‡1 Testing Princetitute-Muffia Omertà

by givin' 'em a wee fit:

Equation 31

Long-Lost Vast Eclipse Cycles: 781 Years & 795 Years
Saros-Series-Closer Perigee Lunar Eclipse: –830 Feb 4
Was Earth-Orbit Apse-Motion Known in Antiquity?
When Did Babylon Start Tracking Saros-Series?
All Garnished with Yet More[1] MuffieMyth MirthBalls

by Rawlins[2]

Princeton-Institute-Muffia[3] History-of-Science Wisdom:

The conclusion[4] of the Muffia's late don-of-dons, Otto Neugebauer of the Princeton Institute for Advanced Study,[5] evidently extrapolating (to all antiquity) his frustration at a spent-lifetime of inability to relate his precious Babylonian astrological-cuneiform-text (ACT) ephemerides to specific observational bases:

> In all ancient astronomy . . . . the search for causes is as fruitless as in all other historical disciplines.

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[1] The causes of the present paper's pointed top title are discussed below at, e.g., fn 5 & fn 139. Most of the paper is devoted to presenting (& exploring the remarkable implications of) a burst of serious new findings regarding the empirical & math methods of ancient Greek astronomers. Nonetheless, those *DIO-J.HA*-followers, looking for the dependably entertaining math-antics of their favorite modern imps, will not be disappointed by the Muffia-circle jollies presented below at §B4 [Aaboe 1955], fn 66 [Menzel & Gingerich 1962], §H4 & fn 36 [both Neugebauer 1975], & §K3 [B.Goldstein & Bowen 1995] — which so convincingly prove that eminent Ivy League scholarship doesn't have to be dull. Or accurate. Or even plausible. (Or pronounceable: *Muffiemythmirthballth* . . . ?)

[2] Since physicist Robert Newton's 1991 death, physicist Rawlins has been the most hated figure in the History of science field. (See, e.g., *DIO 1.1* ‡1 fn 20, Rawlins 1991W §B1, & *DIO 4.2* ‡7 §§B9&B19. Also *DIO 4.3* ‡15 §C4.)

[3] The incomparable Ivy League "Muffia", comprising some of academe's most glamorous supermuddles, was introduced to *DIO* readers at *DIO 1.1* ‡1 §§C5, C7, & C12. See fn 5. Also "Black Affidavit" at *DIO 1.3* ‡10. And "Casting Pearls Before Pyglets" at *DIO 4.1* ‡4.

[4] Neugebauer 1975 p.107. (Similarly *ibid* pp.108&643.) Compare to fn 136.

[5] Due to its long association with O.Neugebauer, the Princeton Institute continues, uncomplainingly & unqualifiedly, to confer prestige and funding upon the skewed and-or outright-censorial output of the nest of Neugebauer-clonies whom *DIO* has reverently dubbed: the Muffia. See, e.g., fn 139, §M5, and Rawlins 1991W fnn 170&172.

On the other hand, DR has for well over a decade held that extant evidence indicates that Greek science was far more empirical[6] than has been generally believed by Historians of science (Hist.sci). The following paper bears critically upon this larger issue.

Muffia Omertà

Secret,[7] typically-suppressive 1993 pre-publication advice from the unfalsifiable Muffia, warning against a Cambridge University-trained mathematician's repeated citation (in a 1994 Springer book) of DR's finding of the first evidence of Greek influence upon Babylonian astronomy (discovery now widely-accepted,[8] despite the Muffia's worst efforts):

> Rawlins again! Always ingenious. Never convincing. I wouldn't quote him.

Gnawing Holes in the Dike

The generously-funded (*DIO 4.3* ‡15 fn 24) Muffia's ungenerous attempt to deny heretic DR all credit (for *any* contribution to ancient astronomy: fn 124) has the usual Dutchboy-dike-nightmare flavor that characterizes a classic coverup-history:

It starts with a hush,
And ends with a gush,
When holes outnumber fingers,
And kings run out of slush.

Our unevadable eq. 31 (below) punches yet another[9] fist-sized hole into the Muffia's ever-straining omertà-dike, the shoring-up of which continues to squander Hist.sci credibility & resources, and to require increasing doses of anti-ulcer strategy & Plastic-Manly acrobatics. Eq. 31 will add a further invigorating challenge to the cult's dedicated 26$^{\mathrm{y}}$ dirty-tricks[10] crusade to wipe heresy from the face of the Earth — and egg from the face of itself.

John Fauvel's 1994 Presidential address to the British Society for the History of Mathematics shares *DIO*'s liberal tolerance for strange speculation — but adds a crucial warning (highly recommended to certain R.Newton-haters: see *DIO 4.3* ‡15):

> A problem only arises when . . . proponents try to rule other approaches out of court in venomous and vicious ways which correspond, perhaps, to a Thatcherite handbag, an obstinate conviction that one is right and everyone else is wrong, in which humility, openness and gentle questioning are to be despised.

[6] E.g, Rawlins 1982G p.265 (& attendant correspondence with *Isis*). Also Rawlins 1987 p.236 (1) & p.237 (a). Throughout the present paper, the existence of high-level ancient Greek empirical science is repeatedly found to be consistent with our available evidence, including two ancient equations not previously solved: eqs. 10&31.

[7] We thank Springer-Verlag for its integrity in transmitting this gem of Muffia cultism. And for this venerable firm's intelligence and (more important) fairness in ignoring same.

[8] See below, fn 137.

[9] For other examples, see, e.g., *DIO*'s series, *Competence Held Hostage* (*DIO 4.1* onward).

[10] See, e.g., *DIO 4.3* ‡15 §B18; also *DIO 1.1* ‡1 §C7 & fn 20, and Rawlins 1991W §H2.

Summary

We find that ancient Greek astronomers, by using eclipse cycles about 7 to 8 centuries long (eqs. 8, 19, & 31), established-confirmed the sidereal, synodic, & anomalistic months to an accuracy of about 1 timesecond or better. The Babylonian part of the empirical base of this Greek math is shown (eq. 11 & §E6) to go back at least a century earlier than the oldest eclipse (−720) hitherto known to have been used by the Greeks. Our results also help establish (eqs. 29-31 & fn 115) the use of continued fractions during high antiquity. Additionally, we lodge two tentative suggestions: [a] that saros[11] series ("ss") of eclipses were being tracked at least as early as −830, and [b] that ancients had accurate knowledge of the solar anomalistic year. Further, the History-of-science center is challenged (§J2) to deny the significance of our astonishing match (to Ptolemy) at eq. 31.

A Hipparchos' Most Reliable Eclipse-Interval

A1 It is well-known that, for analyzing the synodic & anomalistic motions of the Moon, Hipparchos' basic empirical relation was the neat 345 yr cycle[12] (*Almajest* 4.2):

$$4267^{\mathrm{u}} = 4573^{\mathrm{v}} = 345^{\mathrm{g}} - 7^{\circ}1/2 = 4630^{\mathrm{w}}1/2 + 11^{\circ} = 126007^{\mathrm{d}}01^{\mathrm{h}} \tag{1}$$

(Superscripts here & below: d = days, h = hours, m = timeminutes. Lunar: u = synodic months, v = anomalistic months, w = draconitic months. Solar: g = anomalistic years [fn 38], y = tropical[13] years, y = sidereal years, K = Kallippic years [§F1]. Degree-remainders merely signify 360ths.)

A2 Ptolemy says that this relation[14] was the source of Hipparchos' value for the length of the synodic (calendaric) month:

$$M_{\mathrm{A}} = 29^{\mathrm{d}}31'50''08'''20'''' = 29^{\mathrm{d}}.53059413580 \tag{2}$$

A3 Kugler 1900 and the Neugebauer-Muffia have contradicted Ptolemy by claiming that eq. 2 was taken by Hipparchos from the Babylonian "System B". (Eq. 2 is indeed found on Babylonian cuneiform texts.) And it has frequently been noted (at least since Copernicus) that dividing 4267^{u} into $126007^{\mathrm{d}}01^{\mathrm{h}}$ doesn't quite produce eq. 2. (Situation clearly explained at Aaboe 1955 p.122 & Neugebauer 1975 p.310.) For these two reasons, the Muffia rejects Ptolemy's account. (See, e.g., Toomer *1984 Almajest* p.176 n.10.)

A4 By contrast, Rawlins 1991H §B10 has argued that Ptolemy was essentially right in connecting eq. 2 to Greek reasoning (eq. 1). (This finding tends to vindicate the cautious warnings of Dicks 1994 §B2.)

[11] In this paper, I use the word "saros" to signify the interval of eq. 14; and "saros series" is abbreviated "ss".

[12] Several equations in this paper relate successive quantities (e.g., synodic months, anomalistic months, draconitic months, anomalistic years, & days), using serial equals-signs. It should be stated explicitly that, in each such serial equation, all quantities (past the first) are computed directly from the number of synodic months. E.g., in eq. 11, 290315d07h is found by multiplying U = 29d.530595 (the real length of the synodic month in −323, in solar days of that epoch) by 9831 — not, e.g., by multiplying the length of the anomalistic year by (795 − 65/360). Other real lunar periods for Phil 1 (−323/11/12): anom mo V = 27d.554584, drac mo W = 27d.212222, sid mo S = 27d.321668. (In these equations [also in, e.g., eqs. 22&23], equality is not meant to be exact: it holds only to the precision displayed.)

[13] Tropical-years here can refer to real ones (§F2) or the Metonically-defined "tropical" year (eq. 30: $1^{\mathrm{y}} \equiv 235^{\mathrm{u}}/19$), which (as suspected since T.Mayer and now justly emphasized by N.Swerdlow & K.Moesgaard) leads nearly to eq. 16 (fn 43), the direct empirical basis of which was the S.Solstices of −279 & −134 (Rawlins 1991H eq.8).

[14] In eq. 2, the last few decimal digits would be superfluous even were the last sexagesimal place accurate. Several other values (e.g., eqs. 8 & 13) are also rendered here in varying degrees of overprecision.

A5 Rawlins 1991H §A5 traced to Hipparchos[15] Babylon's "System B" yearlength[16] on one of the major cuneiform tablets containing eq. 2, and Rawlins 1985S showed how inclusion of ancient rounding (during the math descent) indeed could have permitted eq. 2 to have been derived from eq. 1, just as Ptolemy said.

A6 But Rawlins 1985S suggested that the astronomer who actually used eq. 1 to establish eq. 2 was Aristarchos (fl. c.280 BC). Rawlins 1985S specifically proposed that the lunar eclipses of −620/4/22 and −275/4/18 may[17] have formed the particular ancient 345 yr-interval (see eq. 1) on which was founded[18] eq. 2, an amazingly accurate value — correct (then *and now*) to a fraction of a timesecond. Its accuracy in antiquity was 1 part in ordmag 10 million. (See fn 12, Rawlins 1991H fn 1.)

