‡2 Babylon's System A & the 1274 BC Eclipse

The Oldest of All Traceable Eclipse-Records

by Dennis Rawlins

Gratitude to Opposites

This investigation and the one (‡3) immediately following it were both triggered by my recent fortunate encounter[1] with a learned analysis by Bernard Goldstein: the lead paper in the 2002 Feb *Journal for the History of Astronomy*. I am obliged and delighted to here acknowledge the debt. I've also, on numerous occasions, benefitted from chats with Alex Jones & John Britton regarding Babylonian lunar theory.

Goldstein's paper (following on the heels of DR's delivery of ‡1 at the British Museum the previous June) was clearly intended to encourage and stimulate the discovery of the long-unknown sources of the Babylonian lunar periods. BG expresses a becoming humility and amiability in carrying out his mission. True, if he follows his group's sad tradition, he will never take pleasure in the present unexpected potential fruits (‡2 & ‡3) of his own paper; but we are here expressing our thanks to him and to the *JHA*, regardless — and will continue to hope for some untraditionalism.[2]

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[1] The 2002 Feb *JHA* arrived at The Johns Hopkins University's central library on 2002/3/14; I first saw the B.Goldstein paper 3/16. On 3/18 (13:02EST), I thought I faintly recalled that double eq.1's 1010^{y} (eq.2) was the length of a very long saros-series I'd encountered at some point in the past. Then, with bizarrely atypical unrushedness, I delayed 7 hours before finally getting around to running a global search through the whole *DIO* file, swiftly finding Rawlins 1996C §H2's 1010^{y} saros-series. This unleashed the present paper; and shortly thereafter (2002/4/3-4) also the following paper: ‡3.

[2] One can rationally dispute the precise date-estimates proposed in this paper and the next (‡3). But those experienced in astronomy will discern the obvious strength of the analyses' general foundation: [a] Long eclipse-cycles were the only reliable method which scientists of the era in question possessed (and attested) for determining their high-accuracy monthlengths, especially the difficult anomalistic month. [b] This firm basis takes us (via eq.2 here & ‡3's Hipparchos-redolent eq.3) inevitably into the previously-unknown region of 13th century BC eclipse data. Opposition to these findings will surely stress: [i] DR is an amateur in Babylonian "astronomy". [ii] The era suggested is extremely remote. [iii] No records of 13th century BC eclipses survive directly today. [iv] How could early calendars date them accurately, anyway? [v] Our new findings have forced us to the seemingly-risky (though see ‡3 fn 12) conclusion that three Babylonian tablets (ACT 100, 104, 150), computed for c.200 BC, were back-calculations actually performed at least a half-century later (after −140). See ‡3 §D1.

Contra these potential complaints: [i] DR openly boasts of being a green amateur (*DIO 1.2* fn 19 & *DIO 3* fn 197). (Are the "pros" also turning a little green, when one who doesn't even seek their grant-funds is solving some of their own field's mysteries?) [ii] The Ammizaduga Venus Tablets evidently bear pre-13th century data; and the strength (§§A2&A3) of the presumption of long-eclipse-cycle-basis is far stronger than a mere argument-from-absence (fn 7). [iii] As for attestation: there's not-a-jot of testimony describing any means used by Hipparchos or earlier astronomers for finding accurate lunar months, other than by the multiply-attested (‡3 §E5) method of long-eclipse-cycles. (Ptolemy's own alleged methods are later, were fabricated, and *don't generate integral period-relations*: §A3.) [iv] Babylon knew what day it was, despite its unsteady pre-Metonic calendar (fn 37). [v] Back-calculations were (and are) ordinary astronomical work (‡3 §D1); the only 200 BC Babylonian tablets based upon Hipparchos' draconitic equation also happen to be the only ones that do not bear a date-of-writing; the only Hipparchos-ratio-based material that *is* dated happens to be post-Hipparchos (*idem*).

A reader can make up his own mind regarding which arguments here are primary; but he shouldn't be surprised at a few unfalsifiable-adamantine reactions to the issues raised by these papers. We'll set a more scientific example by (‡3 fn 10 & §D5 [& see *DIO 11.2* p.31]) openness to alternate theories, plus ready acceptance that discovery of a 200 BC-inscribed tablet computed via ‡3 eq.1 would instantly disprove Hipparchos' authorship of that equation. [And, in case radiocarbon testing can tell 100 BC tablets from 200 BC ones, *DIO* will welcome such checks. (Also for the Ammizaduga copies.)]

A The Magnificent Durability of Babylonian Eclipse-Recordkeeping

A1 It is well-known that the central relation of Babylonian lunar System A is the ratio (see, e.g., Goldstein 2002 p.7f or Neugebauer 1975 pp.478&501):

$$6247^{\mathrm{u}} = 6695^{\mathrm{v}} \tag{1}$$

(As in ‡1, we will here use our standard abbreviations: d = days, h = hours, u = synodic months, v = anomalistic months, w = draconitic months, g = anomalistic years).

