefic planets Jupiter and Venus.[48] It was no doubt in relation to the degree of brightness that the planets came to represent benefic or malefic qualities in the first place. In relation to the system of analyzing *izbu*s, which were abnormal and unpropitious in and of themselves, in much the same way as malefic planets were 'bad' and unpropitious in and of themselves, the parallel in Late Babylonian texts concerning planets shows that such associations had nothing to do with physical essences, but rather with the value of the phenomenon as a portent, propitious or unpropitious. In the context of the planets, nothing can be inferred as to the planets' nature as physical phenomena from the Babylonian standpoint. Far from representing Canguilhem's "machine monster", or Bichat's "mechanical pathology",[49] malefic planets had 'by definition' negative interpretive value within a divinatory schema.

Thus, the norm for an *izbu*, as for a malefic planet, was simply that untoward events were signaled in each case. Their appearances could, however, signal propitious events if an inversion of the binary values right/bright = good or left/dim = bad, or the like, occurred. Consistent with Koch-Westenholz's observation of what was of chief interest to the diviner's inspection of the liver, that is, in visual description rather than underlying causes of variation or deformation, the *izbu*s were a focus of interest because they represented a class of negatively evaluated forms.

As in extispicy, implicit in the *izbu* omens was the notion of a norm against which *izbu*s, as a class of phenomena, were judged abnormal, and in relation to which the scholarly imagination spun its variations on normal. Indeed, *izbu* omens occasionally use the term 'normal' to refer to a part of the newborn not construed as anomalous, however attached it was to the anomaly. Thus:

> If there are 2 *izbu*s and they are normal (*kajamānu*) except the second one protrudes from his (the first one's) mouth: The king will be defeated, and his army [...] his troops and his suburbs will be devastated.[50]
>
> If an *izbu* has 2 heads, and the second one rides (above) the normal (SAG.UŠ) one: Rebels will revolt against that prince.[51]
>
> If an *izbu*'s eyes are normal (SAG.UŠ.MEŠ), but it has a third one on its forehead: The prince [. . .][52]

The malformed birth omens attest to the keen study of the morphological variation of animal and human births alike, in which was embedded the idea of a norm. As Canguilhem also pointed out, morphological anomaly is not, by definition, pathology, which

48 Rochberg-Halton 1988, 319–324, and Rochberg 2010, 135–142.

49 Canguilhem 2008 [1965], 90.

50 *Izbu* VI 28, see Leichty 1970, 87. My omission.

51 *Izbu* VIII 50, see Leichty 1970, 106.

52 *Izbu* X 58′, see Leichty 1970, 126. Text broken at ellipsis.

seems to have first been conceived by Aristotle in the *Physics*,[53] where a monster is an error of nature. The separation of monsters from prodigies, according to A. W. Bates, did not occur until the sixteenth century:

> [N]either classical embryology nor its medieval interpretation required it [the separation between monsters and prodigies] to be made. In medieval times monsters were *peccata naturae* (slips of nature) and in common with other rare or unusual happenings they were 'unnatural': to the medieval mind expressions such as *praeter ut in pluribus* (outside that which occurs frequently) and *praeter naturam* (beyond the range of nature) were interchangeable.[54]

Consistent with this remark, Daston and Park observed that in the Middle Ages "the explanation of monsters by natural causes" could be found side by side with the idea that monsters were divine portents, sent by God as a warning for sinners.[55] In their words, "[monsters] were suspensions of that [natural] order, signs of God's wrath and warnings of further punishment."[56]

Daston and Park's *Wonders* devoted a chapter to the phenomenon of monstrous birth in the early modern period, presenting a case study of a monster born in Ravenna of the early sixteenth century. This birth could almost have been an entry in the *Izbu* series, as it

> had a horn on its head […] and instead of arms it had two wings like a bat's, and at the height of the breasts it had a *fio* [Y-shaped mark] on one side and a cross on the other, and lower down at the waist, two serpents, and was a hermaphrodite,[57] and on the right knee it had an eye […].[58]

