‡3 Hipparchos' Draconitic Month & 1245 BC Eclipse

Late Use of 13th Century BC Data Independently Confirmed Hipparchos' Debt to Babylon: Gerald Toomer Vindicated?

by Dennis Rawlins

A How the Ancient Draconitic Month-Source Got Investigated

A1 Shortly after 2002/4/3-4 midnight, while[1] pondering the prospect of the foregoing paper's inevitably meeting rejective stolidity from a certain group of scholars, I was mentally resorting to the point that all precise ancient lunar periods have by now been traced to eclipse periods. But then I suddenly recalled that such tracing had in fact not ever been accomplished for the ancients' ultimo (marvelously accurate)[2] synodic-draconitic relation

$$5458^{\mathrm{u}} = 5923^{\mathrm{w}} = 5849^{\mathrm{v}} + 147^\circ = 441^{\mathrm{g}} + 97^\circ = 161178^{\mathrm{d}} \qquad (1)$$

where I have here tossed in several extra later-useful items, additional to the well-known integral numbers of synodic & draconitic[3] months. (As previously in both ‡1 and ‡2, we adopt our standard abbreviations: d = days, h = hours, u = synodic months, v = anomalistic months, w = draconitic months, g = anomalistic years.)

A2 This famous relation (eq.1) is ascribed (see §C7) by Ptolemy to Hipparchos' analysis of eclipse-pair data chosen to avoid the effects of lunar-anomaly differences — though modern scholars have (again[4] wrongly, as we are about to see) rejected Ptolemy's 1st-hand report upon Hipparchos' work.

A3 As to wrong-headedness: as a matter of ironic personal confession, I should say that I've long presumed [e.g., Rawlins 1996C fn 59] eq.1's source would never be known. Why? Because initially the difficulty in finding an eclipse period here looks staggeringly intimidating: the Δv remainder is 147° — *about 2/5 of a circle*. Thus, searching analogously to ‡2 §A4, we see that the only multiplicative integer which has a hope of producing a useful eclipse cycle (from eq.1) is 5; but multiplying eq.1 by 5 would produce a cycle over 2200^{y} long! — much too remote (implying use of eclipse data from c.2500 BC).

B Draconitic Jackpot

But 3 considerations spectacularly rescued this at-first-seemingly-hopeless[5] situation:

B1 I realized that since the number of synodic months in eq.1 is even, we can find a possibly-useful relation just by halving[6] eq.1:

$$2729^{\mathrm{u}} = 2961^{\mathrm{w}}1/2 = 2924^{\mathrm{v}}7/10 + 1^\circ + = 221^{\mathrm{g}} - 131^\circ = 80589^{\mathrm{d}} \qquad (2)$$

[1] I am thus again (see also ‡2 fn 1) deeply indebted to Goldstein 2002.

[2] See Rawlins 1996C fn 55. [Cause of accuracy (far superior to 579^{y} cycle): see fn 7.]

[3] The draconitic month ("eclipse month") is the time the mean Moon takes to return to a node.

[4] See likewise at ‡1 §A10.

[5] One recalls A.C.Doyle's penetrating observation (also acknowledged at Rawlins 1973 pp.148-149) that whatever seems most to complicate a problem, can be the key to solving it.

[6] I later noticed that I'd already come upon eq. 2 quite independently of Hipparchos — and had even published it as Rawlins 1996C eq.18 — noting only in passing that its double equalled eq.1. Therefore, in the six years since Rawlins 1996C eq.18 was published [1995/12/31], no one — myself most emphatically included! (note that Rawlins 1996C's expression of eq.18 obscured the key 5-factor) — has had the sense simply to follow DR's integral-multiple enhancement approach (e.g., Rawlins 1996C: eq.10→eq.11 [and §I2 in reprints]): mere multiplication by 5 — which would have produced the upcoming discovery of eq.1's source: eq.3.