A2 Among the several professional historians who deal regularly with Babylonian materials, there has long persisted a strangely infectious notion (to the point of rather inflexible orthodoxy) that such long period-relations (more than 500^{y} in this case) are illusory, that centuries-long Babylonian lunar relations were instead built up by indoor mathematical manipulation from far shorter ones, an idea perhaps related to the unkillable popular myth (justly scoffed at by Neugebauer 1957 p.152 [vs Neugebauer 1975 pp.107&643]) of ancient scientists as a bunch of dreamy non-empiricists.[3] Yet to an astronomer, it is chapter-one obvious (see also Rawlins 2003J §J1) that celestial periods are found *most accurately* by using extremely long temporal baselines.[4] (This is simply *standard procedure* for astronomers. The preferability of such an approach was also self-evident to modern historians' own Hellenistic hero, C.Ptolemy, each of whose alleged derivations of his tables' periods [for Sun, the five planets, and all lunar cycles] used positions observed centuries apart.) This, because division by a lengthy time-interval reduces the effect of measurement-errors (at each end of the interval) to trivial proportions. See at ‡1 §A3 how ordmag 1^{h} errors in the empirical basis for M_{A} melt into an error of less than a timesec in M_{A} itself. (See also Rawlins 1996C fn 110.) Note: if one bases a long cycle upon a short one, empirical errors' effects will *obviously* be artificially inflated — what ancient astronomer would invite that? Is there even a single attested case of such ancient manipulation?[5] Why are certain historians so ready (p.26) to jettison self-evident proper scientific procedure (normal both anciently & modernly) even in the face of ancients' undeniable repeated & consistent success in getting results whose impressive accuracy is consistent only with such solid scientific means?

A3 Some scholars' antennae seem permanently unequipped to receive yet another extremely clear (and quite elementary) signal: *only eclipse-cycles automatically & exclusively produce integral*[6] *period-relations* — which is just how all ancient pre-Ptolemy (fn 2) lunar motions were expressed (‡1 eq.2 & ‡3 §E1). See §H.

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[3] The alibiing of Ptolemy's sins often goes in the direction of rapturously declaring it only natural — even outright admirable (Graßhoff 1990 pp.214-215, Rawlins 2002V fn 57) — that Greek theorizing submerged empiricism. Problem: how could the ancients have (centuries before Ptolemy) gotten all three of their key monthlengths (eq.4 or ‡1 eq.2 [anomalistic]; ‡1 eq.1 [synodic]; & ‡3 eq.1 [draconitic]) correct to one part in ordmag a million merely *by indoor logical conjuring*? [Least accurate month: anomalistic, as expected from our hypothesis. Analogy: Rawlins 1985G §5 ¶3.] Again (p.3): this is just another case of a lapse in common sense regarding probabilities — forgetfulness perhaps related to a common modern-historian confusion (Rawlins 2002V fnn 7&35) of ancient semi-comprehending *transmitters* with the brilliant *originators* of ancient astronomy's refined achievements.

[4] DR has long noted (Rawlins 1987 & Rawlins 2003J) that all five of the short planet periods cited at *Almajest* 9.3 are descended from long tropical cycles, themselves derived (via 1°/cy precession) from wellfounded similarly-long empirical sidereal relations (*DIO 2.1* ‡3 fn 17). [Note: a surviving papyrus (Jones 1999A 1:67, 69-80; 2:2-5, Pl.1) records a 104 AD astronomer observationally checking the Jupiter relation 315 syn revs = 344 sid yrs. (See *DIO 9.1* News Notes for our admiring astonishment at this find.)] To create *Almajest* 9.3's short relations, Ptolemy (or whoever) divided such long tropical cycles by integers (Rawlins 2003J): Saturn, 11 (or 7 or 9); Jupiter, 6; Mars, 8; Venus, 62 (or nearby); Mercury, 5. (I.e., long cycles bred short ones: the very inverse of historians' perception.) The truth is especially obvious in the case of Jupiter where, if a short tropical relation were primary, it would have to be the neat 83^{y} cycle, not Ptolemy's 71^{y} one — whose remainder is (relatively) 50 times bigger!

[5] Promoters of such unattested ancient math manipulation simultaneously reject the upsetting but now obvious implications of ancient math manipulation which *is* effectively attested, e.g., ‡1 eqs.6&12.

[6] Or half-integral period-relations — which mere doubling renders integral. (See ‡3.)

A4 Let us start with a Ptolemaic example of §A2's approach, demonstrating the fruitfulness of using extremely long eclipse-cycles — such being the obvious and natural empirical base for the determination of accurate lunar period-relations, a method which was explored in an earlier *DIO*: see Rawlins 1996C §E, where we found that merely tripling or quintupling Ptolemy's last synodic-anomalistic lunar relation (fn 7) found eclipse cycles. In the case of eq.1, just *a simple doubling* will do the trick, instantly producing the central equation upon which the Babylonian System A lunar periods were founded:

$$12494^{u} = 13390^{v} = 13558^{w}1/2 - 22^{\circ} = 1010^{g} + 42^{\circ} = 368955^{d}1/3 \qquad (2)$$