Shortly after the creature's birth, enemy troops came and sacked the city of its birth. As the contemporary source remarked further:

53 Aristotle 1941, Second Book, ch. 8.

54 Bates 2005, 113, parentheses in the original, brackets added by the author.

55 As Daston and Park (2011 [1998], 181–182) said: "The contemporary French chronicler Johannes Multivallis related its [the Ravenna monster's, FR] deformities to particular moral failings: 'The horn [indicates] pride; the wings, mental frivolity and inconstancy; the lack of arms, a lack of good works; […] the eye on the knee, a mental orientation solely toward earthly things; the double sex, sodomy. And on account of these vices, Italy is shattered by the sufferings of war, which the king of France has not accomplished by his own power, but only as the scourge of God.' My ellipsis; the second bracketed insertion ['indicates'] was added by Daston and Park, all other bracketed insertions were added by the author.

56 Daston and Park 2011 [1998], 51 (my insertions).

57 See Leichty 1970, 8 on hermaphroditic *izbu*s.

58 Luca Landucci, *A Florentine Diary from 1450 to 1516*, cited after Daston and Park 2011 [1998], 177. My ellipses; the bracketed insertion ['Y-shaped mark'] was added by Daston and Park.

> It seems as if some great misfortune always befalls the city where such things are born; the same thing happened at Volterra, which was sacked a short time after a similar monster had been born there.[59]

The similarity between the ancient Near East and Western Europe in prognosticating from monstrous births could be due ultimately to the Greco-Roman cultural bridgehead that enabled material of Near Eastern origins to penetrate Western Europe.[60] The idea that it was God's work within, or against, nature that provided an explanation for monsters, however, is altogether different from what was conceptually available in cuneiform texts.[61]

During the Neo-Assyrian period, the untoward consequences of *izbu*s, as well as those of many other signs, both for those given by the gods in the heavens or on earth, as well as signs from extispicy,[62] were dealt with by means of rituals called *namburbi*, performed by an *āšipu* or *mašmašu*.[63] As is the case in many technical terms in the Akkadian scholarly corpus, *namburbû* is a loan from Sumerian NAM.BÚR.BI, meaning 'its BÚR', with NAM acting to nominalize the verb BÚR. The Akkadian equivalent for Sumerian BÚR is *pašāru* 'to loosen', or 'undo', 'release', even 'exorcise'.[64] These rituals were utilized against the evil portended by ominous signs, as well as other potential dangers (e.g., temple offices not carried out properly, headache or disease among the army and horses going on campaign, the effects of sorcery and witchcraft, the evil of fungus).

In his full-length treatment of the *namburbi* ritual,[65] Richard Caplice discussed the semantics of *pašāru* in order to specify the purpose of the ritual. He pointed out that among the fundamental senses of this verb is that of a restoration to order, in contexts where the word is used to mean 'untangle' or 'unravel', i.e., to a state of right order. He cited a passage from *Šurpu* which states that the evil of sorcery may be unraveled by "the symbolic and magically efficacious act of unraveling a tangle of matted material".[66] He concluded that it is this sense that applies in the *namburbi* ritual against ominous signs. What is being untangled, or set to rights, is, as Caplice argued, the evil (HUL/*lumnu*) portended by signs, not the sign itself. It is clear in any number of *namburbi*s that this is the case, for example the *namburbi* against the evil portended by certain birds.[67]

59 Daston and Park 2011 [1998], 177.

60 See Jacobs 2010, 317–339.

61 Similarly, in the *Treatise on Monsters* of Fortunio Liceti of the early modern period, the possible generation of monsters is understood as "*supernatural*, *infranatural*, and *natural* productions". Although he speaks only about these natural causes, Liceti does not fail to mention that "the sole, efficient cause is Almighty God, that is, motive Intelligence and the Heavens". (Cited after Hanafi 2000, 35; italics by Hanafi.)