B2 Then, using the fact (§A3) that eq.1's Δv is nearly 2/5 of a circle, I understood the key (2002/4/3-4, 00:50EST) — and immediately turned it: multiplication of ***half*** of eq.1 by 5 produces an eclipse-cycle, and one of length far more reasonable than 2200^{y}-plus.

B3 Still, at over 1103^{y}, the implied cycle is huge enough that it would formerly have been automatically discarded. Yet now, after ‡2 eq.1 delivered us naturally and inevitably to a 1010^{y}-cycle origin for System A (‡2 eq.2), millennial remoteness has come well within our purview: indeed, we're about to see that the early foundation-eclipse here will *fall right into* the very mid-13th century BC bin which the revolutionary findings of ‡2 (§A6) had already inadvertently prepared the way for. [Yet a 3rd 13th cy BC bin-hit (p.3 fn 1) at Rawlins 2003P §E2!] (If a regular *DIO* reader hasn't yet had a dawning of awareness of why ancient astronomy is an inductive scientist's cloverpatch, it isn't ever going to happen.) So, we simply multiply eq.1 by 5/2 (or eq.2 by 5) — and thereby hit the ancient-draconitic jackpot, namely, the hitherto-secret[7] eclipse-cycle that produced the well-known ratio eq.1:

$$13645^{\mathrm{u}} = 14807^{\mathrm{w}}1/2 = 14623^{\mathrm{v}}1/2 + 7^\circ = 1103^{\mathrm{g}} + 63^\circ = 402945^{\mathrm{d}} \qquad (3)$$

C A Cascade of Verifications

C1 Anyone familiar with orbit theory knows the only type of eclipse-pair that can straightforwardly produce eq.3's odd (1/2-circle-anomaly!) lunisolar relation is: one eclipse near lunar perigee, the other around apogee — both partial eclipses of similar magnitude. To students of the history of ancient mathematical astronomy, this extremely special type of equation will instantly have a familiar smell: only one astronomer is known (*Almajest* 6.9) ever to have found the draconitic month by using a perigee-apogee partial eclipse-pair, namely, the internationally famous 2nd century BC Hellenistic astronomer, Hipparchos.

C2 As in ‡2 (§A7), we find ourselves with a delicate cycle: while eclipse-pairs satisfying the 1103^{y} cycle are not very rare, those with apsidal alignment are quite unusual. So we next list the near-equal-magnitude perigee-apogee eclipse-pair possibilities from c.500 BC to Hipparchos (again finding — as also in ‡2 §§B2&B4 and Rawlins 1996C §E6 etc — that the prospects occur in temporal bunches, far from randomly); and we give mid-eclipse anomaly v in brackets, magnitude m in parentheses (both data *DIO*-calculated), and the Meeus-Mucke (MM) numbers (consistently differing by 35) at left:

MM02&37:	−1604/09/05 [357°] (0.12)	&	−501/11/20 [184°] (0.18)
MM02&37:	−1550/10/08 [349°] (0.02)	&	−447/12/21 [176°] (0.16)
MM35&70:	−1298/10/12 [178°] (0.35)	&	−195/12/25 [006°] (0.20)
MM35&70:	−1280/10/23 [176°] (0.37)	&	−176/01/06 [004°] (0.21)
MM19&54:	−1274/06/21 [183°] (0.96)	&	−171/09/02 [008°] (0.93)
MM35&70:	−1244/11/13 [171°] (0.39)	&	−140/01/27 [359°] (0.26)
MM19&54:	−1238/07/12 [177°] (0.68)	&	−135/09/24 [002°] (0.81)

C3 We can readily dispense with the early pairs. (These were only listed in §C2 in order to illustrate how many centuries can go by with no appearance at all of eclipses satisfying the §C1 conditions required for utility in draconitic period-determination via eq.3.) So we concentrate upon the last 5 eclipse-pairs of §C2, where we of course note a coincidence which is delightfully indicative, since we are looking for a partial eclipse: of the three extant Hipparchos lunar eclipses, the only partial one he is known[8] to have reported (also used) was that of −140/1/27 *an eclipse which is right there* in the short §C2 list — and specifically stated (*Almajest* 6.5) as having been observed in Hipparchos' Rhodos (not Babylon, note).