(As to whether it could be an accident that eq.2 is an eclipse cycle: see analysis at ‡3 §E.)
A5 The foregoing's main surprise is swiftly apparent to any Babylonian-astronomy scholar: eq.1 was probably discovered in the 3rd century BC (§B3); therefore, eq.2 requires that the inventor of System A had access to (almost certainly Babylonian) eclipse records of the *13th century BC*, none later than (§B4) 1274 BC — a date which is more than 500 years before[7] what have been attested (& generally accepted) as the earliest eclipse records that came down to classical-era astronomers. (But see Jones' suggestion [& Rawlins 2002V §B3 vs Rawlins 2003P §E4] that Ptolemy had only 2nd-hand knowledge of early data.) It is over 400^{y} before the earliest record we previously had even good *in*direct evidence for ancient use of (via §A4's 795^{y} eclipse cycle): the −830/2/4 eclipse. [See §G, below.]
A6 Though the suggestion of 13th century data (surviving into the Seleukid era) may initially appear outré, there are considerations weighing strongly in its favor: [i] No other direct empirical basis for eq.1 (accurate to nearly 1 part in a million) has ever previously explained it. [ii] A remarkable confirmation of extremely ancient Babylonian eclipse records is about to arise quite independently in the paper immediately following this one (see ‡3 §B) — and the indicated record in that case is also from the 13th century BC: specifically 1245 BC (within just a few decades of the range suggested above at §A5).
[Yet a *third* 13th-century-eclipse indication has now appeared: *DIO 13.1* ‡2 §§E2&E3.]
A7 As in the 795^{y}-eclipse-cycle case cited in §§A4&A5 (also exhibiting a 22° remainder [Rawlins 1996C eq.11] — which verges on the outer limit [‡3 fn 17] of eclipse-pair possibilities), the 1010^{y} cycle is an extremely fragile relation: a 1010^{y} eclipse pair occurs very, very seldom (unlike the quite common 345^{y} pairs of ‡1 eq.2). That infrequency presumably inconvenienced those ancient pioneers who were trying to establish eq.1 empirically — but it is a fortuitous boon to the modern historical detective: it severely restricts the number of eclipses that could have contributed to eq.1's ancient discovery. Therefore, we are assisted in narrowing the sample of eclipses (and thus the era) that could have underlain eq.1.

[7] Conventional scholars interpret *Almajest* 3.7 as saying that only from Nabonassar's time (747 BC) were observations preserved. But Ptolemy just says this is so "on the whole". (Toomer 1984 p.166 n.59 notes that extant cuneiform records are generally consistent with that date, though, given these records' thinness, one can hardly conclude anything firm in such fashion. [See 2003 note at end of this paper.]) In response: [a] Ptolemy does not claim that nothing at all survives from an earlier time. His statement appears to imply that continuous records start with Nabonasser [747 BC]; however, our proposal here is not that a continuous eclipse-record (from the 13th century BC down to Ptolemy) survived intact, but rather that a small bunch of 13th century BC data came through — either [i] exceptionally and in precious isolation, or [ii] as the oldest data (among centuries of spotty records between c.1300 BC and Nabonasser) then available, deliberately selected in order to found System A's central synodic-anomalistic period-relation (eq.1, as it turned out) upon as long a temporal baseline as possible. [b] Conservatives continue to be silent about the fact that the only solutions yet presented that explain (perfectly) Ptolemy's final lunar synodic-anomalistic equation ($3277^{u} = 3512^{v}$ [fn 21]) require eclipse data that cannot be later than 831 BC. (See Rawlins 1996C eq.10 & §E6 [or Rawlins 2003P eq.3]. See also Rawlins 2003J §L on antiquity of implied Babylonian planet records.) [c] Wise young Hugh Thurston is fond of quoting a wise old adage (especially wise in the study of ancient science, where over 99.9999% of the physical records are lost): absence of evidence is not evidence of absence. (See ‡1 fn 2. Or: did Alcor not exist in 128 BC?) [d] If we cannot accept any finding in ancient science without direct attestation, then: should we all park our brains at the entrance to the ancient science field? Is it forbidden to induce beyond the texts?

B Behind System A: the Saros-Series-Pair Prime Suspects

B1 The eclipse-pairs satisfying eq.2 are few in number, and (rather like the situation for the 795^{y} cycle: Rawlins 1996C §F) the visible ones do not occur at all uniformly in time. For that very limited number of pairs which do occur: in every case, the 2nd eclipse belonged to a saros-series whose Meeus-Mucke (1992) number was 5 greater than that of the 1st eclipse's Meeus-Mucke number. So, we will group our data according to Meeus-Mucke saros-series numbers (using the prefix "MM" to signify those numbers):
B2 Some sample pairs[8] from before 480 BC:

MM18&23:	−1952/06/16	&	−942/08/08
MM15&20:	−1841/12/14	&	−830/02/04
MM21&26:	−1811/05/19	&	−801/07/11
MM31&36:	−1558/10/07	&	−548/11/29
MM27&32:	−1547/03/12	&	−537/05/05
MM31&36:	−1540/10/17	&	−530/12/10
MM33&38:	−1518/08/16	&	−508/10/07
MM33&38:	−1500/08/26	&	−490/10/19