62 For two kinds of apotropaic rituals for extispicy, see Koch 2011, 456–465, and Koch 2010, 46.

63 See Maul 1994.

64 See CAD, Vol. 12, P, 236b, s.v. *pašāru*.

65 Caplice 1963.

66 Caplice 1963, 23.

67 Caplice 1967, Texts No. 25, 26, 27, and 28, and republished in Maul 1994, 234–248, 268–269, and 256–268.

Expressions used in the sources are unequivocal in saying their purpose is to 'make the evil pass by' – a phrase used as well in the context of lunar eclipses portending untoward events – or 'so that the evil not approach (the man)'. Indeed, the undoing of evil is the goal. However, as in the passage quoted at length below, the *izbu* itself will also be destroyed in the process of undoing its evil. Similarly in reference to a lunar eclipse, a *namburbi* is performed against its evil portent, but the eclipse itself is also in effect undone, as the lunar disk becomes bright again. In each case, the purpose of ritual action is to restore the order of things threatened by the appearance of a bad sign.[68]

The following is a series of *namburbi* rituals for dispelling the evil of an *izbu*, collected on one tablet. To dispel the portended evil the supplicant went symbolically before the divine judge, the sun-god Šamaš, and by means of plants, the river, or strings of beads, cast it out.

> If in a man's house there was an *izbu*, whether of cattle, or of sheep, or an ox, o[r a goat], or a horse, or a dog, or a p[ig], or a human being, in order to avert that evil, [that it may not approach] the man and his house:
>
> ---
>
> You go to the river and construct a reed hut. [You scatter] garden plants. You set up a reed-altar. Upon the reed-altar you pour out seven food-offerings, beer, dates, (and) *šasqû*-flour. [You set out] a censer of juniper. You fill three *laḫannu*-vessels with fine beer, and [you set out] [. . .]-bread, DÌM-bread, 'ear-shaped' bread, one grain of silver, (and) one grain of go[ld]. You place a gold ZU on the head of that *izbu*. You string a gold breast-plate on red thread. You bind it on his breast. You cast that *izbu* upon the garden plants. You have that man kneel, and recite thus:
>
> ---
>
> Incantation: Šamaš, judge of heaven and earth, lord of justice and equity, who rule over the upper and lower regions, Šamaš, it is in your hands to bring the dead to life, to release the captive. Šamaš, I have approached you; Šamaš, I have sought you; Šamaš, I have turned to you. Avert from me the evil of this *izbu*! May it not affect me! May its evil be far from my person, that I may daily bless you, that those who look on me may forever [sing] your praise!
>
> ---
>
> You have him recite [this] incantation three times. The man's house [will (then) be at peace] [. . .], and before the river [you recite] as follows:
>
> ---
>
> [Incantation: y]ou, River, are the creator of ev[erything]. [. . .]-sun, the son of Zerūti, whose [personal god is Nabû, whose personal goddess] is Tašmētu, who [is beset by] an evil *izbu*, is therefore frightened (and) terrified. Avert [from him]

68 Caplice 1967, 26. See also Caplice 1971, 166–168, Text No. 65, republished in Maul 1994, 458–460.

> the evil of this *izbu*! May the evil not approach (him), may it not draw near, [may it not press upon (him)!] May that evil go out from his person, that he may daily bless you (and) those who look on [him] may forever sing your praise! By the command of Ea and Asalluḫi, remove that evil! May your banks not release it! Take it down to your depths!
>
> ---
>
> Extract that evil! Give (him) happiness (and) health! You recite this three times, and cleanse the man with water. You throw tamarisk, Dilbat-plant, *qān šalali*, a date-palm shoot, (and) the *izbu*, together with its provisions and its gifts, into the river, and you undo the offering-arrangement and prostrate yourself. That man goes to his house.
>
> ---
>
> [You string] carnelian, lapis-lazuli, serpentine, *pappardillu*-stone, *pappardildillu*-stone, bright obsidian, *ḫilibû*-stone, […] (and) breccia on a necklace. You place it around his neck for seven days [. . .] the evil of that *izbu* will be dis[sipated].[69]