[7] Probably never published even in antiquity: see §D4. Relatively, eq.3's anomalistic remainder is c.4 times better than the 579^{y} cycle's (fn 9; Rawlins 1996C fn 59): barely half the Δv (7° vs 13°), for twice the time-base. [Some believe that H quit fn 9's neat 579^{y} eclipse cycle for eq.1's *shorter* 441^{y} anomalistically-nonintegral eclipse cycle.]

[8] See *Almajest* 6.5&9. Note that Hipparchos could (with sufficient accuracy) know the anomaly of both eclipses by calculation from his already-established anomaly tables, founded upon ‡1 eq.2.

C4 Next indicator: we emphasized at §C1 that Hipparchos is the only astronomer known to have used[9] a perigee-apogee pair of partial eclipses to determine the draconitic month.
C5 Which perigee eclipse did he use for this purpose? Again: the −140/1/27 eclipse!
C6 Of all these eclipses, which is nearest perigee? Check at §C2. (And the pair ending in −135 [Hipparchos' era] has the lowest mean absolute deviation from the apsidal line.)
C7 And, further, who is the only astronomer who is attested to have discovered eq.1? Hipparchos — at *Almajest* 4.2 (Toomer 1984 p.176 or Pedersen 1974 p.163).
C8 How does Ptolemy say (*idem*) Hipparchos did it? With equal-magnitude eclipses.
C9 Is it even necessary to re-cap the foregoing connexions to Hipparchos? Despite decades of consensus that eq.1 was a Babylonian creation, we now possess strong & coherent evidence (§§C3-C8) that Hipparchos discovered it (as Ptolemy informed us: §C7), presumably using the −1244/11/13 & −140/1/27 eclipse-pair.

D The Surprising Consequences of §C9

D1 Some System B Babylonian texts reflect use of eq.1 in calculations for lunar latitudes c.200 BC, well before Hipparchos (Neugebauer 1955 p.127, Neugebauer 1975 p.523). Which seems[10] to favor −195 (§C2) as eq.1's date.[11] However, those cuneiform records which tabulate eq.1-based lunar latitude data (from −205 to −75: ACT 100, 104, 122, 123, 150) are largely just calculation-lists, bearing no date-of-writing. [Yet, as helpfully noted by A.Jones, other tablets with c.200 BC latitude data do carry explicit dates: ACT 101, 102, 135. But, hitherto-overlooked: [a] With one exception, the *dated* latitude-function tablets *do not* exhibit eq.1's 5458^{u} period. [b] The exception is ACT 122; whose explicit date-of-writing is −102 (Neugebauer 1955 1:144), *post*-Hipparchos — and very close to the date of another Babylonian tablet (ACT 210) that unquestionably used Hipparchan data (Rawlins 1991H eq.9). [c] The *pre*-Hipparchos-dated tablets all *conflict with eq.1*. See tabular comparisons at Neugebauer 1955 pp.131, 135, 162, showing incompatibility of ACT 101, 102, & 135 with eq.1-based ACT 100, 104, & 150, resp. [d] Of the six latitude tablets computed for c.200 BC, *all* three eq.1-based ones are undated, while *all* three dated ones are non-eq.1-based. So the very tablets once taken as proof that Hipparchos swiped eq.1 from Babylon, now seem to favor his authorship of it,[12] *a point independent of eq.3*.]

[9] See *Almajest* 6.9, where Ptolemy justly criticizes Hipparchos for slips in seeking (or more likely confirming, as earlier guessed at Rawlins 1996C §D3) the draconitic motion from an apogee-perigee 579^{y}cycle eclipse-pair: −719/3/8 & −140/1/27. (If Hipparchos looked back another 579^{y} to the −1298/4/18 near-perigee partial eclipse, he found it occurred well below Babylon's horizon.) But his highly accurate synodic/draconitic ratio (eq.1) was not improved upon by Ptolemy or any other ancient. [Note (fn 7): the −1244/11/13 eclipse was much nearer apogee than the −719/3/8 one.]