B3 A systematic search was made for 1010^{y} pairs whose latter eclipse occurred during the centuries following 500 BC, where the earlier eclipse could be seen in Babylon and the latter either there or in Alexandria. Revealingly, no pairs at all were found where the 2nd eclipse occurred between −244/2/7 (useless, since its −1255/12/15 mate was entirely invisible[9] in Babylon), and +67/5/17 (both it and its −943/3/25 mate were invisible): a blank of more than 300^{y}! All of which suggests the 3rd century BC as the approximate origin-epoch of eq.1. In §C1, we will present further evidence for such a date.
B4 Now to the post-500 BC eclipse-pairs not already noted. From the MM30&35 group:

−1442/01/22 & −432/03/15

But, notably, by far our richest saros-series matchup here is MM34&39, which (due to a longterm-near-stable anomalistic relationship between the two series) handed a bunch of 1010^{y} pairs to any 3rd century BC astronomer *who had access to the rich eclipse-record heritage of Babylon*. This single group (MM34&39) produced three visible[10] pairs:

−1345/10/22 & −335/12/14
−1291/11/23 & −280/01/16
−1273/12/05 & −262/01/26

[8] Most not visible in Babylon at both ends. The −1841/12/14 & −830/2/4 pair was already noticed at fn 1. See Rawlins 1996C §H2. It might be fun to speculate that this very early pair was the basis of System A's eqs.1-2. But (even aside from enormous inherent improbability, e.g., a huge discontinuity in ΔT's variation and-or a Chinese [!] report of the prior event): such a stretch-recourse is quite needless when so many other 1010^{y} pairs are known to end much nearer the era of the first firm extant evidence of System A's existence.

[9] According to modern theory, which of course is subject to change in response to future findings. Just in case it ever turns out that the −1255/12/15 eclipse was recorded (and this would only be a decade before the −1245 eclipse of ‡3 §C9), we may here note that the −1255 & −244 pair parallels a complete 1010^{y}-long saros-series (§B5): the 2nd eclipse ends series MM39, and the 1st eclipse is adjacent to MM39's beginning: see §B3 method [b].

[10] The last eclipse listed (−262/1/26) was invisible west of around Persepolis but its conclusion could easily have been recorded in the eastern part of the Seleukid empire. (See §E4.) Lunar theory was by then advanced enough that an eclipse's mid-time could be found (to all required accuracy) from the umbral emersion-time, simply by correction from indoor tables.
[Note added 2008. The original edition's list carelessly (since no use was made of it) included the −1417/09/09&−407/10/31 pair; but the −1417 event was invisible in Babylon.]

B5 Given the 1010^y feature of eq.2 (not to mention §A1), we note in passing that both saros-series MM34 and MM39 lasted 1010^y — and we are pairing eclipses (from each series) which are themselves 1010^y apart. This suggests that the very choice of 1010^y as an interval (not an especially attractive one, otherwise) may have been related to Babylonian saros-series-tracking.[11]

B6 How would a classical-period scholar determine the lunar anomaly for a 13th century BC eclipse? Possibilities:

[a] As Goldstein 2002 p.3 notes, Lis Brack-Bernsen in 1994 very ably laid out a case [see, more recently, Brack-Bernsen 1999] that regular Babylonian non-eclipse data could've identified anomalistic variations. [Britton 1999 p.220 believed that eclipses underlay Babylonian lunar theory; but he has later come to have doubts on that point.] If such means permitted determining the day (hardly hour) of apogee near early eclipses, an eclipse that occurred on an estimated apogee-day could be paired with an eclipse 12494^u (1010^y+) later to produce eq.2.

[b] A scholar of the 3rd century BC could have realized that, given the nearly steady pace of the gradual lunar-anomalistic shift (averaging $-3°$/saros: Rawlins 1996C eq.14), which accrues during a saros-series' duration (ordmag a millennium), one could simply take a very long (e.g., 1010^y)[12] saros-series (fully visible or no) and compare the eclipse at one end of it to an eclipse 1/2 year beyond the eclipse at the other end — and the two eclipses' lunar anomalies would be roughly equal (within about $10°$). Such an approach could have produced eq.2.

[c] If an ancient scholar believed that eq.2 was a stable cycle — that is, if a set of 1010^y-spaced eclipse-pairs seemed to exhibit closely equal intervals — then he might use them as Aristarchos used the 345^y cycle to find the month's length (see ‡1 eq.3 & §A2).

B7 However, option [c] (using several eclipse-pairs — as against the two one-pair methods: [a]&[b]) would be based upon an illusion, since eq.2 is actually *not* very steady. True, as we saw in ‡1 §A2, the best idea for finding the anomalistic month from period-returns is the identification of a near-perfect return in both lunar and solar anomaly (which would indeed ensure the constancy of the pairs' intervals). But the 1010^y cycle's duration (eq.2) is much less stable than the 345^y cycle's (‡1 eq.3). Not only is eq.2 less accurate & less frequent (in eclipse-occurrence) than the 345^y cycle (so one doubts if enough data could allow even a try at showing eq.2's constancy); but it (eq.2) also has a far less perfect return in solar anomaly g, causing periodic error with serious amplitude: the solar anomalistic remainder is $\Delta g = 42°$ [eq.2], vs merely $7°1/2$ in ‡1 eq.3. Lunar anomaly remainders ($-8°$, $-1°$, resp) add lesser error-amplitude. For the 345^y cycle, these two unwelcome amplitudes' sum is merely $2^h/3$ (rms even less: ‡1 §A3), while for the 1010^y cycle the sum is c.4^h. (For the 795^y cycle [Rawlins 1996C eq.11]: c.5^h.) [See Rawlins 2003P §F7 tabulation.]