A7 We conclude this preface by reminding readers that (see *Almajest* 4.2) the 345 yr cycle is exactly 17 repeats of the simpler, more familiar[19] equation:

$$251^{\mathrm{u}} = 269^{\mathrm{v}} \tag{3}$$

B His Lardship Sweetens Yale's Rep with Fudge-Babylonienne

B1 But there is a longstanding mystery about eq. 1: the $-7^\circ 1/2$ solar-motion remainder is discordant with respect to any yearlength hitherto known to have been used by the ancients. The discrepancy has been frequently noted.[20]

B2 At length, Yale University's A.Aaboe 1955 (pp.123-124) made the clever[21] discovery that the $-7^\circ 1/2$ remainder in eq. 1 could almost be explained by assuming use of the fundamental[22] Babylonian ratio of yearlength[23] to monthlength:

$$1^y = 12^{\mathrm{u}}22'08'' \tag{4}$$

Almost.

B3 The charmingly plausible Aaboe, an enormous mathematical talent, has since become the venerable loving-papa, guru-mentor, & shepherd of current lamb-brained Muffiadum: an above-it-all, *Prince*ly architect of its public-noncitation-cum-private-slander strategy[24] towards heretics. For openmindedness on central sacred tenets, Aaboe can match the real pope.

B4 Through the very same forced-math approach previously perfected[25] by his own mentor, Princeton Institute's Otto Neugebauer, Aaboe 1955 calculated sexagesimally (using eqs. 1&4) as follows:

$$4267^{\mathrm{u}}/(12^{\mathrm{u}}22'08''/1^y) = 344^y58'42''\ldots = 345^y - 0^y01'17''\ldots = 345^y - 7^\circ 42' \tag{5}$$

and he then rounded the remainder to the nearest $1^\circ/2$ to find $-7^\circ 1/2$. This rendition was printed by *Centaurus*, which noted nothing amiss. (Aaboe is now on *Centaurus*' Board.) It was then copied by Pedersen 1974 p.163, who helpfully omitted the sly ellipses with which Aaboe had larded eq. 5 — though these are the key to (& cover for) Aaboe's deft illusion. (Aaboe, Pedersen, & *Centaurus* are all from: the state of Denmark.)

B5 For, unless one follows carefully, one can miss the trick: the sexagesimal remainder in the middle of eq. 5 ought to be $1'18''$, not $1'17''$. Simple subtraction. (See Rawlins 1991W §G9 comments on: [a] Aaboe-protégé N.Swerdlow's sneers at E.Rosen's errors of arithmetic, and [b] the glass-house irony of stoner-age Muffies. See also fn 28 on Muffia outrage at noncultists' alleged academic dishonesty!) Why do Muffiosi have such a magic-touch penchant[26] for this sort of thing? (Note that MacArthur Fellow [see *DIO 4.3* ‡15 fn 24] Swerdlow's most polished forced-math gem, cited at *DIO 1.1* ‡5 fn 7, debuted in a thesis heavily assisted by Aaboe. The world loves a quick learner.) Answer: some moderns (§B5, §K2, & fn 128) are as determined to find Babylonian influence[27] in Greek astronomy as their hero C.Ptolemy was determined to discern his own theories embedded in recalcitrant reality — so, when the need arises, they will resort to the same math methods:

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[15] See fn 137. It is an indication of how highly Hipparchos was regarded in antiquity that his −134 S.Solstice had become internationally accepted on a level with Meton's revered −431 observation.

[16] Even the Muffia's 1st pope calls this yearlength part of System B (Neugebauer 1955 p.200), which thus independently supports DR's suggestion that Babylonian astrologers' System B was derived from Hellenistic science: fn 15 & Rawlins 1985S.

[17] The −620/4/22 eclipse is one of only four pre-600 BC eclipses preserved by Ptolemy (*Almajest* 5.14). The middle of the −275/4/18 total eclipse was below the horizon at Alexandria, but the eclipse started about 18° above the horizon, at 4 AM Local Mean Time there. Just adding a rough total eclipse semi-duration of 110^{m} or 120^{m} would then provide the correct time of mid-eclipse to within about 10^{m}. (And reports from sites further west could have improved the firmness of the mid-eclipse time.)

[18] Hipparchos almost certainly used the attested 345 yr-pair: −490/4/25-26 (*Almajest* 4.9) & −145/4/21-22 (*Almajest* 3.1). Though eq. 3 may already have been known, the stability of eq. 1's 345 yr time-interval would show its superiority vs. other multiples (fn 19) of eq. 3. This consideration reminds us that no one eclipse-pair could establish eq. 1; either trios were used at both ends (to establish anomalies), or (far more likely in my opinion) examination of several 345 yr-pairs revealed the striking fact that eq. 1's interval (126007d01h) was virtually identical for all such pairs, *observably* varying less than an hour (fn 56): an ordmag 1-in-10 million constancy that is very probably (though see fn 30) the direct, convenient basis of the even more accurate eq. 2. Curiously, it has been little noted (probably due to Ptolemy's unlearned preference for non-period-return "proofs": fn 51) that the astonishing constancy (fn 56) of eq. 1's 345 yr interval was, historically, *the* critical, unevadable, plainly *visible* (not theoretical) empirical evidence for *the secular stability of lunar mean motions*. The fact that lunar periods were remarkably (§A6) constant (despite the seeming untamability of confusing short-term lunar wanderings) is one of the most important of all ancient (or modern) astronomical discoveries.

[19] Eq. 3 (sometimes credited to Kidenas ≡ Kidinnu: see van der Waerden 1974 p.240 & Neugebauer 1975 p.611 n.36) was probably suspected long before eq. 1. (However, see fn 18.) Note that eclipses can occur for much shorter multiples of eq. 3 than 17, namely, for 4 & 9 repeats. Moreover, there is much better draconitic commensurability (than for eq. 1) at 13 repeats (3263^{u}), though solar anomalistic commensurability is ordmag 10 times worse, since 3263^{u} equals about $263^{\mathrm{g}} - 69^\circ$. Finally, I note that 22 repeats = 5522^{u} = 5918^{v} = $5992^{\mathrm{w}}1/2 - 17^\circ$ = $446^{\mathrm{g}}1/2 - 20^\circ$. (So Aristarchos might have been able to use the −719/9/1-2 and −272/2/15-16 eclipses as a rough foundation for eq. 3 — if he could correct for solar anomaly differential.) But this is not as helpful a relation as eq. 1.

[20] At least since Manitius 1912-3 1:196 note b. Taking Phil 1 as epoch: for 4267 real synodic months, eq. 1's solar remainder would have been $-6^\circ 1/3$ for the real sidereal year, $-1^\circ.6$ for the real tropical year, and -3° for the Hipparchos-Ptolemy year.

[21] DR has long admired Aaboe's intellectual gifts. See, e.g., *DIO 4.3* ‡15 fn 8 & Rawlins 1987 n.35. On 1994/5/8, Aaboe perceptively noted (at the Dibner Inst conference at MIT) that a nice way of finding Hipparchos' Autumn & Winter seasonlengths (88d1/8 & 90d1/8, resp) is by subtracting Spring & Summer seasonlengths (94d1/2 & 92d1/2, resp) from (365d1/4)/2. This is quicker than Ptolemy's *Almajest* 3.4 math, & the results are identical. (See also fn 44.)

[22] See, e.g., fn 66 & van der Waerden 1974 pp.106, 232.

[23] We indicate sidereal years in eq. 4, though we don't know what kind of years were intended by the System A Babylonian astrologers who used the equation. Indeed, we don't even know if *they* knew (or cared) what kind of year the equation devolved from.

[24] All right, so it doesn't take a Klausewitz to anticipate that academe's vaunted archons will do absolutely nothing to curtail such medieval, reason-evading behavior. (When arrogant academic cults suppress evidence-based discussion, they leave no rational avenue for idea-evolution. So, unsurprisingly, the banned parties occasionally react along extra-rational avenues. E.g., *DIO 4.3* ‡12 fn 4.)

[25] See the three forced-math entries under Neugebauer in "Casting Pearls Before Pyglets", *DIO 4.1* ‡4 ("Muffia Muff-Catalog: the Incompetence-Chargers' Competence"). But note also his distancing himself (see §B7 & end of fn 29) from Aaboe's egregious eq. 5 sleight.

[26] For catalogs of other entertaining instances of Muffia (& Princeton Inst) fudge, see "Black Affidavit" at *DIO 1.3* ‡10, and "Casting Pearls Before Pyglets" at *DIO 4.1* ‡4. (Note that the Princeton Inst is named for the town and has no more official relation to Princeton University than the Stanford Research Institute has to Stanford University.) Meantime, Princetitute-supported slander-scholarship continues (e.g., Britton 1992 p.xvi) to bluff-suggest that dissenters' work is massively error-riddled — but, when challenged (Rawlins 1991W fn 252) to reveal the purportedly enormous List-Here-in-My-Hand of actual heretics' errors, Muffiosi have for years stayed as secretive as Joe McCarthy.

[27] Question: has a single case of unattributed Greek use of Babylonian astronomy ever been established? (Why would Greeks even bother to hide their use of such elementary work? — which was hardly worth stealing.) We know about use of Babylonian material in the *Almajest* (see, e.g., Dicks 1994 §D1); and of Babylonian math in the lesser work of Geminos 18.9. See also Neugebauer 1975 p.601. But all of this is openly cited. So, why do Muffiosi assume that large unattributed borrowings went on? For the hyperwispy, sometimes miscomputed nature of the few alleged Muffia proofs of said borrowings, see, e.g., §B5 and Rawlins 1991W §§G2-G9 & fn 73, and below here at §K. (Note contrast to DR's fn 46 suggestion of hidden use of heliocentrist work: nobody ever persecuted a Greek for using Babylonian astronomy, but heliocentrism was a dangerous heresy in antiquity. As later during the Dark Ages.)

force & artifice. (Princetitution-funded Britton 1992 loyally repeats the Muffia's: [a] attack on RN's honesty[28] [p.xvi] & [b] the cult-speculation-become-cult-fact [pp.x & 132 n.4] that Greek lunar theory's mean motions came from Babylon [see here at fn 128] — contra Dicks 1994, e.g., fn 46.) Note that it was A.Aaboe who (1976/3/9) called R.Newton's research: "incompetent work in my realm". (See *DIO 4.3* ‡15 §G9.) For the Muffia's own dazzling competence here (which accounts for this paper's *J.Hyster.Astron* co-header), see fn 1.

B6 If the eq. 5 error is corrected, the remainder instead comes out as $-7°46'$ — which would hardly be rounded to $-7°1/2$. Indeed, in antiquity, either expression ($-7°42'$ or $-7°46'$) would just be rounded to $-7°3/4$, not to $-7°1/2$ (or $-8°$).[29]

B7 It is to the credit of Neugebauer 1975 (p.312) that he later cleans up after this error. (Not wishing to embarrass his own protégé, he doesn't expose Aaboe's fudging.) But he then acts as if rounding $-7°46'$ to $-7°1/2$ is nothing much, alleging that Ptolemy reported (*Almajest* 4.2) that Hipparchos "rounded this deficit to" $7°1/2$. In fact, Ptolemy nowhere states that Hipparchos rounded anything here.