The final point to consider is whether *izbu*s were understood as an expression of divine wrath, in the manner argued in the context of later European monstrous births. This is an interpretation deeply rooted in Assyriological literature, going back, according to Caplice, to Julian Morgenstern in the early twentieth century.[70] We find it again in Stefan Maul,[71] and in Amar Annus' introduction to the volume on divination, where he said, "according to Namburbis, the person to whom the evil omen was announced had to placate the anger of the gods that had sent it to him and effect the gods' revision of their decision", thereby achieving "a correction of his fate which the gods had decreed".[72] In the *namburbi* rituals for the *izbu* quoted above, the person in whose house an *izbu* appeared presents himself before the sun-god and says the incantations that ask the god to rid him of the evil omen and prevent that evil from approaching. The ritual does not involve appeasement of the gods either on the part of the supplicant or the *āšipu* in charge of the ritual performance, but consisted of various symbolic acts of casting off (onto the plants, into the river) and cleansing, as well as the request through incantation for restitution by the divine judge, Šamaš. Šamaš is not to be placated, but to receive the plea and make a decision. Just as the *izbu* omens' interpretive structures had to do with norms and abnormality, the ritual against an *izbu*'s portended evil acted

69 See Caplice 1965, 125–130, Text No. 10, republished in Maul 1994, 336–343. Note that the lines of the tablet have been run together for space saving. In general, words or letters inside square brackets mean that the broken tablet has been restored, while parentheses mark translator's glosses. Parentheses, brackets, and ellipses were inserted by Caplice, except the first ellipsis of the last paragraph (where Caplice has another restored portion inside brackets).

70 Caplice 1963, 28–29 and note 1, where he cites Morgenstern 1905.

71 Maul 1994, 10.

72 Annus 2010, 7.

to remove or keep evil away and re-establish the norm. As expressed in the *namburbi* text, the norm, or the normal, was taken as happiness and health.

The question of whether an *izbu* signifies divine anger is still not resolved, however, as the *namburbi* for the evil of an eclipse, mentioned above, offers another perspective. A lunar eclipse was the manifestation of a disturbance of the moon-god, often expressed as that god's being in mourning or emotional distress (*lumun libbi*, literally 'trouble of the heart'). In some contexts *lumun libbi* means 'anger.'[73] The afflicted person is required to set up an altar to the moon-god, Sin, present offerings and, prostrated, recite a prayer three times before the moon/moon-god, as the celestial body and the god are, for the purpose of the ritual, one and the same:

> May the great gods make you bright! May your heart be at rest! May Nannar of the heavenly gods, Sin the exorcist, look (hither)! May the evil of eclipse not approach me or my house, may it not come near or be close by, may it not affect me, that I may sing your praises and those who see me may forever sing your praises![74]

The text adds for the *āšipu*:

> You have him recite this, and you undo the offering arrangement. You perform the [ritual] for the evil of signs and portents, and the evil of eclipse will not approach him.[75]

The exhortation in the prayer for the quieting of the moon-god's heart is a clear reference to *lumun libbi*.[76] Whether the connotation is of the moon's grief, or his anger, is not clear, even though the phrase is normally understood in astrological contexts to mean 'grief.' In any event the eclipse was construed as a sign of the moon-god's state of mind, which had to be restored to its normal state of brightness (and happiness) by an offering and the recitation of the prayer. This strikes a contrast to the *izbu*. As the *izbu* was not referred to as the result of divine anger, the ritual does nothing to appease a god, but rather it brings the matter before the sun-god as judge to restore things to normal.[77]

73 CAD, Vol. 9, L, 250b, s.v. *lumun libbi*, meaning 2.

74 Caplice 1971, 168. Parenthesis by Caplice.

75 Caplice 1971, 168. Brackets by Caplice.

76 Interestingly, in the section preceding the prayer (line 5′), a reference is made to the condition of the afflicted one's heart.