B8 A further complicating factor for method [c]: the most fragile eclipse pairings (such as 1010^y & 795^y) cannot come off when apogee-proximity is too great, so an average of even the densest & most scrupulously-collected set of observed results will not yield a correct mean month. This is inevitable when a large and quite unrandom[13] fraction of the sample is comprised of eclipse-pairs which are not mutually umbral. (By contrast, this is not a serious problem for ‡1's 345^y case.) Since all the intervals for the 1010^y eclipse pairs in our key saros-series-pairing (MM34&39: §B4) were above-average,[14] the most exact ancient empirical averaging of 1010^y-pair records would have yielded a result a few hours too-high.[15] This is a provocative point (which we'll return to at §E3) — since the System A synodic monthlength *was* in fact seriously too high.

B9 All of which warns us of the invalidity[16] of applying method [c] (looking for a stable interval) to a set of 1010^y eclipse-pairs. The safest conclusion here is that one-pair methods ([a]&[b]) are likelier[17] [Note added 2013. On later reflection, DR inclines (uncertainly) rather to supposing that option [c] was the (flawed, as indicated) source of eq.1.]

[11] See fn 9 — and the now-somewhat-less-dreamy speculation at Rawlins 1996C §H6.

[12] A saros-series of length 1010^y is the 2nd longest in the period under examination. (Note §B5.) [An odd coincidence: the longest Ptolemy sidereal planet-cycle (Mars: Neugebauer 1975 p.906 Table 15) is 1010^y long.]

[13] See the huge gap in 1010^y pairs specified in §B3.

[14] Mostly just short of $368955^d1/2$. Compare this to eq.2.

C Eq.1's 3rd Century BC Origin

C1 A starting consideration: as late as Kallippos' calendar (epoch $-329/6/28$), the month's true length seems not to have been known even within 20^s (Rawlins 1991H fn 1), while System A's synodic month was only off by 4^s (§§D1&E1); so we can probably eliminate the pair ending at $-335/12/14$ — and all the earlier ones.[18] Thus, the preferred candidates' 2nd eclipses are $-280/1/16$ and $-262/1/26$.

C2 Survival of the Babylonian "Saros Text"[19] luckily may help us probe further, if perhaps on rather thin ice. This text directly attests to the length of the System A anomalistic month V_A (Neugebauer 1975 p.501), by telling us (in degrees)[20] what half of it equals:

$$V_A/2 = 1,22,39,49,30 = 4959°49'30'' = 13^d279°49'30'' \tag{3}$$

C3 So, simply doubling eq.3 produces the Saros-Text-attested System A anomalistic month V_A, which was (and is) correct within a fraction of a timesec:[21]

$$V_A = 9919°39' = 27^d199°39' \tag{4}$$

C4 The noteworthy and perhaps revealing feature about eq.4 is the strikingly imprecise-looking Babylonian expression for V_A: $9919°39'$. But there are two distinct ways of interpreting this feature. The next two sections will investigate these in order.

[15] The 795^y-pair interval of Rawlins 1996C §E7 was (typically) even further *below*-average.

[16] Even if one preferred option [c]: the eclipse-pair ending at $-262/1/26$ is still the best guess for providing System A's date, since it ended (for 300^y: §B3) a hitherto-long-accumulating series of visible 1010^y eclipse-pairs — so the period following -262 was (for centuries thereafter) the time of maximal availability of fresh 1010^y eclipse-pair data.

[17] Option [c]'s several problems (§B7) have pushed us towards preferring the conclusion that a single eclipse-pair launched eq.2 and thus System A. And note that, even if [c] were accepted, there would be a most-recent pair of the data-set adopted; so we still end up trying to identify (among the data listed in §B6) the single 1010^y eclipse-pair that immediately launched System A.

[18] However, anyone inclined to date System A at around 500 BC, might make something of the density of 1010^y pairs ending in the period -548 to -490 (§B2).

[19] Note the provocative coincidence that we are here finding (eq.9) that the key parameter (V_A) revealed by the "Saros Text" is founded upon saros-series data (MM34&39: §B4).

[20] Babylonians divided the day into degrees, not (as the Greeks did) into our hours of $1^d/24$.