C Old Question: Why Is Eq.1's $-7°1/2$ Remainder Incorrect? New Answer: It Isn't.

C1 On 1995/4/23 (even while editing this paper), I independently came upon an explanation of the $-7°1/2$ remainder which: [a] fits it to well within normal ancient rounding precision (i.e., no Yale trickery required), and [b] suggests the hitherto-unsuspected theory that the ancients had made a fundamental discovery, namely, the apsidal precession of the Earth (or, for the geocentrists, of the Sun). Note well: ancient eclipse-analysts would have had more motive than anyone to know the motion of the Earth's apogee, since (as Ptolemy says at *Almajest* 4.2) solar anomaly-inequalities hindered their search for integral-return eclipse cycles. (The smallness of the solar remainder in eq. 1 was primarily what made it preferable to eq. 11 or eq. 12 — and to the various cycles cited in fnn 19&57.)[30]

C2 It seems that the solar apogee A was placed near 60° (fn 44) by Kallippos (330 BC, when the correct A was c.63°), though (*ibid* fn 199) the mature Hipparchos put it at 65° and later at 67° (146 BC & 128 BC, resp, when the real A = c.66°1/2: Rawlins 1991H §C8). In the recent excellent[31] paper, van Dalen 1994, it is proved that the *Almajest* equation-of-time table is based upon apogee A = 66°, and van Dalen conjectures (p.116 & n.24) that this could be due to Serapion (c.1st century BC). (The author[32] & DR agree that 66° could well be just a convenient rounding of 65° or Ptolemy's 65°1/2. But it is also possible that this is one of a series of successively increasing values ancient astronomers used for A over the centuries.)

[28] E.g., Rawlins 1991W fn 252 & *DIO 1.1* ‡3 §D3.

[29] Neugebauer 1975 p.312 seeks solace by citing an attribution of $-8°$ to Hipparchos in an unreliable (*ibid* p.310 n.6) late Greek source. But $-8°$ is a rounding of $-7°1/2$, not vice-versa. And this does not explain the Hipparchos version reported at *Almajest* 4.2. However, considering his own obsession with tying Greek work to Babylonian (Rawlins 1991W fn 73), ON deserves credit for showing (Neugebauer 1975 p.298) that the explanation for eq. 1's $-7°1/2$ remainder could be Greek.

[30] Best ordmag-1000^{y} synodic-anomalistic cycle: $16092^{u} = 17246^{v} = 17463^{w} - 9° = 1301^{g} + 3°$ = 475206d08h (double last cycle in fn 57). Evident nondiscovery of this cycle in antiquity lends support to the position that regular eclipse records did not go back into the 2nd millennium BC, contra DR's §§H1&H6 speculation. (The extremely high accuracy of eq. 2 was more consistent with the amplitude of the 1301^{g} cycle's variations than with those of eq. 1: fn 56. But averaging several 345^{g} pair-intervals would (fn 18) produce comparable accuracy.)

[31] I particularly admire van Dalen's exemplary perfectionism, which his Table 4 (p.131) typifies — and which is directly responsible for ultimately producing a perfect reconstruction of a complex ancient procedure. However, the paper's credits at p.149 n.1 exhibit several problems, which I have informed him of, without reply. (See also *DIO 4.3* ‡14 fn 4.)

[32] See the reasoning of van Dalen 1994 pp.127&136.

C3 Extant ancient information supports this hypothesis only in a crude way. (No solar A values survive directly from the 3rd century BC.) The A values cited at §C2 are not highly accurate; but they at least suggest that there was awareness of the secular increase of A: the rough pace and sign of the difference between the A values of Kallippos and of Hipparchos supports the general thesis that there was. And their contemporaries may have been more accurate yet: §C13. Note: since geocentrists' large (conveniently Sun-shrinking: Rawlins 1991W fn 280 & §R14) parallax-guesstimates would degrade the accuracy of apogee-determination, it is reasonable[33] to ascribe to ancient heliocentrists the here-proposed discovery of correct solar apogee-precession.

C4 The fact that no (surviving) ancient astronomical text mentions this is not critical, as the case of the Earth's obliquity shows: [a] The obliquity (23°2/3) used by genuine ancient astronomers is nowhere directly attested in extant works. (See Table 1 of *Competence Held Hostage #2* at *DIO 4.2* p.56.) [b] The fact that the obliquity was gradually decreasing is also not found in any surviving ancient astronomy text. Yet the accuracy of values used by Hipparchos (& perhaps earlier by Eudoxos)[34] suggests that ancient scientists could have recognized the obliquity's decline. And, at Plutarch *Moralia* 411A, we find an explicit statement (though in a strange context) that the obliquity was decreasing.

C5 *Almajest* 4.2 treats the $-7°1/2$ remainder in eq. 1 as if it is longitudinal; not only longitudinal but: *sidereal* longitudinal. (See *Almajest* 4.2: "with respect to the fixed stars"; transl. of Toomer 1984 p.175.) This is patently inconsistent with Ptolemy's solar orbit, whose 65°1/2 apogees's constancy is tropical, not sidereal.[35]

C6 Ptolemy does not tell us what Hipparchos' opinion was on this point. However, the same chapter also emphasizes what is important about a cycle's solar remainder, namely: the discrepancy in *solar anomaly*. I.e., in an intelligent ancient's rendition of eq. 1, $-7°1/2$ would be solar anomaly, not longitude. Startling fact: *the $-7°1/2$ remainder is correct for solar anomaly, not for longitude.* Only one potentially contentious question remains: was this correctness due to Greek skill or to luck? If the former, then high Greek astronomy was more advanced than previously believed by anyone — most definitely including myself.

C7 One interpretation of *Almajest* 4.2 is that Hipparchos' solar orbit precessed, unlike Ptolemy's. A further refinement on such theorizing: did Hipparchos identify the Sun's anomalistic motion with its sidereal motion? (This hypothesis would eliminate the §C5 inconsistency of Ptolemy's references to the latter instead of the former when speaking of the $-7°1/2$ remainder.)

C8 We do not know.[36] But, fortunately, we do not need to know, because: the stars have nothing to do with eclipse periods. (The stars could all be tripping the trepidation[37] tango, without any effect on eclipses, if the solar & lunar models are independently established: fn 38. The only relevance here occurs if the solar apse was presumed to precess with the stars: §C7.) Again, for eclipse analysis, the only aspect of solar motion that matters is: anomalistic. Indeed, the best way to express §C6's point is in the form of a common-sense question (which seems so obvious in hindsight): wouldn't the heart of eq. 1's 4267^{u} relation — namely, $4573^{v} = 345^{g} - 7°1/2$ — be unacceptably hybrid & inconsistent if it used anomalistic returns for the Moon, but not for the Sun?

[33] However, fn 39's method might be used by either side of the helio-vs-geo-centrist controversy.

[34] See Rawlins 1982C eq.28 & fn 9.

[35] *Almajest* 4.2 also speaks of eq. 14 as sidereal, which (fn 66) it is not. (I.e., use of the sidereal year in eq. 14 will not produce the 10°2/3 remainder cited.) But it is not unusual for us to find that Ptolemy does not understand the basis of his own material. See, e.g., the periods of the planets: fn 51 and *DIO 2.1* ‡3 fnn 16, 36 & 38.

[36] Neugebauer 1975 pp.293&298 suggests 2 different possible values for Hipparchos' sid yr: 365d1/4 + 1/144 & 365d1/4 + 1/100, adding that Hipparchos may've believed the sid yr inconstant. [ON (who flays others' anachronisms) commits 2 sins at *ibid* p.1083: [a] Giving the modern (not ancient: fn 38) anom yr. [b] Rounding this AENA 1900 value, 365d.25964134 (≈ 365d1/4 + 1/104), decimally to 365d.2596 & then expressing it sexagesimally as 365d15′34″33‴36⁗, c.1000 times more precise than accurate. Same muffs (*idem*) for both trop&sid years; p.1084 exhibits similar (less severe) oddities for lunar periods, plus misprinting 16″ as 18″ in 2nd anom mo expression.]

[37] See fn 36, Neugebauer 1975 p.298, & *DIO 3* fn 29.

C9 Thus, let us test quantitatively the hypothesis[38] that the ancients knew[39] the Sun's anomalistic motion; we start by proposing[40] an accurate value G_{s} for the solar anomalistic year, rounded in typical ancient[41] fashion:

$$G_{\mathrm{s}} = 365^{\mathrm{d}}1/4 + 1/100 \tag{6}$$

and divide it into eq. 1:

$$4267 M_{\mathrm{A}}/G_{\mathrm{s}} = 345^{\mathrm{g}} - 7^\circ 33' - \tag{7}$$

— where we recall (§A1) that superscript g = solar anomalistic years.

C10 If we had (in eq. 7) instead used the actual[42] (unrounded) synodic month & anomalistic year, then (fn 38), the deduced remainder would have been $-7^\circ 28'$.

C11 There is no difficulty at all in believing that an ancient scientist rounded either result (eq. 7 or §C10) to $-7^\circ 1/2$. (We see that eq. 1 implies a G_{s} value accurate to ordmag 10^{s}, almost as accurate as Aristarchos' sidereal[43] year.)

C12 Thus, two elementary considerations recommend our speculation that the ancients knew the solar anomalistic year: [a] It fits eq. 1's remainder without any forcing (or even an assumption of prejudice-convenient ancient observational or theoretical error), while no other hypothesis does. [b] *The anomalistic year is the only year that is in fact mathematically relevant to that remainder* (§§C6&C8). The coincidence of [a]&[b] may not be proof, but it is attractive. [Note added 2018. Papyrus *P.Fouad* 267A bears a solar-motion column consistent with yearlength $365^{\mathrm{d}} + 1^{\mathrm{d}}/(102\ 2/3)$: Rawlins 2018U §K5.]

C13 And this adds more credibility to the position that the famous geocentrist astrologers, Hipparchos & Ptolemy, drew much of their astronomy from often-unnamed[44] but able[45] astronomers (probably heliocentrist)[46] — not politically well-connected — whose high-technical-level research texts were much less widely circulated and thus now lost (Rawlins 1984A p.984).

C14 So, in sum, we have the edifying spectacle of top modern ancient astronomy experts spending almost a century puzzling over — and conjuring up the most ingenious (even forced: eq. 5) theories to explain — the large error of eq. 1's $-7^\circ 1/2$ remainder. And now it turns out that: there never was an error that needed explaining. . . . (Note the parallel to the "mystery" of the disparate metrologies supposedly underlying Eratosthenes' & Poseidonios' differing Earth-size values, a discrepancy-problem which dozens of analysts persistently worried for over a century [ignoring intelligent warnings by D.Dicks & O.Neugebauer], a mystery which eventually turned out to have a simple, purely physical[47] not metrological cause.)