77 Cf. the Sumerian incantation included in another *namburbi*: "Incantation: The sign that is evil shall not approach the man! At the word of Utu [= Šamaš, FR], baillif of the gods, who defeats the sign that is evil for man, (who defeats) anything (evil) that approaches, – though the man (lit. seed of man) himself be unaware of it [alternatively: may not be aware of it, FR] – it shall not approach him to his detriment! Like water – water poured into the canal – his punishment shall not approach him! His evil shall not hover about him! (These are) the words of Enki and Asalluḫi." See Caplice 1967, 273–274, Text 25: 14′–19′, and translation on p. 276, quoted here (parentheses by Caplice).

The foregoing discussion aimed to show that *izbu*s, premature or malformed births, were not conceived of as monsters, and the word *izbu* does not signify a 'monster,' except in the classical sense of a portent. The evil portended by an *izbu* does not seem to have been conceived of as a result of divine wrath, or as punishment for human sin. They were not errors of nature, or deviations of nature from its own laws, but only portents in the same way as were other ominous signs in cuneiform, i.e., as part of a language of divine communication in the exta, in the heavens, and in other domains, for indicating both favorable and unfavorable consequences of representations of or deviations from the norm.

Variation with respect to a conceived norm made signs ominous, but not all omens in the cuneiform world were anomalies. Phenomena that fell within norms were also portentous, and were deemed propitious. Again, where Canguilhem placed the conception of the monster, or the monstrous, in the context of living phenomena, the cuneiform material leveled the playing field for all ominous phenomena, not reserving 'monstrosity' for the living, indeed, not expressing the notion at all.

## 3 Celestial signs and astral phenomena: regularity and anomaly

Norms for cyclical astral phenomena were defined differently from those in the biological realm, where the definition of health, or 'normal,' permits a good deal of variability before one begins to speak of a defect or an anomaly. Evidence of this kind of standard of measure by the healthy appearance of the liver or other organs is found in extispicy omens. In the *izbu* omens, the standard itself was anomalous, as just discussed.

Periodic phenomena in the heavens, on the other hand, are amenable to counting, or other arithmetical methods by which to construe regularity. As a result the term meaning 'normal' in astral omens is *minītu*, from the verb *manû* 'to count.' Thus, the day when sun and moon were in opposition on an anomalous day of the lunar month, was expressed as *ina la minâtišunu*, literally, 'not according to their (calculated) norm,' or a lunar eclipse might occur *ina la minâtišu* 'not according to his (the moon's) (calculated) norm.'[78] A similar expression is constructed with the word *simanu* 'time,' i.e., 'not according to its time,' where the sense of a celestial body's appearance anomalously is conveyed.[79]

Such references relate to Canguilhem's first notion of normal as the measured or calculated mathematical mean. But the function of the norm in the various cuneiform divinatory contexts, astral, extispicy, and *izbu* omens alike, was to differentiate the meaning of those signs by deviations from 'normal.' Where Canguilhem focused on the de-

78 CAD, Vol. 10, M, Pt. 2, 87a, s.v. *minītu*, meaning 1d.

79 CAD, Vol. 15, S, 269b, s.v. *simanu*, usage c.

velopment of ideas of pathology in life forms, he noted the problem with such a notion in physics and mechanics. While celestial divination was oriented to phenomena that would also be of interest to later physics and mechanics, the interest in them was as signs, in the same framework as the liver and the *izbu*. From the point of view of divinatory knowledge, there was a unity between the signs in heaven and the signs on earth; all belonged to the category of signs. The other aspect of Canguilhem's investigation into the normal were his remarks about the ideal, which, as cited before, were described as "a positive principle of evaluation, in the sense of a prototype or a perfect form". David Brown has explicated this idea in early astronomical and celestial divinatory texts concerning cyclical astronomical phenomena and the arithmetical schemes devised for reckoning with them in a divinatory context.[80]