[21] V_A is several times more accurate than the better-known Greek value for V: ‡1 fn 4. The occurrence of such a smack-on-the-mark hit, found *en route* to the determination of the less accurate System A synodic month (§§D1&E1), suggests a possibility: did System A's establisher have outside access to an accurate V_A? (A notion encouraged by [Rawlins 1996C §E7 & by]: the oddity that though V is much harder than M to gauge accurately, the ancient computer treats V_A as primary, with M_A then computed only secondarily *from* V. See *idem*.) More conservative view: by accident, the error in eq.1 ($6247^u = 6695^v$) nearly cancelled the error introduced by the System A inventor's use (in eq.2) of too high a value (eq.5 or eq.7) for the 1010^y interval. Note: eq.1 was about 3 times less accurate than the famous relation (Rawlins 1996C eq.3) $251^u = 269^v$ resulting from ‡1 eq.2 — or than the equally impressive if less-wellknown late-Ptolemy relation (Rawlins 1996C eq.10) $3277^u = 3512^v$.
[Note added 2003. Rawlins 1996C §E tripled 3277^u, finding eclipse cycle 9831^u, unusable for Ptolemy. But quintupling 3277^u gets eclipse cycle 16385^u. Improbably, 2 of Ptolemy's 4 eclipses, 125/4/5 & 136/3/6 (*Almajest* 4.9&6, resp), have mates (visible Babylon) 16385^u earlier: $-1200/7/11$ & $-1189/6/12$, resp. Details: Rawlins 2003P. Both pairs yield same pseudo-stable 483859^d interval (actually [pulsing] high by 1 part in c.2 million). See p.3 fn 2 & Rawlins 2003P §§E4&F5 & fn 6.]

D Whole-Day Rounding at Both Ends of the Eclipse-Pair

D1 Since it is likely[22] that the hour of the 1st eclipse (a millennium earlier) did not survive, it is reasonable to ask whether it's coincidental that the $1'$ imprecision in V_{A} (eq.4) corresponds to the 1^{d} imprecision in eq.5's numerator and to the 4^{s} error in the System A synodic month M_{A} (eq.6): all three imperfections are roughly 2 parts in a million.
D2 If this approximate triple-coincidence is meaningful, then the inventor of System A rounded both[23] ends of his eclipse-pairs to the nearest whole day — and computed his anomalistic month V_{A} as follows:

$$V_{\mathrm{A}} = 368956^{\mathrm{d}}/13390 = 9919°39'13'' \approx 9919°39' = 27^{\mathrm{d}}199°39' \tag{5}$$

which matches the attested value (eq.4).
D3 Note: for Greek time-measure (fn 20), the key unit-rounding-step in eq.5 will not produce the attested eq.4 result (making it $27^{\mathrm{d}}13^{\mathrm{h}}18^{\mathrm{m}}37^{\mathrm{s}}$ instead).[24] Which suggests that the computer of V_{A} was Babylonian.

E Whole-Day Rounding at Only the Early End of the Eclipse-Pair

E1 Our other and potentially more precisely-fruitful interpretation starts by wondering: if it were known[25] that V_{A} was as crude (eq.5's rounding) as it appears, then why would the Saros Text's ancient calculator carry his figuring (via eq.1) of the System A synodic month M_{A} to so many places (see Neugebauer 1975 p.501)? —

$$M_{\mathrm{A}} = 6695V_{\mathrm{A}}/6247 = 29^{\mathrm{d}}31'50''19'''11''''\ldots = 29^{\mathrm{d}}.5306444\ldots \tag{6}$$

Starting with this consideration, we probe by testing eq.5 backwards — and find thereby that V_{A} will end up *looking*[26] remarkably round if the numerator in eq.5 is:

$$t = 368955^{\mathrm{d}}7/8 \tag{7}$$

E2 This t (eq.7) was seriously mistaken (high by roughly a a half-day),[27] an error which became[28] the main factor degrading the accuracy of the contingent System A synodic month; however, this slip may turn out to be of critical assistance in telling us today which of our eclipse-pair candidates produced the t which led to System A's monthlengths.

[22] [But note Rawlins 2003P §E5's curiosity about the basis of Ptolemy's highly accurate 3277^{u} equation (fn 21).] For the scholar who established System A: assuming he knew of the 1st eclipse report's time-roughness (it actually occurred nearer 6 AM than 6 PM), then he reasoned (wrongly) that the benefit of the antiquity of the −1273/12/5 eclipse outweighed the disadvantage of its crudity. (Hipparchos was faced with a parallel dilemma when considering whether to use Meton's similarly corrupted epochal solstice-time: Rawlins 1991H §§B3&B8.)

[23] The problem here is that precedent consistently shows that a classical-era astronomer attempting to determine a very large period, by using a longago day-epoch-anchored 1st datum, did not round his own 2nd datum to the nearest whole-day. Two examples at Rawlins 1985H.

[24] Natural unit-roundings (occurring at key reconstructed steps) have been interpretively used earlier in this issue: in the ‡1 derivation of the System B month; there, roundings twice consistently suggested (§A8 [but note there Britton's simple Babylonian theory] & fn 2 item [d]) that the inventor of System B worked in Greek time-measure. Now, this same reasoning attracts us (in §D, at any rate) to the conclusion that Babylonian time-measure was used in the computations of the inventor of System A.

[25] The argument here is analogous to that of ‡1 §A11, except that there is more reason in that case to be sure that the computer (Aristarchos) knew 1st-hand the true precision of the crude-looking quantity (since he'd probably computed it himself). By contrast: it's unlikely that the flukishly-surviving Saros Text was authored by System A's originator, so the author may have known nothing about V_{A}'s actual precision or origins.

[26] See eq.9. [But keep in mind that eq.6's inaccuracy is apt to eq.4's apparent imprecision.]