C15 Anybody out there still wondering why I find the history of ancient astronomy so endlessly surprising, fascinating, & rewarding? And why I don't take entirely seriously everything The Experts say? (See *DIO 4.3* ‡15 §H12.)

D Doubling the 345^{g} Interval

D1 Returning to the question of the empirical base for eq. 1: whether Hipparchos was discovering or checking it, he might (fn 18) have preferred having as long an interval as possible, since that is astronomers' secret for determining accurate heavenly motions. The catch with the eq. 1 cycle is that (as Ptolemy correctly points out at *Almajest* 4.2) the number of draconitic months corresponding to $126007^{\mathrm{d}}01^{\mathrm{h}}$ is not quite semi-integral: as one sees from eq. 1, it misses precisely $4630^{\mathrm{w}}1/2$ by over 11°. Since (even for lunar perigee) an eclipse can only occur within c.12° of a lunar node, there will be few eclipses having a 345^{g}-cycle-match two cycles ago: 690^{g} into the past. (Odds against: ordmag 1-in-100.)

D2 Yet, of the (merely three) known eclipse observations by Hipparchos, one of them, that of $-140/1/27$ (partial, magnitude m = 3.1 digits at the S.limb: *Almajest* 6.5&9), is in fact two cycles of 345^{g} (eq. 1) later than an earlier eclipse (visible at Babylon), namely, that of $-830/2/4$ (partial, m = 0.5 digits at the N.limb).[48]

D3 Moreover, since both of these two eclipses occurred very near lunar perigee, we can point to Hipparchan precedent: at *Almajest* 6.9 (& 4.2), Ptolemy reports Hipparchos' use of a pair of near[49] apsidal-line eclipses (one of them the -140 eclipse, again), to determine the

[38] Ptolemy did not recognize the precession of the solar apse. (But he also did not know either the correct obliquity or its temporal variation — or even the *fact* that it varied.) Stronger marks (than these) against our hypothesis: [i] Many ancients had trouble finding the tropical year (fn 43); fixing the anomalistic year (actual value, for Phil 1-epoch, by Newcomb's solar theory: 365d1/4+1/102) would be tougher yet. [ii] The ancients knew the Moon's anomalistic motion only to a precision of c.$1^\circ/4$ per 100^{y} (even though the Moon's orbital eccentricity was more than thrice Earth's), but this error is approximately the size of the difference between the precessions of the solar apse & the stars. [iii] Rawlins 1991W §N5 estimates an error of nearly 5° in 300 BC astronomers' estimate of the lunar apogee. However, on the other hand: [a] Finding solar anomalistic motion is simpler than lunar (not dependent upon a blizzard of possible cycles). [b] It was civil-calendar considerations that wrenched (fn 13) the tropical year to fit eq. 30, but such factors were irrelevant to either the anomalistic year or the sidereal year, which were of no popular interest whatever. This contrast — and the known high accuracy of ancients' adopted sidereal yearlengths — encourages the theory that the ancients also had an accurate value for the (equally non-civil) anomalistic yearlength. (Aristarchos' sidereal yearlength was ordmag 100 times better than his tropical yearlength: fn 43.) [c] A hypothetical ancient scientist who determined solar anomalistic motion need not have known anything about stellar precession in order to obtain the correct remainder in eq. 1. [d] If Neugebauer 1975 p.298 can speculate, from eq. 1, that the ancients may have had an accurate figure for the precession of the equinoxes (though both the implied sidereal & tropical years he proposes are highly inaccurate — even while ancient values for the former are known to have been very accurate: item [b] above & Neugebauer 1975 p.601), then the at-least-as-credible speculations here ought to be permissible.

[39] See §C4, §C13, & fn 46. [Did ancients do fn 56's parenthetical math in reverse to find $-7^\circ 1/2$ and thus G_{S} via eq. 7?]

[40] If the "σ'[ ]" at the end of the table of yearlengths relayed at Neugebauer 1975 p.601 is actually a fragment of an otherwise-lost word, then the yearlength cited equals eq. 6.

[41] Specifically: Hipparchan rounding. (Compare to eq. 16.) I see that precisely eq. 6 is provided at Neugebauer 1975 p.298, but is there called the sidereal year. See §C7.

[42] According to the American Ephemeris version of the Brown-Newcomb luni-solar theory. (Adjusting for Earth-spin acceleration is obviously needless in this case.)

[43] Rawlins 1991H fn 1.

[44] Kallippos (Aristotle's astronomer) was famous, yet even his solar orbit hasn't survived directly. It is reconstructed at Rawlins 1991W (fn 152) from his Spring&Summer lengths (Autumn&Winter were likely found by the neat method of Aaboe: fn 21), yielding A = c.60°, consistent (*idem*) with Sample A' of the Ancient Star Catalog. (Though, van der Waerden 1988 pp.88f makes an intelligent case for $A = 75^\circ$.) This reconstructed orbit was pretty accurate (§C2, Neugebauer 1975 p.627, & *DIO 1.1* ‡5 fn 13); indeed, its error-wave-amplitude appears to have been less than that of any of Hipparchos' three successive solar orbits (*ibid* §§G10, K10, & K9).

[45] See, e.g., Rawlins 1982G, Rawlins 1985G, Rawlins 1987 (p.238 & fn 38). Also Rawlins 1991W §§K4, N17, eqs.23&24.

[46] See §C3 & fn 45. Also Rawlins 1991W §§O2, O4, & O6, & the comparison-table at §P2.

[47] See, e.g., *DIO 4.1* p.2 (News Note A). Not that DR's tidy, entirely novel (physical) solution of the problem is likely to cure the stade-scrunching-for-Eratosthenes tribe's incurable passion for the uncurious mission of: juggling evidence to keep looking for an ad-hoc traditional solution to only *one* separate half of a problem where *both* halves have already been neatly solved together (untraditionally). Dutka 1993: [a] Makes Eratosthenes "right" by arguing (pp.63-64) for Hultsch's reconstructed stade of 158 m and claims (p.56) that the well-established 185 m stade = 1/8 Roman miles (adopted in Rawlins 1982N App.A&B) was widely used only centuries after Eratosthenes — this despite the uncooperative fact that the reliable Greco-Roman historian-ambassador Polybios, whose life overlapped Eratosthenes', testifies (*Hist* 3.39.8) that the Romans marked their miles every 8 stades. (So, c.200 BC, there was no serious uncertainty to the stade.) [b] Fails to cite the critical point that DR's theory (ascribing each ancient value's error to atm refraction) simultaneously solves (to high precision: ordmag 1%) *both* the (very discrepant) Eratosthenes & Poseidonios values, 252,000 st & 180,000 st, resp. (And this is accomplished by using a *single* value for the stade: the same standard, wellknown 185 m value found even in most dictionaries. See *DIO 2.3* ‡8 §A. [Typo at §A8: for 252,200 read 252,000.] Also *DIO 4.2* ‡9 §M.) No other simple, coherent theory does so. Dutka 1993 p.64 claims that the reason for the 180,000 st value's lowness is not known. He might've instead noted: [i] a coherent explanation exists for both figures, but [ii] he prefers the theory that explains only one of the figures.

[48] Both magnitudes are *DIO* calculations, as are the following. The $-830/2/4$ mid-eclipse was at 20:57 Babylon mean time (20:39 Babylon apparent time), at $\lambda = 129^\circ.0$ & $\beta = -1^\circ.2$, near 77σ Leo. (DR's calculations of eclipse times here & at, e.g., fn 64 are subject to c.1/4 hr uncertainty.) The $-140/1/27$ mid-eclipse was at 21:58 Rhodos mean time (21:42 Rhodos apparent time), at $\lambda = 125^\circ.2$ & $\beta = +0^\circ.7$, less than 3° east of Regulus. Both positions topocentric and E&E of date — both λ subject to a few arcmin of uncertainty; and the cited times are subject to non-independent uncertainties (slightly larger for the -830 event) of a few tenths of an hour.

[49] Actually, whereas the -140 eclipse is only 1° from perigee (fn 65), the -719 eclipse (which *Almajest* 6.9 says Hipparchos paired with the other for his 7160^{u} cycle: fn 52 & fn 59) was 14° short of apogee. Ptolemy correctly notes (*Almajest* 6.9) the consequent serious effect upon the equation of center.

Moon's draconitic motion (eq. 19).[50] Note: Ptolemy's *idem* criticism of this method is valid — his own *Almajest* 4.9 method is superior[51] to Hipparchos' use of a 7160^{u} cycle.[52] (Perhaps Hipparchos was using the cited -719 & -140 pair for confirmation, not discovery.) But Ptolemy's result is slightly worse than Hipparchos' (or whoever's: §A6) eq. 19.

D4 So, both the considerations cited (§D2 & §D3) recommend the strong possibility that (for finding the lunar anomalistic motion, as discussed at *Almajest* 4.2) Hipparchos would have used the pair of perigee eclipses highlighted above (-830 & -140).

D5 And, knowing that 9146 anomalistic returns had occurred during 8534 synodic months (twice the eq. 1 numbers cited at *Almajest* 4.2), he could (with twice the empirical confidence yielded by single-cycle data) thereby have obtained his anomalistic month by the following arithmetic:

$$V_{\mathrm{H}} = 8534M_{\mathrm{A}}/9146 = 251M_{\mathrm{A}}/269 = 192123683^{\mathrm{d}}/6972480 = 27^{\mathrm{d}}.554569 \qquad (8)$$

which was correct to about 1 timesec! To be precise: the mean error (of eq. 8) during the centuries[53] discussed in this paper = $-1^{\mathrm{s}}.3\pm0^{\mathrm{s}}.1$. (Understand: empirically determining anomalistic motion is an ordmag more difficult than determining synodic motion.) The eq. 8 anomalistic monthlength V_{H} is the basis of the (evidently Hipparchan)[54] daily motion given at *Almajest* 4.3&4 (based on eqs. 2&3):

$$v_{\mathrm{H}} = 360^\circ/V_{\mathrm{H}} = (360^\circ/M_{\mathrm{A}})269/251 = 13^\circ03'53''56'''29''''38'''''38'''''' \qquad (9)$$

And Ptolemy's *Almajest* 4.3&7 value (the basis of his *Almajest* 4.4 anomalistic motion tables) differs from eq. 9 by merely[55] $-12''''$.

[50] Hipparchos' & Ptolemy's approaches are better than that Aristarchos may've used. Yet, see fn 79.

[51] Though in the guise of an 8 1/2-century-span (note §I15 item [d]) trio-trio proof, Ptolemy's *Almajest* 4.6 development attains laughably overneat agreement with Hipparchos' anomalistic motion (fn 55). (As also for the planets, Ptolemy's *amateurish inexperience* with the empirical unreliability, of the anomalistic mechanisms of his orbit-models, led him to prefer a nonintegral-return "proof" of the mean motion, instead of the integral-return observations on which the mean motion tables were actually founded — by scholars who knew from frustrating experience that untamed non-ignorable perturbations would degrade any other type of empirical foundation. See fn 18 & fn 35.) I note that between two of the *Almajest* 4.6 eclipses (-719/3/8-9 & 136/3/5-6), there were 45 Metonic cycles = 10575^{u} = $10476^{\mathrm{w}} - 19^\circ$ = 312286d01h. In a Metonic series of syzygies, eclipses recur every 24 Metonic cycles and thereabouts, so the pair cited here is part of the 2nd return of this effect (i.e., the cluster of eclipse-pairs centering about the 48 Metonic-cycle interval).