Already from the earliest periods an interrelated group of ideal units of time reckoning was devised for accounting purposes, and because those units came to undergird early Babylonian astronomy as well as the tradition of *Enūma Anu Enlil* omens, they would remain in the cuneiform scholastic tradition for two millennia.[81] This group of ideal units focused on the 360-day year of 12 30-day months. Of the local Sumerian calendars in the Ur III period, where real month lengths varied, calendar months in the city of Nippur became standard and were later taken over as the month names of the ideal calendar (12 30-day months = 1 ideal year) common to the scholarly traditions of the astral sciences, both astrology (celestial and natal divination) and astronomy (MUL.APIN, Astrolabes), prior to ca. 500 BCE, and even later in some cases.

Already in the Old Babylonian Period, the variation in length of daylight was understood as deviations from the ideal dates of the equinoxes. The earliest evidence for the quantitative model for daylight length is found in an Old Babylonian text (BM 17175+).[82] In four sections, one for each schematic season, the text gives the model as follows:

> [On the 15th of Addaru, 3 (minas, or 3,0 UŠ) are a wa]tch of the day, 3 (minas, or 3,0 UŠ) are a watch of night; [Day and night] are equal. [From the 15th of Addaru to ] the 15th of Simanu is 3 months. [On the 15th of Simanu, the night] transfers 1 (mina, or 1,0 UŠ) of the watch to the day. [.... 4 (minas, or 4,0 UŠ) is the wa]tch of the day, 2 (minas, or 2,0 UŠ) is the watch of the night.[83]

This model of the ideal year assigned the equinoxes and solstices to the midpoints, or 15th day, of months XII, III, VI, and IX. For each schematic season, or quadrant in the ideal year, the length of daylight shifted by 1 unit. Therefore, from vernal equinox

80 Brown 2000a, 113–122 for his description of 'period schemes', and 146–155 for their impact on celestial divination.

81 Englund 1988; Brown 2000b; Brack-Bernsen 2007; Britton 2007, 117–119. For month lengths and the Babylonian calendar, see Britton 2007 and Steele 2007, 133–148.

82 Hunger and Pingree 1989, 163–164.

83 Text enclosed in square brackets is restored. Text in parentheses is a translator's gloss.

to summer solstice, the length of day increased by '1', from summer solstice to autumnal equinox, daylight decreased by '1', and so on, producing a model for the change in the length of daylight in which the ratio of longest to shortest day length was 2 : 1. Thus: 3 (VE) + 1 = 4 (SS) − 1 = 3 (AE) − 1 = 2 (WS) + 1 = 3 (VE). This scheme is not practical for the geographical latitudes of Mesopotamia, but it is the simplest, indeed most elegant, way to model the experience of increasing and decreasing durations of daylight around two extremes (summer and winter solstices), provided the model is contructed on the ideal year. The mean value was expressed in sexagesimal notation as the number 3, i.e., 180 (3 × 60), and represented one-half of the circle of the day (360 degrees) when daylight and night were of equal length (180 degrees).

Another group of astronomical texts, with exemplars from the Middle Babylonian and Middle Assyrian periods, and now called 'astrolabes', arranged in circular or list form three groups of heliacally rising stars month by month together with numerical values for the length of day in those months. This group of texts provided the full complement of numerical values that made up the model attested in the Old Babylonian Example cited above. In Tab. 1, 'C' designates the value for length of daylight, taken as constant for the duration of the month.

Again it is clear that the mean value of the table is 3 (= 3,0), representing the length of daylight (or night) at the equinoxes.

From the standard Assyro-Babylonian astronomical compendium known as MUL. APIN, preserved from exemplars dating to the 7th century BCE, statements concerning length of daylight show that the Old Babylonian model for variation in daylight (Tab. 1) was still being transmitted. The text says, for example:

> *ina Nisanni* UD.15 3 mana maṣṣarti mūši 12 UŠ *napāḫu* ša Sin
>
> On the 15th of Nisannu (= Month I) a nighttime watch is 3 minas; 12 UŠ the (daily retardation of the) rising of the moon.[84]

This gives the same value for daylight length at the vernal equinox, but it occurs in the first month *Nisannu*, rather than the twelfth month *Addaru*. The remaining cardinal points of the year were also shifted up one month, from XII to I for the vernal equinox, as just seen, from III to IV for the summer solstice, and so on. This shift in the calendrical reckoning of the cardinal points did not alter the underlying schematic model for daylight length variation.