[10] I have nothing against adjusting our findings to an earlier date, if future tests, e.g., discovery of an eq.1-based tablet dated to c.200 BC, point (like ACT 174, for ‡1 eq.1) to eq.1's currency around then (fn 10), hinting at, say, Babylon or Apollonios as originator. Note: our −140 date doesn't itself prove Hipparchos' authorship, since 13th century BC data's very antiquity can be seen as favoring invention by those with readiest access to early eclipses, namely Babylonians. If he took eq.1 from Babylon, he could've been using 13th century BC eclipse data unknowingly. But the theft-theory accepts a longshot coincidence: he mimicked another's apogee-perigee method for one eclipse-pair (579^{y}: fn 9), while stealing the fruit of the same method applied to a separate eclipse-pair (1103^{y}: §C2).

[11] But by modern theory, the matching −1298/10/12 eclipse's end most probably occurred well before Babylon moonrise. Plus: the −195 eclipse's anomaly was further from perigee [lunar anomaly $v = 6°$] than the neat −140 eclipse's [$v = -1°$]. The −176 & −171 eclipses are also rather mediocre in this regard: $v = 4°$ & $8°$, resp. [Another anomalistically inferior possibility: −231/12/4 [$v = 11°$] ($m = 0.19$) & −1334/9/21 [$v = 184°$] ($m = 0.29$).] But each of these pairs' 1st eclipse had v nearer apogee than did the −1244 & −140 pair, whose 2nd eclipse was nearest perigee. Yet, 2nd v being more knowable, its apse-proximity was primary to the ancients, which favors −140.

[12] Even aside from Hipparchan items [a]-[d]: there was no eq.3 eclipse-pair from −447 to −176 whose 2nd member was within 5° of the apse, further indicating that 200 BC cuneiform data based upon eq.1 were back-calculations; unless we say eq.1 was found c.500 BC but kept secret for 300^{y}.

D2 Thus, if §C9 is true, future historians should be less casual in tacitly assuming (as we all unthinkingly have, until now) that ACT were always written very near the time their data were calculated-for. After all, Babylonians were more into tabular calculations than observations (see, e.g., Neugebauer 1957 p.97); and back-calculations were common in antiquity (see, e.g., ACT 122 & 135 [Neugebauer 1955 1:144 & 161], Neugebauer 1975 p.525, probably Pliny 2.53), as they are today (e.g., Meeus & Mucke 1992).
D3 The theory that Hipparchos used a Babylonian eclipse-record of −1244 (or −1280: §D5) suggests his access to material which we have no hint was known to Aristarchos.[13] For years, Gerald Toomer and circle have proposed that Hipparchos had a strong Babylonian connexion. To explain the foregoing, it may not be absolutely necessary to accept Toomer's entire theory (especially the idea that Hipparchos' math-astronomy was nontrivially Babylonian), but it is nonetheless only fair (and in accord with the principles set forth at, e.g., *DIO 10* ⊙2&⊙21) to own that our new results suggest that Toomer has been more perceptive than we previously thought. So: we wish his theory good luck down the road, with respect to future indications and perhaps even solid verifications. [Vs. *DIO 13.1* §E4.]
D4 And, to help launch that hopefully productive journey, we ask: how is it that a small cluster of surviving 13th century BC eclipse data seems never to have become public, though evidently accessible to a privileged few, such as System A's inventor & Hipparchos? We have discussed previously (Rawlins 1999 fn 6) the controversial question of insider-secrecy in ancient science. Neugebauer 1957 p.144 suggests it's just a myth, even while owning that some Uruk astronomical cuneiform tablets state that they should only be shown to "the informed". How could Hipparchos have known of an apparently-private Babylonian record of the −1244/11/13 eclipse, unless he had close links to the priests of Babylon? Again, such considerations tend to favor[14] the credibility of Toomer's daring hypothesis.
D5 The foregoing has potential utility for present science: if Hipparchos really used a −1244 eclipse-report, this would set an upper limit upon the era's ΔT[15] (perhaps favoring secular quadratic over cubic-spline in Morrison & Stephenson 2004 Figs.2-3) at the value where the −1244/11/13 eclipse (also −1238/7/12's [fn 16]: did H use both?) would've ended for Babylon around moonrise. [But OK eastward: India, China.] Among the more interesting other approaches: possibly Hipparchos didn't use the −1244 & −140 eclipse-pair, but instead based his eq.1 upon another[16] viable eclipse-pair candidate on our §C2 list: −1280/10/23 & −176/1/6. This recourse carries the enticement that the latter pair's older eclipse (potentially underlying eq.1) is merely 7^{y} from our earlier-induced (‡2 eq.8) 13th century BC eclipse-record: −1273/12/5. And it would ease the cuneiform-calculation time-disjunct cited at §D1. I add these thoughts so as to provide all sides of the issue, even though I opt for the in-hand (guaranteed non-cloudy-weather) eclipse which we *know* Hipparchos observed (and used for just the sort of apogee-perigee analysis we're discussing), namely, that of −140/1/27; note also its superior anomaly (fn 11).