[52] *Almajest* 6.9 (again using the -719/3/8-9 eclipse) takes advantage of the near-commensurability: $716^{\mathrm{u}} = 777^{\mathrm{w}}$, where 10 such cycles give an approximate (fn 49) demi-return in anomaly. (True, thirteen 716^{u}-cycles would have been much better: $9975^{\mathrm{v}}1/2$. But, to use this, Hipparchos would've needed data from c.900 BC.) [See below at §F9.]

[53] For the real ancient lunar and solar periods and motions in this paper, I use modern-calculated values for Phil 1 (-323/11/12 Alexandria apparent noon). [For lunar acceleration, see Dickey *et al*, *Science 265*:482 (1994/7/22).] This includes (where apt, e.g., fn 12) the effect of Earth-spin-acceleration on the length of the solar day (*DIO 1.1* ‡5 fn 11). Our comparisons (of modern calculations) to ancient reports will involve some circularity (Rawlins 1991H fn 5) for judging values depending on the Moon's mean synodic period in solar days (see fn 12 for real value, eq. 2 for ancient standard value); so I estimate its accuracy (§A6) by assuming that our knowledge of ΔT for that era is good to roughly a quarter-hour.

[54] We are not sure whether Hipparchos found his lunar tables' anomalistic month V_{H} (eq. 8) — & thus anomalistic motion v_{H} (eq. 9) — from the M_{A}-multiple approach (i.e., using eqs. 2&3 to find eq. 9) or by the straight division of eq. 1's day-interval (126007d01h) by 4573. Ptolemy's *Almajest* 4.2 discussion indicates the former, and we adopt it throughout as the primary procedure here. (Note that the same approach — use of eq. 2 — provides our near-perfect match, below, between eq. 22 and eq. 23.) For nonintegral anomalistic returns, the difference can be nontrivial for a single eclipse-pair; however, working with the average of a few empirical day-intervals will yield results almost identical to the M_{A}-multiple method. See fn 110. (Also: fn 56 & fn 79.)

[55] More precisely: $-11''''46'''''39''''''$ (explained at *Almajest* 4.7). Note the discerning comments of R.Newton 1977 p.179 on the transparency of Ptolemy's pretense to empiricism & originality at *Almajest* 4.7. (But see here at fn 61.) RN's charge is bolstered by our realization that the Ptolemy v_{J}'s actual error (ordmag 1 part in a million) is ordmag 10 times larger than the above-cited tiny ($-12''''$), supposedly-empirical difference between Ptolemy's v_{J} (*Almajest* 4.3-4&7) and Hipparchos' v_{H} (eq. 9)! (Ptolemy's shift from v_{H} to $v_{\mathrm{J}} = 13^\circ03'53''56'''17''''51'''''59''''''$ made a very slight improvement in accuracy, though his analogous *Almajest* 4.2&9 correction, of Hipparchos' draconitic motion, degraded its accuracy by a similarly trivial amount. However, it is hard to fault Ptolemy in the latter case, since eq. 19 was already so near-perfect [§F8 & fn 78] that any attempt to improve it was almost certain to effect an opposite outcome. Which may be why Ptolemy eventually reverted, at *PlanHyp* 1.1.6, to the unaltered original: our eq. 19, Heiberg 1907 pp.78-79.) Moreover, the difference between v_{J} and Ptolemy's other (later) value for the anomalistic motion (v_{Y} in eq. 13) is about twice as big as v_{P}'s error — and 16 times larger than Ptolemy's superfluous $-12''''$ shift.

D6 Hipparchos' apparently (§D3) deliberate use of the -830 perigee eclipse suggests an intriguing question: did he (or the Babylonians: §H3) know just how close to the apse this eclipse really was? (It was only about a degree from perigee: fn 65.) If so, then the ancients probably had access to a compact eclipse trio[56] of about this time: -832-830 — more than a century before the famous *Almajest* 4.6 trio (-720-719) which we have previously believed to be the earliest such Babylonian data used by the Greeks. The earlier trio is from the reign of the Assyrian ruler, Shalmaneser the Third (859-824 BC); the latter is from the reign of Sharrukin the Second (722-705 BC) — otherwise known as Sargon (e.g., Isaiah 20.1).

E Independent Evidence for Ancient Use of the −830/2/4 Eclipse

E1 At Ptolemy's *PlanHyp* 1.1.6 (Heiberg 1907 pp.78-79 or Neugebauer 1975 p.902 eq.5), there is a hitherto-unexplained equation:

$$3277^{\mathrm{u}} = 3512^{\mathrm{v}} \qquad (10)$$

Tripling this relation [see *DIO 11.1* ‡2 fn 21 on quintupling it] finds an eclipse cycle:

$$9831^{\mathrm{u}} = 10536^{\mathrm{v}} = 10668^{\mathrm{w}}1/2 + 22^\circ = 795^{\mathrm{g}} - 65^\circ = 290315^{\mathrm{d}}07^{\mathrm{h}} \qquad (11)$$

(We again recall that superscript g = anomalistic solar years: §A1.) Remarkably, this relation (and thus eq. 10) was about as accurate (§E4) as the much more famous 251^{u} relation (eq. 3 or eq. 1), whose error is estimated at §D5.

E2 In passing hindsight, we may note that there were several long[57] synodic-anomalistic period-relations which were more accurate than either eq. 3 or eq. 10. One of the best[58] would have been:

$$7042^{\mathrm{u}} = 7547^{\mathrm{v}} = 7642^{\mathrm{w}} - 18^\circ = 569^{\mathrm{g}}1/3 = 207954^{\mathrm{d}}11^{\mathrm{h}} \qquad (12)$$

[56] Consultation of Oppolzer 1887 (p.330) indicates that a quad (foursome) of eclipses was available, of which any 3 could have served as a trio for ancient geometric purposes: -832/3/26-27, -832/9/20-21, -831/9/9-10 (see fn 103 & §H4), & -830/2/4-5. The mean amplitude of the eq. 1 time interval's variation was $\pm$0h.5; this amplitude-smallness is, of course, mostly due to eq. 1's slight sidereal-year $-7^\circ1/2$ remainder. (Multiply twice sin $3^\circ3/4$ times the 2° solar eq.ctr amplitude, and divide by the $1^\circ/2$ hourly synodic motion, to find 0h.5. Checking other cycles [fn 19] based upon eq. 3 would verify that this amplitude was consistent with solar-anomaly-causation; see fn 39.) Which would suggest (to an analyst who wasn't correcting for solar anomaly) that averaging an around-the-zodiac set of 345 yr pairs' intervals ought to have produced a more accurate mean interval than would a single 690 yr pair. The temporal stability of the interval — whether 345 yrs or 690 yrs — is its primary recommendation: fn 18.

[57] Other useful synodic-anomalistic cycles:
$1520^{\mathrm{u}} = 1629^{\mathrm{v}} = 1649^{\mathrm{w}}1/2 - 1^\circ = 123^{\mathrm{g}} - 40^\circ = 44886^{\mathrm{d}}1/2$;
$5787^{\mathrm{u}} = 6202^{\mathrm{v}} = 6280^{\mathrm{w}} + 11^\circ = 468^{\mathrm{g}} - 47^\circ = 170893^{\mathrm{d}}13^{\mathrm{h}}$;
$8046^{\mathrm{u}} = 8623^{\mathrm{v}} = 8731^{\mathrm{w}}1/2 - 5^\circ = 650^{\mathrm{g}}1/2 + 2^\circ = 237603^{\mathrm{d}}04^{\mathrm{h}}$.
Unstated lunar-anomaly remainders: $+1^\circ$, $+0^\circ$, & -0°, resp. (For eqs. 1, 10, & 20: -1°, $+1^\circ$, & -145°, resp.)

[58] The solar anomaly remainder of eq. 12 was about double eq. 11's. But, given the size of both remainders, neither relation would yield highly accurate day-length intervals without (fn 79) averaging or solar-anomaly corrections.

Had the eclipse of −149/1/7 been visible to Hipparchos, he could have paired it with that of −719/9/1-2 (*Almajest* 4.6), except[59] that the later one occurred below his horizon.[60]

E3 Returning to the attested 3277^{u} relation: if (analogously to §D5) we combine eq. 10 or eq. 11 with eq. 2, then we have

$$V_{\rm Y} = 9831M_{\rm A}/10536 = 3277M_{\rm A}/3512 = 27^{\rm d}.554600508 \qquad (13)$$

E4 The error in eq. 13 was $+1^{\rm s}.4\pm0^{\rm s}.1$ — error about same size as that of eq. 8, but of opposite sign. (Eqs. 8&13 are both accurate to about 1 part in 2 million — impressive, though not quite up to the accuracy of eq. 2: see Rawlins 1991H fn 1.) So the mean of eq. 3 & eq. 10 was just about right, and it is a credit[61] to Ptolemy's judgement[62] that he recommended both values (and no others). Another way of putting it: the average of Ptolemy's two estimates (eq. 8 & eq. 13) of the lunar anomalistic month was almost exactly accurate: error ordmag $1^{\rm s}/10$.

E5 Again (as with the 690 yr cycle noted at §D1), we find that this 795 yr cycle's number of draconitic returns exceeds (eq. 11) a half-integral value by an amount (22°+) which is just short of the outer limit (25°−) for pairs of perigee-eclipses. Therefore, again, very few observable eclipse-pairs will satisfy eq. 11 — and the majority of these will be in the general vicinity of perigee.

E6 Further, said pairs occur not randomly but rather in bunches. (See §F11.) Astonishingly, the last pair that happened before Ptolemy (who imparted eq. 10) started with the −830 eclipse — an event which occurred a thousand years before! That pair[63] was: −830/2/4 & −36/12/7. The latter eclipse (−36) is just one $345^{\rm g}$ cycle after the −381/12/12 eclipse (which is attested at *Almajest* 4.11),[64] and the former (−830) is the very eclipse we already suggested (at §D2) Hipparchos might have used for the 690 yr cycle.

E7 Note: the actual interval between the 2 eclipses of §E6 was $290315^{\rm d}02^{\rm h}$ ($5^{\rm h}$ shorter than $9831^{\rm u}$ in eq. 11). Division by $10536^{\rm v}$ (eq. 11) produces $V = 27^{\rm d}.554583$, ordmag $0^{\rm s}.1$ different from reality (fn 12), — i.e., much more accurate than either eq. 8 or eq. 13. Therefore, eq. 10 could be a remnant of the ancients' very best value of the anomalistic month V.