Another section of MUL.APIN clarified the scheme for an entire ideal year.[85] The section not only spelled out the lengths of night for each ideal month but also included the value for the visibilities of the moon, whether from rising or before setting.

84 See Hunger and Pingree 1989, 102: Tablet II ii 44. My parentheses.

85 MUL.APIN II ii 43–iii 12.

| Month | C (in mana) | C in UŠ | = Hours | Cardinal Points |
|---|---|---|---|---|
| XII | 3;0 | 3 | = 12 hr | Vernal Equinox |
| I | 3;20 | 3,20 | = 13 hr 20′ | |
| II | 3;40 | 3,40 | = 14 hr 40′ | |
| III | 4;0 | 4 | = 16 hr | Summer Solstice |
| IV | 3;40 | 3,40 | = 14 hr 40′ | |
| V | 3;20 | 3,20 | = 13 hr 20′ | |
| VI | 3;0 | 3 | = 12 hr | Autumnal Equinox |
| VII | 2;40 | 2,40 | = 10 hr 40′ | |
| VIII | 2;20 | 2,20 | = 9 hr 20′ | |
| IX | 2;0 | 2 | = 8 hr | Winter Solstice |
| X | 2;20 | 2,20 | = 9 hr 20′ | |
| XI | 2;40 | 2,40 | = 10 hr 40′ | |

**Tab. 1** Astrolabes' scheme for variation in daylight length. (The relation between the measures for C in the second and third column is 1 *mana* = 1,0 UŠ. Mana was a unit of weight, for measuring water into the water clock. 1 UŠ = 4 time degrees.)

MUL.APIN's interest in the night lengths and visibilities of the moon is followed by a short passage explaining the calculations for the duration of lunar visibility using a 'difference' coefficient (*nappaltu*), e.g.,

> 40 NINDA nappalti ūmi u mūši ana 4 tanaššīma 2,40 nappalti tāmarti tammar
>
> multiply 40 NINDA, the 'difference' of daylight and night, by 4 and you will find 2,40, the 'difference' of the visibility of the moon.[86]

The same numerical values for daylight lengths according to the model of MUL.APIN and the Astrolabes also underlie the calculation of the duration of visibility of the moon at night, found in Tablet 14 of the omen series *Enūma Anu Enlil*.[87] The duration of lunar visibility was of course related to the length of night, and the value given as the IGI.DU$_8$.A = *tāmartu* 'visibility' of the moon is figured as 1/15th of the length of night. For example, in Month I day 1, day = 3 and 1/6 and night = 2 and 5/6. On this day the IGI.DU$_8$.A of the moon is given as 11;20, the result of dividing the length of night

86 MUL.APIN II iii 15 (slightly modified translation from Hunger and Pingree 1989, 108).

87 Al-Rawi and George 1991/1992, 52–73.

by 15. For an equinoctial day, e.g., Month I, day 15, the $IGI.DU_8$.A of the moon is given as 12. Night length at the equinox = 3,0 (moon rises at sunset and rises at sunrise and is visible the entire night, for 180°). The value for $IGI.DU_8$.A is 12, which is 1/15 of 3,0 (= 180°). The length of night was always the complement to the length of day, where on any given night of the schematic year, day + night = 12 *bēru* = 24 hours = 360 UŠ (360 degrees of time = 24 hours). Values were given in monthly intervals, but a statement from MUL.APIN confirms that these values could also be interpolated on the basis of semi-monthly values for the length of the day: "The Sun which rose towards the North with the head of the Lion turns and keeps moving down towards the South as a rate of 40 NINDA per day."[88] 40 NINDA per day is the result of the regular increments or decrements of 10 units each ½ month, i.e., in 15-day periods. Further interpolations could be made by dividing by 30 (the number of days in a schematic month) the semi-monthly differences between values for the daily retardation of the moon throughout the schematic year, tabulated in *Enūma Anu Enlil* Tablet 14.[89] These quantitative descriptions were results of modeling, not measuring, the variation in length of daylight and the underlying structure was the ideal year, 12 30-day months or 360 days.