[13] If System A's 1010^{y} cycle was Aristarchos' 1st try, he dropped it for System B's better 345^{y} cycle.

[14] However, another possibility [see especially Rawlins 2003P §B etc] is that the early eclipses were publicly known — but later fell into disuse as more precise data became available. After all, this is not the first time we have encountered strong proof of important ancient data's existence — despite their total disappearance from the surviving literature. See, e.g., Hipparchos' EH & UH solar orbits (Rawlins 1991W §§K9&G10, respectively), and his several adopted obliquity-values (Diller 1934 & *DIO 4.2* p.56 Table 1, Rawlins 1982C eqs.27&28, Rawlins 1985G eq.9 & pp.262-263, Nadal & Brunet 1984 p.231, Jones 2002E) as well as the 781^{y} eclipse cycle (Rawlins 1996C eqs.20-31).

[15] The accepted spin rate's establishers don't claim tight validity back to 1300 BC, where ΔT's 2σ is ordmag 10°. [Speculation added 2002/9/29. Earliest [alleged] Chinese astronomical records: c.1300 BC; reliable ones: c.720 BC. Babylon eclipse-record chronology very similar. Linkage?]

[16] The −1238/7/12 & −135/9/24 pair is also possible, but rather less attractive (than other pairs considered here) because little of the −135/9/24 eclipse was visible in Hipparchos' Rhodos.

E Odds

E1 The technique used by *DIO* throughout the present papers and Rawlins 1996C is: tracing known ancient lunar period-relations to parent eclipse-cycles. *DIO* has done this for five precise long lunar cycles known to the ancients, of lengths: $1979^{u} = 160^{y}$ (Rawlins 1996C §I2 [& Jones 1999G]), $2729^{u} = 2961^{w}1/2$ (above, eq.2), $3277^{u} = 3512^{v}$ (Rawlins 1996C eq.10), $6247^{u} = 6695^{v}$ (‡2 eq.1), $9660^{u} = 781^{y}$ (Rawlins 1996C eq.21), where y = sidereal years. But could the here-alleged parent-relations be merely a set of accidents?

E2 One approach (parent→child) asks: how likely is it that each original directly-empirical eclipse cycle (eq.3 and ‡2 eq.2) just happened to have a common prime factor(s) on both sides of the equation? (Any such shared factor[s] was of course removed [by division] when the relation was published, to simplify&compact the ratios as much as possible, a perfectly natural mathematical step, but one which inadvertently left a *disguised* eclipse-cycle to posterity. E.g., the 345^{y} cycle, ‡1 eq.2, was of course divided by 17 to produce the famous and misleadingly [‡1 fn 5] roundish [but extremely accurate] simple relation: $251^{u} = 269^{v}$. See ‡1 fn 21.) For 2 numbers roughly of size N, the probability $D(N)$ that they Don't share a prime factor is nearer 50-50 than one might suspect. I find:

$$D(N) \rightarrow 6/\pi^2 \quad \text{as} \quad N \rightarrow \infty \tag{4}$$

(converging rapidly). Of the original empirical cycles we assert underlay §E1's relations, only one ($9660^{u} = 781^{y}$) doesn't primeshare; but the 2tailed probability, of chance deviation (from expectation: 3) by 2 or more prime-shares, is statistically insignificant (c.1/6).