E8 Also, both the −830/2/4 & −36/12/7 eclipses were very near perigee. Likewise for the −140/1/27 eclipse proposed at §D2 as a possible 690 yr-cycle-match with the −830/2/4 eclipse. That is, all 3 of the eclipses we've concentrated on here (§§D&E) were perigee-events:[65] −830, −140, −36.

F The Precessing ss-Bound anomalistic-Triangle

F1 The average saros series (abbrev "ss") lasts about 8 centuries (see fn 69, fn 73, & §F3 for details), as its successive eclipses slowly grow, crest, & then fade in magnitude. The famous $18^{\rm y}11^{\rm d}$ period between ss eclipses is governed by the relation:[66]

$$223^{\rm u} = 239^{\rm v} - 3^\circ = 242^{\rm w} - 0^\circ 28' = 6585^{\rm d}1/3 = 18^{\rm K}10^\circ 2/3 = 18^{\rm g}10^{\rm d}2/3 \qquad (14)$$

for which we remember (§A1) that superscript K = Kallippic years of length $Y_{\rm K}$, where:

$$Y_{\rm K} = 365^{\rm d}1/4 \qquad (15)$$

in contrast to the Hipparchos-Ptolemy "tropical"[67] yearlength adopted at *Almajest* 3.1-2&9.4:

$$Y_{\rm J} = 365^{\rm d}1/4 - 1/300 \qquad (16)$$

F2 For comparison: the actual tropical yearlength at the epoch of Phil 1 was about $365^{\rm d}1/4 - 1/133$, thus eq. 16's $Y_{\rm J}$ was too high[68] by $6^{\rm m}$ — i.e., $10^{\rm h}$/century!

F3 The mean ss-length of 8 centuries is governed[69] by eq. 14's $-0^\circ 28'$ draconitic remainder,[70] which requires roughly a few dozen saros to cover the 22°− range surrounding a lunar node, in which mean-condition eclipses can occur. Though ss-lengths can be less than 7 centuries and more than 10 centuries, the average ss lasts[71] between 8&9 centuries[72]

[59] Several thoughts are suggested by the lack of attestation of the 569 yr cycle: [a] Since 569 yr-cycle eclipse-pairs are not rare, then the Greeks' access to 8th century BC Babylonian eclipse material was much less full than is suggested by Ptolemy (fn 84). (Rawlins 1985S has implied that the data available to Greek astronomers from this time indeed may have been fragmentary; however, see §C1's alternate explanation for ancients' evident non-use of eq. 12 & such.) [b] Since fuller data are cited by Greeks from the 6th century onward, identifications of 569 yr-cycle pairs should have been made. [c] Possibly the Greeks did use either this cycle or a similar one (e.g., §D3's $7160^{\rm u} = 7770^{\rm w} \approx 7673^{\rm v}1/2 \approx 579^{\rm y}$, at *Almajest* 6.9) to find the empirical basis for eq. 19, so that (contra the suggestion of Rawlins 1985S) eq. 19 was found not from eclipses separated by $5458^{\rm u}$ (or eq. 18's $2729^{\rm u}$ or its triple [662 yrs], which has a better lunar anomalistic return but a remainder of 40°, nonetheless) but by continued-fractions analysis. (Eq. 19 is mentioned at *Almajest* 4.2, but not in connection with an empirical eclipse-pair $5458^{\rm u}$ apart.) See fn 79. Note that if eq. 19 was derived by continued-fractions (and its prominence by Hipparchos' era is likely related to mathematically-refined investigations), then we will probably not be able to trace its ultimate empirical foundation (see Neugebauer 1975 p.106-107, partially cited here at the outset: fn 4) — especially if it is not built upon a specific period-relation, as eqs. 11 & 31 each were.

[60] Aristyllos may have had the opportunity of discovering the 569 yr cycle from the eclipse pair: −831/9/9-10 & −261/1/15-16 (interval 207954d18h).

[61] See also Ptolemy's draconitic reversion: fn 55.

[62] For our similar but far greater debt to Ptolemy, see Rawlins 1991W fn 94.

[63] This pair ended a series of such 795 yr-pairs (connecting two ss), a series which had started with the pair −1047/9/27 & −252/7/30. (Neither of these two eclipses was visible in Europe or Babylon. Of this series, the first pair visible in Babylon was −1029/10/8 & −234/8/10-11.) Note, however, that this series of 795 yr-cycle pairs was not the only one that included eclipses in Hipparchos' time. Pairs which ended other such series: −935/3/26 & −140/1/27 (fn 86), and −924/2/24 & −130/12/27. (But neither could have been used by Hipparchos, since each contained at least one invisible eclipse.) The latter instance is notable for being a one-eclipse-pair series! — which imparts an idea of just how delicate the 795 yr cycle is. (Its respective mean anomalies v = −113° & −112°, and resp magnitudes m = 0.4 & 0.6; so this is virtually the outer edge of possibility for 795 yr-cycle eclipse pairs, remarkably far from perigee.) [All $795^{\rm y}$ pairs are from saros-series whose Meeus-Mucke numbers differ by 53.]

[64] If, despite its large solar-anomaly remainder (−65°), eq. 11 (795 yr base) was found via the −36/12/7 eclipse, then the discoverer preferred it to eq. 1 (345 or 690 yr base) simply because its interval was more than twice as long. The −36/12/7 mid-eclipse was at 22:51 Alex Mean Time (22:56 Alex App Time), at λ = 74°.7 & β = −0°.9 (topocentric); its magnitude m = 6.9 (N.limb).

[65] The three cited eclipses' lunar mean anomalies were: +1° (−830/2/4), −1° (−140/1/27), & +1° (−36/12/7).

[66] See *Almajest* 4.2 or Geminos 18.6. The 10°2/3 remainder is rendered as $10^{\rm d}2/3$ at Neugebauer 1975 p.502, from dividing the Babylonian yearlength/monthlength ratio 12;22,08 into 223. (The discrepancy once temporarily misled a highly eminent Hist.sci referee.) Speculations on the original Babylonian figure, 12;22,08 (eq. 4, regarded by the Muffia as "perhaps the most fundamental parameter in Babylonian astronomy", Aaboe 1955 p.123; see Dicks 1994 fn 29 & its concluding emphasis): [a] Was it caused simply by an ancient's division of the mistaken version into 223? (The remainder is misrendered as 10d1/3 on p.vi of Menzel & Gingerich 1962, where we also find an example of the most elementary of ancient astronomy errors: −412 confused with 412 BC.) Or [b] Did an eclipse-investigator find it by dividing the anomalistic year by the synodic month? — a ratio which, if then divided into 223, would have given $18^{\rm g}10{\rm d}39'$ or (eq. 14) $18^{\rm g}10{\rm d}2/3$. Regardless, note that the Greek version (using 10°2/3: eq. 14) is peculiarly hybrid: Kallippic & Aristarchan. It was based upon dividing the Kallippic yearlength (eq. 15) into $223M_{\rm A}$, where $M_{\rm A}$ (eq. 2) is non-Kallippic (*DIO 1.1* ‡7 fn 1). See Heath 1913 pp.314-315, Neugebauer 1975 p.603, & Rawlins 1985S. Aristarchos' saros-based Great Year was 2434 yrs (Rawlins 1985S suggests effectively 4868 yrs), using a very-nearly Kallippic year — 1st reconstructed by the adventurous genius of P.Tannery.

[67] See fn 13.

[68] Causes examined in Rawlins 1985H.

[69] I.e., multiplying the famous saros-period (eq. 14: $18^{\rm K}10^\circ 2/3$) times (22°−)/(0°28′/saros), we have a figure close to fn 72's 8 1/3 centuries.

[70] The $-0^\circ 28'$ draconitic remainder was the real value. (In all the cycle-equations of this paper, the remainders displayed are the actual [*DIO*-computed] ones, for the epoch Phil 1 = −323/11/12. See fn 53.) This was also the value implicit in the ancient astronomers' eqs. 2&19. Eq. 14's real −3° anomalistic remainder is just an average. However, over the centuries (during ss-life), the actual increment from eclipse to eclipse varies by less than ±1° (perigee vs. apogee).

[71] Throughout this paper, we completely ignore penumbral eclipses, consistently taking an "eclipse" to mean an umbral eclipse, in which a relatively sharp visible Earth-shadow sweeps across the Moon's disk. (If penumbral eclipses were included, then ss length would be between 12 1/2 and 15 1/2 centuries. Note the oddity that: all the longest umbral ss are part of short penumbral ss, while all the longest penumbral ss contain only short umbral ss.)

[72] Taking the weighted average of the fn 73 ss-lengths (minus 1), and multiplying by eq. 14, yields 837 yrs for the average (umbral) ss-length for the data of Liu & Fiala 1992 Table 3.1.

— but in fact very few (about 1/12th) are in this "mean" range, most ss being nearer the extremes: about 70% are either between 7&8 centuries or between 9&10 centuries.[73] The median ss lasts 44 eclipses (also the mode) or 775 years. (See data of fn 73.)

F4 The reason that ss-bounds and their anomalies are critical here is that the 795 yr cycle can only barely occur at all; thus, it must involve grazing (low magnitude) partial eclipses — and grazing eclipses usually (though, see §H2) only occur near ss start or end. And, if the grazing eclipse is near apogee, then the lunar-disk+Earth-shadow sum is too small for intersection, so the cycle could not succeed regardless of how symmetric are the two eclipses' angular distances from the node (preferably about 11° each). Since the 22°+ remainder in eq. 11 is a little over double the 11°− mean-condition limit (for how far from a node an eclipse can happen), then 795 yr-cycle eclipse-pairs can occur for most anomalies, but they are far more likely near perigee (where the limit is not 11° but more than 12°).

F5 The happy circumstance, that several centuries passed (between 37 BC and the death of Ptolemy)[74] without a 795 yr pair occurring, is the fortunate accident which enables us to prove from eqs. 10-11 that the Greeks were using eclipse data from no later than −830/2/4 — i.e., *more than a century* earlier than had been established by now-surviving explicitly dated records (the earliest of these being the −720/3/19-20 eclipse reported at *Almajest* 4.6).

F6 Two neat eclipse period-relations (eqs. 17&18) establish what I will call the "PBT": the Precessing ss-Bound anomalistic-Triangle, governing ss-starts&ends; and this triangle's slow-motion precession in turn explains[75] the long gap (in the occurrence of 795 yr-cycle eclipse-pair-ends) following the −36 eclipse.