[27] Compare t in eq.7 (System A) to t in eq.2 (real). (And see fn 30.)

[28] By the hypothesis of this section (§E).

E3 Now, we already encountered above (§§B7&B8) the likely cause of a significant portion (roughly 1/4) of the total error; if eq.7 applies, then the remaining part (about 3/4) of this total comes from a factor which DR has elsewhere already adduced (Rawlins 1985H) to explain most of yet another astronomical-calendar systematic overstretching-tendency (Hellenistic astronomers' *always*-overlong [Rawlins 1999 §C10] estimates of the tropical year): an ancient scholar's use of ancient-to-him calendar-related astronomical data often forced him to use time-reports that recorded merely an event's *date*[29] — not its hour. In which case, he would — whether knowingly or not — use the epoch hour (i.e., starting or zero hour) of the day *containing* the event. This would incidentally pad the interval upwards (by a half-day on average).[30] Again: this would occur simply because the 1st-eclipse record was mis-cited implicitly or explicitly to the day-epoch of the calendar of the 1st eclipse's observer. In the case of Babylon, the day started at evening (Neugebauer 1975 p.1067): 1/4 day before the modern day-epoch, midnight.[31]
E4 Therefore, to begin the process of identifying the eclipse responsible (via eq.2) for System A's fundamental period-parameters, we merely subtract $1^{\mathrm{d}}/4$ (§E3) from the $7^{\mathrm{d}}/8$ remainder just realized at eq.7. This elementary arithmetic tells us that the computer estimated (not very accurately) that his eclipse's middle occurred half-way through the afternoon (i.e., 5/8 through the day modernly figured from midnight), which we'd call 3 PM. (Babylon would've quantified it as: 45° short of day's end.) A 1^{h} error would be unremarkable for Babylon. (See Dicks 1994 fn 46.) But, if occurring too early (c.2 PM), such an eclipse would be invisible even in the eastern Seleukid empire.[32] So the mid-time of the eclipse we are looking for would have to be c.4 PM in order simultaneously to satisfy ($\pm 1^{\mathrm{h}}$) eq.7 while being partly visible at least somewhere in greater Babylonia. Checking the times of every eclipse on our §B4 list of candidates (by direct calculation — or via published canons of eclipses or full moons), we find that only one eclipse-pair makes the cut: that whose later member is the −262/1/26 partial eclipse, the end of which was visible in the eastern part of the Seleukid empire (fn 10): Persepolis, Tehran, and beyond. For Babylon, this eclipse's middle occurred (invisibly) about 16^{h} (4 PM) Babylon Apparent Time.[33]
E5 Since for our chosen pair, we now possess the times for the 1st eclipse (§§B4&E3) and the 2nd eclipse (§E4), it is easy to reconstruct the interval t used by System A's inventor: since he thought the time of eclipse was −262/1/26 5/8, we have

$$t = [-262/1/26\ \ 5/8] - [-1273/12/4\ \ 3/4] = 368955^{\mathrm{d}}7/8 \tag{8}$$

So the ancient founder of System A was able to calculate his anomalistic month:

$$V_{\mathrm{A}} = (368955^{\mathrm{d}}7/8)/13390 = 27^{\mathrm{d}}199°39'00''.4 \approx 27^{\mathrm{d}}199°39' \tag{9}$$

which gloriously matches eq.4 (Saros Text) with a seemingly round result — (packing more precision than superficially apparent)[34] that evidently had a special appeal for ancient ephemeris-creators (see compendium at ‡1 fn 5), presumably for reasons of convenience and easy remembrance.

[29] See Rawlins 1991W fn 223 for brief discussion of the responsive progression of ancients' eclipse-report precision, as theorists' interest in accuracy advanced.

[30] And by about the same amount in our single case: fn 27.

[31] So, if the −1273/12/5 eclipse was believed by System A's originator to have occurred at the start of the Babylonian day, we would express said local time as: −1273/12/4 3/4 (eq.8) or 6 PM.

[32] Obviously, to be visible at all, an eclipse fitting our conditions should be a winter event — and, as well, it ought to be either a very long eclipse (not the case here) and-or was observed by astronomers situated to the east of Babylon.

[33] One should always keep in mind that ancients used apparent not mean time. (We are assuming that the calculator took account of converting seasonal hours to equinoctial hours.)

[34] Like ‡1 eq.6 (from eq.5). Note that if eq.4 is accurate despite its roundish appearance (our 2nd hypothesis [§E]), this has key implications: [a] The Babylonian-rounding argument of §D3 becomes irrelevant and valueless. [b] Since eq.9 is how V_{A} came out round-looking, the likelihood would be enhanced that eq.9 indeed produced eqs.3&4. [c] Eq.9's pseudoroundishness could explain the oddity

E6 According to n.6 of Britton 1999 (an extensive study, by a scholar long deeply versed in Babylonian materials — and taking a totally different approach [vs ours] to System A: see esp. his pp.219-227), the earliest calculations found (so far) upon unquestionably-System A cuneiform tablets are those of ACT 70; which lists data starting with a full Moon that occurred only a few months after (the full Moon which was) the very −262/1/26 eclipse that we have just shown (§E4) probably launched System A. (The tablet examines full Moons from late −262 to late −251. See Neugebauer 1955 1:117, 3:47.) It is arguable[35] whether we can trust that the tablet was actually computed during that time range; but, of course, I cannot (and should not) refrain from remarking such a close temporal coincidence.