E3 Further, §E2 may reflect a defective viewpoint: e.g., the most accurate submillennial period-relation for any given lunar motion will probably not happen to be an eclipse cycle — and thus it will require several recurrences (multiples) before an eclipse-return appears. Compared to §E2, this reflection starts us into an inverse perspective (child→parent), which will ultimately tell us whether our five results are chance or not: what are the odds that ‡2 eq.1 (obviously not itself an eclipse cycle) would have an unknown simple integral multiple (parent) that happens to be an eclipse cycle — just by pure chance? Well, for any given period-relation, the odds[17] are roughly 1/4 that eclipse-pairs are possible for it. Now, the relation in question is 505^{y} long — so the only possible multiplicative factors (short of a 1515^{y} base) are twofold: 1 & 2. Taking all such factors into account (see math at fn 17), and setting the dates for our §E1 relations as not later than, respectively, 25 BC, 120 BC, 160 AD, 49 BC, 160 AD, we can compute the net pure-chance probability ν that all 5 ancient period-relations would happen to have valid eclipse-cycle parents by pure chance. We do this for several retrosearch cutoff-dates (each given [in BC reckoning] at equation's left):

[17] E.g., in a relation such as ‡2 eq.2, the draconitic remainder could have been $0°\pm25°-$ or $180°\pm25°-$. But we are here stretching things rather too near the extreme outer bound of eclipse possibility: anything beyond about 23° would so limit an eclipse-pair's frequency that a relation thus founded would be valueless. Thus we will use $\pm22°1/2$, which allows 90°/360° or about 1/4 of the ecliptic for eclipse-pair-possibilities. Going back no further than 1300 BC for the 1st eclipse, for the 3277^{u} cycle (known from Ptolemy, 160 AD, 1460^{y} later than 1300 BC), there are (since $1460^{y}/265^{y}$ = 5.5) 5 potential parent cycles for this 265^{y} cycle, found just by multiplying the 3277^{u} relation (§E1) by 1, 2, 3, 4, or 5. (By including 1 here, we're bending over backwards not to stretch odds: after all, had the right-on-the-record-all-along period-relations [1979^{u}, 2729^{u}, 3277^{u}, 6247^{u}] been undisguised eclipse cycles, *they'd long since have been spotted as such*. Dropping 1 from their possible-multiple ranges ***more than doubles eqs.5-7's odds: to 1/72, 1/69, 1/30, resp***.) So the chance of thus fortuitously bumping into a valid eclipse-cycle is $1 - (3/4)^5 = 0.783$. Next, we find the odds on a multiple of eq.2 accidentally having $\Delta v = 0°$ or 180° (within 10°). Fraction of the zodiac involved = 1/9; so for this 220^{y} cycle (limit 1300 BC, 1180^{y} before 120 BC), since $1180^{y}/220^{y} = 5.4$, one can get a potential parent relation when multiplying eq.2 by any of 5 integers (1, 2, 3, 4, or 5); thus, the odds are $1 - (8/9)^5 = 0.445$ for hitting upon an integral or half-integral anomalistic return. In series, the odds against known lunar period-relations leading us to eclipse-cycles in all 5 cases, are significant (eqs.5-6) though not dramatic. The highest improbabilities here are otherwise-based: §E4.