F7 Two relations underlying the PBT are: the wellknown[76] 29 yr cycle,

$$358^{\mathrm{u}} = 383^{\mathrm{v}}2/3 + 2^\circ = 388^{\mathrm{w}}1/2 = 29^{\mathrm{g}} - 20^\circ = 10572^{\mathrm{d}} - 1^{\mathrm{h}} \qquad (17)$$

and the 221 yr cycle

$$2729^{\mathrm{u}} = 2924^{\mathrm{v}}2/3 + 13^\circ = 2961^{\mathrm{w}}1/2 = 221^{\mathrm{g}} - 131^\circ = 80589^{\mathrm{d}}. \qquad (18)$$

F8 This is a good place to point out in passing the critical historical fact that twice eq. 18 is explicitly attested at *Almajest* 4.2 and at *PlanHyp* 1.1.6 (Heiberg 1907 pp.78-79):

$$5458^{\mathrm{u}} = 5923^{\mathrm{w}} \qquad (19)$$

as a near-perfect synodic-draconitic return. (See also Neugebauer 1975 p.310.) And how well did the ancients do, when choosing eq. 19 (= 161178^{d} = $441^{\mathrm{g}} + 97^\circ$) as the basis for their draconitic tables? With components this large, the best choice should be accurate to better than 1 part in 10 million. And the accuracy of eq. 19 was indeed about that fine.

[73] See Liu & Fiala 1992's Table 3.1 (at their pp.24-25). For the 106 saros-series contained fully in that work, the ss-length frequency distribution is given (in Liu & Fiala 1992's Table 3.2, p.26), though only for the ss-length defined by penumbral eclipses, curiously. So, we here supply the table of interest to us in this paper (i.e., ss-length defined by umbral eclipses). For each entry, the ss' number of umbral eclipses is followed by (in parentheses) the number of ss of this length occurring entirely within Liu & Fiala 1992: 39 (2), 40 (10), 41 (9), 42 (9), 43 (12), 44 (14), 45 (3), 46 (4), 47 (1), 48 (3), 49 (0), 50 (0), 51 (0), 52 (2), 53 (5), 54 (6), 55 (5), 56 (9), 57 (5), 58 (7). (Note that, e.g., a 39-eclipse-ss is 38 saros periods long.)

[74] The 1st sure post-Ptolemy 795 yr-cycle eclipse pair was −540/1/9-10 & +254/11/11-12, visible in Babylon & Europe, resp. Pogo 1938 (recommended without checking by Menzel & Gingerich 1962 p.vi) contradicts Oppolzer 1887 in claiming that the syzygies of 236/10/31-11/1 & 247/10/1-2 were eclipses. But Liu & Fiala 1992 & Meeus & Mucke 1992 agree with Oppolzer that no eclipses occurred. (My calculations find: magnitudes $m = +0.0$ & -0.1, resp.) Between 37 BC & Ptolemy's death, the nearest thing to a break in the 795 yr-pair-drought was the pair starting with the syzygy of −812/2/15-16. (An eclipse then could have paired with the eclipse of −18/12/18-19, the start of which was visible in Rome & probably Alexandria.) However, all sources agree that there was no −812/2/15-16 eclipse: Oppolzer 1887, Liu & Fiala 1992, & Meeus & Mucke 1992. (Even Pogo 1938.) I calculate $m = -0.2$.

[75] See §F4 & §F11.

[76] Partial history at van den Bergh 1955 pp.22-23.

(Eq. 19 is less accurate today, merely 2 parts in 10 million.)[77] Which testifies to the level of science in ancient times — and to the power of well-chosen period-relations for revealing astronomical mean motions. (There may also be a bit of luck[78] involved — which didn't work out quite so well for the synodic-anomalistic period-relation: see fn 55 & §L5.)

F9 Who was responsible for the famous eq. 19? There are several possibilities. At *Almajest* 4.2, it is attributed to Hipparchos. Muffia convention (§A3) prefers Babylon. Rawlins 1985S suggests[79] that it (& eq. 2)[80] may be from the time of Aristarchos.
[The origin of eq.19 was finally solved in 2002 at *DIO 11.1* ‡3 eq.3: Hipparchos used the technique described at *Almajest* 6.9 but paired his own −140 eclipse with a much older one (−1244) than that cited by Ptolemy (−719).]

F10 Returning to examine PBT behavior: the little 2° term, by which the anomalistic part of eq. 17 differed from precisely 1/3 of an integer, ensured that (on average)[81] the upper or lower temporal bound of each ss would, in the short run (a few decades), occur at one of 3 evenly spaced points — a nearly equilateral tri-angle that was pretty stable in time (i.e., mean drift = merely c.2°/29 yrs). And eq. 18's similar anomalistic 13° term ensured that, in the long term, each triangle faded into another which was[82] (on average) 13°/221 yrs ahead of the previous one.

[77] van den Bergh 1955 p.24 mentions eq. 19 but (due to basing his p.18 continued-fractions analysis upon modern motions) prefers cycles at pp.18-19 which were not (in antiquity) as accurate as eq. 19. The clever reasoning at van den Bergh 1955 p.24 finds a 1769 yr cycle, but secular variations over such a period will slightly degrade its usefulness. (The same criticism will apply to very long cycles touched upon in the present paper.)

[78] The times of Babylonian eclipses were of poor accuracy (Dicks 1994 §D1 & fn 46), probably due to nonuse of vertical instruments. (Accurate times of ancient eclipses required sundials and fractional hours.) This may've slightly undercut Greek research into synodic-anomalistic cycles. But Babylonian eclipse magnitudes (requiring no instruments at all) were better, and this fact may have helped ensure that the prime Greek & Babylonian synodic-draconitic cycle (eq. 19) was so wonderfully correct.

[79] It should be pointed out that the eclipse-pair (−719/3/8-9 to −278/6/19-20, the latter event half-visible) proposed by Rawlins 1985S is not ideal as a basis for eq. 19: [a] The eclipse-magnitudes are different. [b] The number of anomalistic revolutions is not integral — thus, the actual interval is about 4 hrs smaller than eq. 19 predicted, and the distances from Earth are different (which affects comparative magnitudes). Such effects can be compensated-for (as Ptolemy mercifully notes at *Almajest* 6.9), but otherwise, Ptolemy would justly criticize such an effort (as at *ibid*). (And he might do so, regardless. Ironically, Ptolemy's own much-more-sophisticated *Almajest* 4.6 alleged determination of the anomalistic lunar motion can be faulted for using an interval not anomalistically-integral: this approach makes the result sensitive to numerous needless uncertainties. Unless one is forcing the result. See fn 51 & fn 55.) If Aristarchos used intelligently the −719&−278 pair (as Rawlins 1985S suggests), then he was: [i] doing such primary research that refinements were yet to come, [ii] compensating (requiring an anomalistic theory already: see fn 56 & Rawlins 1991W §N17), or [iii] averaging a whole collection of similar 441 yr-cycle pairs. (See fn 110 for a hypothetical example of such averaging, applied to 781 yr-cycle data.) Such pairs are common. Indeed, the −720/3/19-20 & −279/6/29-30 pair was also available — though the interval was almost a full day less than eq. 19's 5458^{u} = 161178 days. This is the price one pays for non-integral anomalies: here, the lunar eq.ctr-difference minus the solar eq.ctr-diff equals the negative mean lunar elongation diff = 9°3/4, an amount which lunar synodic motion will require most of a day to compensate for. By contrast: for the −719&−278 pair, the lunar eq.centers are nearly equal (within a half-degree), and the mean elongation discrepancy — predominantly due to solar-anomaly-inequality — was barely 2°, or 4 hrs of lunar synodic motion. Thus, the pair is merely c.4 hrs short of eq. 19's 161178 days. (By my calculations, the Babylon mean times were 00:10 & 19:44, resp. These figures are, of course, subject to modest non-independent uncertainties: see fn 48.) Note also that, over a 441 yr interval, the effect of a 4 1/2 hr discrepancy is ordmag 1 part in a million (though, in fact, eq. 19 is alot more accurate than this: §F8), thus, a hypothetical draconitic yearlength based upon the −719 & −278 pair (proposed in Rawlins 1985S) would probably be more accurate than any value known to exist previous to Aristarchos, even without any compensations: simply dividing 5923 into 161177d20h yields the equivalent of about 27d.2122 (which was low by about 2 timesec). At fn 110 (781 yr cycle), we see that ancients (including Ptolemy, 4 centuries later) would indeed use long-interval eclipse-pairs, despite nonintegral anomalistic returns. (Nothing new about this. Indeed, the most famous & widely-used solar-synodic period-commensurability relation of them all, eq. 30, has a huge anomalistic remainder. Using the real [not Metonic] tropical year, we have: $235^{\mathrm{u}} = 19^{\mathrm{y}} + 02\mathrm{h} = 19^{\mathrm{g}} - 06\mathrm{h} = 252^{\mathrm{v}} - 53^\circ = 255^{\mathrm{w}} + 8^\circ = 6939\mathrm{d}17\mathrm{h}$.) Note that, for both situations, further data may have been available.

[80] Fn 43.

[81] Note that eqs. 17&18 both involve *mean* motions.

[82] The modern eq. 18 remainder is more than 14°. But the past-tense references, in our PBT discussions here, apply with virtually equal force today.

F11 This means that, even over several centuries, each ss' bound (whether we track the upper or the lower bound) will stay near one of the three PBT "points" (separated by c.120°)[83] — and these "points" will diffuse only ordmag 10° during that time. This leaves alot of anomalistic space (in the 360° of possible values of anomaly) in which no ss-bound eclipse will occur for centuries on end. Since ss-bound anomaly is critical (§F4) to the probability of a 795 yr-cycle eclipse-pair occurring, the PBT's stability explains how such pairs can virtually or entirely disappear for many years (even centuries) in a row, if none of the three PBT points is near enough to perigee — which happens to have been the case between −36 and the end of Ptolemy's career.

G Identities

G1 Ptolemy alleges (*Almajest* 3.7) that ancient astronomical records were generally rather complete from Nabonassar 1 (−746/2/26) onwards.[84] Thus, the current findings extend (§D6) the period of useful Babylonian records backwards by roughly a century.
G2 But we are left with the question: who discovered eq. 10? — based on the 795 yr eclipse cycle. It could have been Hipparchos. There are two 795 yr pairs of which he might have seen[85] the latter member (both were part of the same ss, ending at −36/12/7), namely, −957/11/20-21 & −162/9/22-23 and −921/12/12-13 & −126/10/14-15. On the other hand: [a] No attested Hipparchos eclipse observation is part of a visible[86] 795 yr pair. [b] The earlier end of any hypothetical Hipparchan pair must be more than 200 yrs previous to the −720 eclipse observation attested by Ptolemy — but resorting to postulating 10^{th} century BC material is unnecessary, since later material (§E6) can explain eq. 10 just as well. (Nonetheless, see the speculation of §§H6-H7.)
G3 So I prefer the least sensational of our options here, one which also ties Hipparchos' 345 yr cycle (double: §D1 & eq. 8) and the 795 yr cycles together — with the −830/2/4 eclipse representing the knot.
G4 Accepting this, we ask: who could have used the −36/12/7 eclipse? (Certainly not Hipparchos, who was long dead by then.) We are now peering into the period between Hipparchos (2^{nd} century BC) & Ptolemy (2^{nd} century AD), a time whose high science has hitherto been a virtual blank[87] in history: now as poorly-attested as it is critical to understanding the flowering of the grandest achievements of ancient science, many of which are reflected in the *Almajest*.
G5 Rawlins 1985K traces the *Almajest* 9.3-4 Venus & Mars tables to the reign of Kleopatra (52-30 BC);[88] so the suggestion (§§E6 & G2-G4), that eq. 10 was discovered in 37 BC, is consistent with the supposition that high astronomy was being maintained & improved at this time by a figure or school(s) whose name can for now only be guessed at. Possibilities include (Neugebauer 1975 p.575): [a] Serapion, who is the earliest figure cited (fn 32) as a compiler of tables for equation of time (which indicates the existence of sph

[83] This c.120° has no relation to the wellknown c.120° (due to the $1^{d}/3$ remainder in eq. 14) by which the local solar times of successive ss eclipses differ — and which is the basis of the convenient 54 yr exeligmos (triple eq. 14): *Almajest* 4.2, Geminos 18.6, van der Waerden 1974 p.103.