F System A: Babylonian or Greek?

F1 Following our evidence (§§E4&E6) on System A's date of birth, we turn to the question of *place*-of-birth. The obvious point in favor of Babylonian (as against Greek) origin is the upfront item: the −262/1/26 eclipse couldn't be seen in the Hellenistic world. However (even aside from the fact that this eclipse was also invisible in Babylon itself), we know that Babylonian observations were transmitted to the Greek world and were used by astronomers there. (See, e.g., ‡1, Rawlins 1991W fn 223.)
F2 Nonetheless, one ultimately senses that System A was Babylonian — at least in place. Summing up:
[a] The −262 eclipse was seeable in the Seleukid empire, not the Ptolemaic.
[b] Early System A lunar material exists only on Babylonian cuneiform tablets.
[c] And, by distinct contrast[36] to Babylon's System B synodic month (‡1 eqs.8&12) and draconitic month (‡3 §C), not-a-jot of (known) high-level Greek astronomy connects mathematically to System A.
[d] See also §D3.
So, the preponderance of evidence is in favor of our (necessarily very tentative) conclusion here that: System A probably originated in Babylon.[37]

G Appendix: Late Use of 9th Century BC Astronomical Data

[Two intriguing items (discovered after 2002/5/31 first-posting of this paper) add to mounting (& surprising) evidence for classical-era utilization of records of celestial observations from well before the epoch (747 BC) of Nabonassar, contra current perception (fn 7).]
G1 Both of these evidences point to the 9th century BC (§A5), near the −830 eclipse which Rawlins 1996C §E6 suggested on other grounds could have been [but see fn 21's appended bracket] used to derive Ptolemy's last lunar equation (Rawlins 1996C eq.10).

that, though eq.4 shows 4^s precision, its V_A was accurate to within 1^s. However, the relative inaccuracy of associated M_A (eq.6) reminds us of the obvious possibility that V_A's accuracy is simply an accident (1st hypothesis [§D]).

[35] Note: the very chronological implication which appears to fit so well here will be doubted in a different context during evaluation of new findings to follow. See ‡3 §D1. (The earliest explicitly dated System A lunar tablet in Neugebauer 1955 [1:100] is −48/47 [ACT 18].)

[36] Question: given ‡3 §§D1&D2, which of System B's monthlengths are we sure did not come to Babylon via Hipparchos?

[37] [Our finding (that 13th century BC eclipse data were usable roughly 1000^y later) has the implication that Babylon maintained calendaric continuity throughout its long astronomical history (our thanks to Alex Jones' skepticism, for triggering this DR realization), a magnificent accomplishment in itself, especially since the Babylonian calendar was irregular until late in the city's history. Yet, despite that apparent impediment, Babylon was evidently (vs. Rawlins 2003P §E5) able to keep its calendars straight: after all, the 8th century lunar eclipse-triad cited by Ptolemy (*Almajest* 4.6) is accurately dated, though it occurred centuries before Babylon's calendar became reliably Metonic.]
[Note added 2013. It seems likely that Babylonian eclipse-specialists privately depended upon the steady Egyptian calendar to avoid being misled by the vagaries of Babylonian & Greek civil calendars.]

G2 [First noted 2002/10/12.]
The c.1000^y size of most Ptolemy *PlanHyp* sidereal planet-cycles requires stationary-point data taken within ordmag 10^y of −830 (Rawlins 2003J §L1).]
G3 [First realized 2002/12/14.]
Jones 1999G's dating Geminos to 90-25 BC puts a lower limit of 825 BC for the date of the 1st eclipse establishing his 800^y eclipse cycle. (See ‡3 §E1, Geminos 8.40-41, & Rawlins 1996C §I2: post-1997 reprints.) This, too, is within ordmag 10^y of −830.
G4 Thus, we now have (§§G1-G3) three independent indications (plus another hint at Rawlins 1996C §D), all pointing to classical-era use of 9th century BC celestial data.

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H A General Theory of Ancients' Cyclicities

Certain Muffiosi are extremely upset at the present paper & ‡3, insisting (with classic-Muffia preternatural surety) that pre-8th-century-BC eclipse records could not possibly have been accessible to Hipparchos-Ptolemy. See *DIO 13.1* ‡2 §H on such opining's mote-beam imbalance, plus startling & crucial implications for the long-curiously-durable former orthodoxy that serious ancient math astronomy was born in Babylon. Muffiosi also carp at our fertile exploitation of long cycles. So let's go beyond ‡4 §B1 to propose a *DIO* general theory: Greeks expressed the mean motions of **all seven** wandering celestial bodies by integral math ratios ultimately founded upon empirical integral cycles: 5 planets (‡4), Moon (‡1 eq.2), & sometimes even the Sun (‡1 fn 17, *DIO 11.2* p.33 item 8).
For attestation & the generally sound reasoning-beneath, see, e.g., fnn 2&4, §A3, & ‡4 §B2.