$$1300{:}\ \ \nu = [1-(3/4)^7]\cdot[1-(8/9)^5]\cdot[1-(3/4)^5]\cdot[1-(3/4)^2]\cdot[1-(3/4)^1] = 1/31 \tag{5}$$

$$1400{:}\ \ \nu = [1-(3/4)^8]\cdot[1-(8/9)^5]\cdot[1-(3/4)^5]\cdot[1-(3/4)^2]\cdot[1-(3/4)^1] = 1/30 \tag{6}$$

$$1500{:}\ \ \nu = [1-(3/4)^9]\cdot[1-(8/9)^6]\cdot[1-(3/4)^6]\cdot[1-(3/4)^2]\cdot[1-(3/4)^2] = 1/14 \tag{7}$$

E4 So, unless we are prepared to fish beyond c.1400 BC (or 1564 BC: see big fn 17 parenthesis) it's unlikely at a statistically significant level (under 1/20) that our proposed (§E1) parent eclipse-cycles are accidental. And the results that have accumulated here draw credibility from other indicia, e.g., §D1 item [d]; also, what are the odds that independent analyses would narrowly (due to the involved cycles' delicate rarity) zero-in on two [now three: see *DIO 13.1* ‡2 §§E2&E3] early eclipses over 1000^{y} before the period under discussion, yet within just a few years of each other? Plus: we have a remarkable flock of Hipparchos-related coincidences (§C9); and we already know that Babylon used Hipparchan material (& vice-versa): Rawlins 1991H §§B11-B12 (& fn 10).

E5 A subtext point: what other compelling explanation (besides long eclipse-cycles) has ever come forth — during all the centuries these mysteries have lain unsolved — for the ancient equations which we have, in this paper and preceding ones (p.3 fn 2), produced potential solutions for? The main mystery now requiring explanation is: why does a certain cult keep cohesively & adamantly spurning an obviously attractive avenue? Why so unreceptive to our long-eclipse-cycle key to explaining both the origin & the high accuracy of: [1] the ancient draconitic month (eq.1), [2] the System A anomalistic month (‡2 eq.1), & [3] the System B synodic month (‡1 eq.2)? (Revealingly, some cultists can't admit even *the possibility* that *DIO* is right here.) I hope to see the day when historians will be asking aloud why this approach was ever particularly controversial. After all, *an ancient example of each of these 3 types of months is anciently* ***attested*** *to have been founded upon long eclipse-cycles*: [1] above at fn 9 (draconitic), [2] ‡1 eq.2 & fn 4 (anomalistic), [3] ‡1 §A10 (synodic). (The last attestation is of the very monthlength which is pointlessly in question! See ‡1 §A2.) One can wish that such precedents and explicit ancient testimony (as well as some idea of how astronomers actually think & work: ‡2 §A2) will ultimately cause appreciation (vs ‡2 fn 5) for what has been achieved here.

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Afterthoughts

This *DIO* issue produces unexpected & compelling new evidence that backs a favorite *DIO*-opposition viewpoint (a Hipparchos-Babylon connexion), suggesting an enormously greater astronomical-recordkeeping achievement (than hitherto known) by the very Babylonian culture so precious to said opposition. Thus, we have an open shot at shrinking some long-running, hitherto-intractable academic divides (p.3). Will the chance be seized?[1]

Today's formerly-dominant ancient-historian clique has 2 loves: C.Ptolemy and Babylon. But: what happens when one lobby clashes with another? (For parallel instance, see *DIO 4.3* ‡13 §G5.) Answer: it turns out that even the testimony of historians' longtime Greatest Astronomer of Antiquity, Ptolemy (whose reliability historians have for decades defended against *DIO*), has become vastly suspect and expendable.[2] Let's review the astonishing breadth (and temporal length & cranial-paleo thickness) of historian-denial here. In *Almajest* 4.2, Ptolemy explains how the best ancient monthlengths were achieved in pre-100 BC times. He there states that the Babylonian synodic and anomalistic monthlengths (‡1 eq.2) came from observations of the 4267 month eclipse-cycle. Certain historians don't believe it. He says that the Babylonian draconitic month (‡3 eq.1) came from observed equal-magnitude eclipse-cycle data. Same historians don't believe it. He says that this draconitic month was due to Hipparchos. Same historians don't believe it. Summing up: all the high-accuracy monthlengths Ptolemy reports (*Almajest* 4.2) from before his own time were directly based upon long eclipse-cycles. Same historians don't believe any were. And what ancient attestations of alternate methods' use do these stalwarts produce? None.