It seems to me that the numerical mean value in this ideal scheme, the value 3 (3,0 = 180), as the representation of one-half of the circle of the day, was not simply a derived mean value from the schemes for daylight length and duration of lunar visibility, but played a determining role in the construction of those schemes.

David Brown drew attention to the role of ideal schemes,[90] showing as well that the numerical value assigned to the ideal, as construed in accordance with those schemes, and deviations from the ideal, was the basis for interpreting propitious and unpropitious signs. This shows the consistency of the scholars' approach among the various domains of signs, and how the notion of a norm was instrumental to the entire system. I would further concur with Brown's insight that the categories by which the heavenly phenomenal world was structured in celestial omens, were, in his words, "devised in order to make the sky above interpretable", and that "it [the phenomenal world] was categorised in this manner *in order* that it could be encoded with signs".[91]

Brown's insight can be applied more widely within cuneiform knowledge corpora. As noted above, phenomena of the liver and exta that deviated from normal were studied as ominous signs. Those appearances that fell within the range of normal were also counted as omens, signaling generally propitious events. As shown above, even some *izbu* omens had elements described as 'normal.'

88 MUL.APIN II i 11–12 (translation from Hunger and Pingree 1989, 72–73).

89 Al-Rawi and George 1991/1992, 58.

90 See note 80 above.

91 Brown 2000a, 153. My insertion, emphasis by Brown.

The foregoing has sought to explore the way in which norms and anomaly were important components of cuneiform scribal thinking about the world of phenomena. Given the necessity of making meaning from signs, these ordering principles aided omen divination in the interpretation of the perceived, experienced, or imagined phenomena of the scribes' spheres of interest. While central to omen divination, the use of ideals and anomaly was not limited to the divinatory enterprise. It is also found in early Babylonian astronomical texts wherein the approach is entirely consistent with that of divination. It seems significant that the establishment of norms against which to define anomaly was employed in the understanding and interpretation of both physical and non-physical phenomena, both terrestrial and celestial. Perhaps, however, in the 'empire of celestial knowledge' the further development of this principle would see its most significant gains.

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# LIS BRACK-BERNSEN: BIOGRAPHICAL NOTES AND SELECTED BIBLIOGRAPHY

# In Honor of Lis Brack-Bernsen

Lis Brack-Bernsen taught history of science at the *Lehrstuhl für Wissenschaftsgeschichte* at Universität Regensburg. Her research interests include the history of astronomy and mathematics with special emphasis on Babylonian astronomy and its development.

A mathematician by training, with physics and astronomy as subsidiary subjects, she was trained in history of ancient astronomy and mathematics by Olaf Schmidt during her studies at the University of Copenhagen. She switched to the history of Mayan astronomy for her Ph.D. studies at Universität Basel, sponsored by the Swiss National Foundation. After a few years as a lecturer at the Mathematical Institute of Copenhagen University, she went to the USA. Following a time bringing up her three children, she returned to full-time research on Babylonian astronomy through a grant from the German Research Foundation *DFG*. After some further years as a lecturer (*Privatdozentin*) at Johann Wolfgang Goethe-Universität, Frankfurt am Main, and a stay as Visiting Fellow at the Dibner Institute, she became lecturer at Universität Regensburg in 1999. Three consecutive *DFG* research projects have enabled her to investigate the development of Babylonian astronomy, and to understand and reconstruct many empirical prediction rules written in Babylonian procedure texts. In 2006, she became a Professor at Universität Regensburg.

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