[1] [Note added 02/9/26f, long after 5/31 posting (www.dioi.org) of this *DIO* & its olivebranches (above, p.10, etc) & alerting of top Muffiosi to them.] Cliquish reaction rules on the present *DIO* issue and (more revealingly&enduringly) on far less reasonably-arguable (*DIO 11.2* p.33) Muffia-offending achievements. (*DIO* is reacting&nonreacting regretfully but aptly.) Esp. sad: [i] Muffia 2000-2002 failure to engage *DIO* despite *Isis*' & (with generous debate-terms) *DIO*'s invitations. [ii] The low-comprehension unappreciativeness of most of the all-Muffiose ref-reports upon Thurston's *Isis 93.1*:58 paper. (Though one report deemed *DIO 1.1* ‡6 [incl. our 1000000000-to-1 fit: above p.2] valid & "brilliant".) [iii] OG's all-too-typical "reply" (*Isis 93.1*:70) was 100% ungenerous. (See also *DIO 11.3* A3.) [iv] Gratuitous continuation at *JHA 33*:15-20 [2002] p.17 of unanimous rejection ***for 70 yrs now*** of A.Diller's greatest discovery, as *JHA* junks Diller's lovely 12 latitude-data-based triumph (key to sph-trig-inception chronology) & accurate ancient measure of the Earth's obliquity & trashes (n.9) a 13th datum ($12^h3/4$ klima) without noting Diller's theory fits it too, *on the nose*. All this in order to push a (*nonexclusive*) theory, based on 1 latitude's 1 datum, which doesn't even fit! (Thurston notes also solstice-equinox confusion: *JHA* 2002 p.15 line 6 [similarly at *ibid* p.16 line 4]; *JHA*-mythic-referee-hifi-déja vu: *DIO 1.3* ‡10.) Diller ironlock-vindicated & double-newdata confirmed: *DIO 4.2* p.56 Table 1; but as always uncited at *JHA 33*:15f [& *JHA 35*:71f].

[2] [Note added 2002/8/13-9/9.] As perceptive scholars will see right off, it's *inherently* likely that the several lunar periods here investigated were based upon huge eclipse cycles. I.e., the only serious question here is: *which* multiple of a relation recovers the underlying eclipse cycle, not *whether* eclipses are the foundation of extant pre-Ptolemy lunisolar relations. (Does any scholar really think that ancients didn't know about the 781^y & 800^y eclipse cycles?! — when: [a] one-fifth of the 800^y relation is anciently attested [‡3 §E1]; [b] we actually possess records of two famous ancient eclipses separated by 781^y [Rawlins 1996C §I5]; [c] the 781^y & 800^y cycles are arithmetically linked by the famous 19 yr cycle [‡1 eq.9]; [d] 800^y is an especially vital eclipse-related period, ***the shortest time in which lunar eclipses return to the same star*** [Rawlins 1996C §I1]; [e] the key 781^y lunisolar relation was known to Ptolemy [*ibid* eqs.27-31], so who'd say he didn't know it was an eclipse cycle?!) As noted (‡2 fn 2 & §A2), the relations' accuracy and the eclipse-cycle method's attestation-exclusivity should've made the truth clear long ago. (In general, the Muffia has [1] failed to find compelling solutions for the major parameters of its own field, [2] can't even *recognize* such when handed them, instead [3] shunning, suppressing, and-or slandering the discoverers.) Why would balanced, provident scholars reject the plainest path-to-solution *as utterly valueless* and thereby commit-for-life to an inevitably-doomed deny-deny policy? (Instruction via J.Bishop at Rawlins 2003J p.32.) Analogous to National Geographic (*DIO 10* §R3) & Gingerich (*DIO 11.3* ‡6 fnn 12 & 57) attempts to fool observers into accepting that *altering empirical-data records* is acceptable, "ingenious", and-or "brilliant"!