[84] Ptolemy seems to be implying that spottier records existed before that time. And his *Almajest* 4.6 statement that the −720-719 trio was "selected" from the Babylonian records of that era also implies more. (See fn 59.)

[85] The −126/10/14-15 eclipse was only 3 months after the last Hipparchos observation we have (−126/7/7 lunar observation: *Almajest* 5.5). But the notability of the −957/11/20-21 eclipse is questionable: at the very start of this grazing partial eclipse (m = 1.7), the setting Moon's altitude (above Babylon's dawnlit horizon) was tiny at best.

[86] Hipparchos' −140/1/27 eclipse is part of a 795 yr pair: it matches the −935/3/26 eclipse. But the −935 event was not visible at Babylon (or Europe).

[87] Neugebauer 1957 p.55: "Early Greek astronomy from its beginnings about 400 B.C. [200 B.C. in 1st ed!] to Ptolemy (about 150 A.D.) is almost completely destroyed, except for a few elementary works which survived for teaching purposes. But the rest was obliterated by Ptolemy's outstanding work, which relegated his predecessors to merely historically interesting figures." For my disagreements with the 2nd sentence, see, e.g., Rawlins 1984A.

[88] See Toomer 1984 p.11's excellent edition of the invaluable Ptolemy king-list.

trig before Menelaos), [b] Sosigenes, who induced Caesar to adopt the $365^{d}1/4$ calendar. [c] Antiochos. [d] Also, I am not sure that Poseidonios' death has been certainly established as occurring before −36. [e] Finally, the most probable discoverer is the same party who actually invented most of great ancient science and math, namely: Anonymous.

H The Saros-Series That Wouldn't Die: a Thread to Hammurabi?

H1 Anonymous is certainly now the name of the 9^{th} century BC Babylonians who took such care to make the precious early −830/2/4 record. Why was it preserved? Striking fact: the grazing −830/2/4 lunar eclipse *ended an extremely long ss*. At magnitude[89] m of merely 0.5 digits (about 1 arcmin of visible umbra), it might hardly have been noticed if not anticipated and looked for at Babylon. Simple (speculative) explanation: the Babylonians had already isolated the saros phenomenon by the 9^{th} century BC. Since such awareness doesn't develop overnight, the hint[90] is there that: ss were being studied in Babylon in the 2^{nd} millennium BC.
H2 Going to the latter end of the particular ss under examination, we find that it *lingered* to an unusual degree. It contained 57 eclipses, spread out over nearly 1010 years[91] — almost the maximum possible duration. (See §F3 & fn 73.) Though the typical ss (e.g., that ending at −1036/3/2-3) dies out with magnitudes fading at about a digit per saros (18+ yrs), those eclipses comprising the peculiarly persistent ss under discussion had a magnitude less than 2 digits for almost *two centuries* before finally dying on −830/2/4. Taking only eclipses visible at Babylon, we find that, as early as −1029/10/8-9, the magnitude was m = 1.9 digits, and it had only fallen to m = 1.7 by −1011/10/18-19. Thereafter, instead of declining, the magnitude stabilized at about 1 1/2 digits and then even *climbed back* a bit: resurrecting from m = 1.6 on −975/11/9-10, up to m = 1.7 on −957/11/20-21 (fn 85) & −921/12/12-13, then down to m = 1.6 on −884/1/3-4. By −866/1/13-14, the magnitude had fallen to m = 1.3, and the next eclipse of this ss visible[92] in Babylon was the last (−830/2/4-5, m = 0.5).
H3 In connection with careful ancient preservation of the −830/2/4 eclipse-record: one might also tenuously speculate that Babylonians of this early epoch additionally were aware (whether from direct speed-estimation[93] or from theory — or perhaps just from the §H2 linger-factor)[94] that the −830/2/4 eclipse was a perigee event. (See also §D6.)
H4 I am grateful to H.Thurston's current paper (in this issue) for bringing to my attention the extreme rarity of the occasional 5 month interval between successive *visible* eclipses. (I.e., visible from one site.) See ‡2 §§B8&B10. (He notes the analysis at Neugebauer 1975 p.130, containing an impressively ornate full-page "proof" [repeated uncritically from *Almajest* 6.6] of the superficially-plausible-but-unfortunately-false Ptolemy-Princetitute proposition[95] [emph added]: "An interval of five synodic months is possible for lunar eclipses, provided that the total length of these five months *is as great as possible* . . . . the greatest possible solar motion [combined] with the smallest possible lunar motion." Neugebauer 1975 p.130 n.2 cites a 5 month pair as an example, but had our Princetitute immortal merely applied grade school arithmetic to *his own example* [the Oppolzer 1887

[89] All modern calculations agree closely on the −830/2/4 eclipse's magnitude: Oppolzer 1887, m = 0.4; Meeus & Mucke 1992, m = 0.4; Liu & Fiala 1992, m = 0.5; *DIO*, m = 0.5.

[90] And see fn 30 on the 1301^{g} cycle's accuracy-compatibility with eq. 2. For counter-hints, see fnn 98, 99, & 103.

[91] This remarkable ss lasted from −1840/6/8 (invisible in Babylon) to −830/2/4 (visible there). These two bounding eclipses' magnitudes were, respectively, m = 0.2 digits & 0.5 digits. (Meeus & Mucke 1992 has m = 0.01 for the former.)

[92] The −848/1/25-26 eclipse occurred well below the horizon at Babylon. Its m = 1.0 digits.

[93] See Geminos 18.5 (Aujac ed., p.94), cited by Pedersen 1974 p.163 n.3.

[94] My impression of Babylonian astronomical sophistication is inconsistent with either non-direct explanation.

[95] Perhaps ultimately due to Hipparchos: Pliny 2.57. (One would suppose that the Princetitute could improve upon 2 millennia-stale math. But, then, see Rawlins 1987 n.30 on the *Almajest* 9.3-4 mean motions: *DIO 4.3* ‡15 §I1.) If so, then neither he nor Ptolemy had checked the −831-830 (or any other) pair, which suggests that they knew of no *actual* 5 month-pair. Neugebauer 1975 (p.130 n.2) has no such excuse: §H4.

times he cites], he would have found an interval of $147^{\mathrm{d}}.3$, or less than $29^{\mathrm{d}}.5$/month, which is *shorter* than the average $29^{\mathrm{d}}.5306$ month. Yet another triumph for the Muffia's supreme ability to know the answer to a problem, without having to bother about mere evidence: *DIO 4.3* ‡15 §I3.) Neugebauer 1975 p.130 n.2 did not know of any such interval where both eclipses were visible to active astronomers. (See also *ibid* p.504 n.12 & pp.525f.) So, the very evening when I first received the Thurston paper, I naturally turned to the 9^{th} century BC eclipse trio cited above (fn 56) and thereby instantly found what Neugebauer had vainly scoured Oppolzer 1887 for: the −831/9/9-10 & −830/2/4 eclipses are five months apart[96] — *and* both were visible[97] in Babylon. So, this very rare short observable interval might have been a cause of Babylonian interest in the −830/2/4 eclipse. Which hints at a further possibility (one that does little violence to what we already know of early Babylonian astronomy): that the −831-830 grazing-eclipse-pair marked the first gleanings of the first glimmerings of organized eclipse-prediction in Babylonia. (Note: −831/9/9-10 = ss-start, and −830/2/4 = ss-end — typical for a 5 month-separated eclipse-pair.)

H5 I will next (§H6) examine yet another possibility — and thereby leave us on one of the horn&horn extremes (of our range of choices): was Babylonian interest in the −830 eclipse due to a 5 month passing affair (the most conservative interpretation at §H4) or[98] to a 1000 year religious marriage (§H6) to the ss?

H6 Our final speculation is certainly the grandest — and (since it goes against my own historical expectations) the most enjoyable: long before the Seleukid era's plague of astrologers (whose indoor-tablets so enthrall modern historians), did early Babylonian lunar priests keep (now-lost)[99] records of the eclipses of entire ss?

H7 If the Babylonians specially preserved the −830/2/4 ss-conclusion eclipse, then are we (in recovering it) holding one end of a thread of traditional Babylonian observations which extend all the way back to the first eclipse (visible at Babylon) of this ss *in the 19^{th} century BC*, the partial (4 digit) eclipse of −1804/6/29. This is near the misty era of Hammurabi and Ammizaduga — the time of the very beginnings of Babylonian scholarship.

H8 We may never know the truth. But merely savouring the possibilities is itself a pleasure. (See the beautiful and attractively overmodest conclusion[100] of Neugebauer 1957.)

[96] DR's spotlighting of the −830/2/4 eclipse (at, e.g., *DIO 2.3* p.90) occurred long before his realization that it was part of a 5 month eclipse-pair. Again (as also for the Neugebauer 1975 p.130 n.2 example discussed above), the mean lunar motion in this interval was greater than usual, not less (though the 147.7 day time interval was slightly above average).

[97] The very occurrence of the −831/9/9-10 dawn eclipse (m = 0.6 digits by Oppolzer 1887, & 0.1 digits by Meeus & Mucke 1992) is said by Pogo 1938 to be questionable & by Liu & Fiala 1992 to have not occurred. I find: eclipse began near start of nautical twilight, magnitude m = 0.3 digits (semi-duration about 0.3 hr). (I do not claim to have proved that the eclipse was seen, but I did not prove that it couldn't have been.) Again (as at §H1), the suggestion is: this eclipse might not have been seen at all, unless deliberately looked-for by astronomers who knew enough to suspect that an eclipse *could* appear (see ‡2 §B11), to reward their patience.

[98] See van der Waerden 1974 pp.115-120 for an argument favoring the short-term option. Further support here at fn 30 and at §§G2-G3 & I15 item [d]. Also, the evident lateness of Babylonian regular adoption of the Metonic calendar; though, tracking the Metonic cycle is not the same as tracking the saros. (Longterm-repeat Metonic eclipse-nests occur only after twenty-four 19^{y} cycles = 456^{y}.)

[99] The lack of records is the most obvious conservative argument against DR's §H6 speculation. So, in the absence of other clear evidence, we must here side with conservatism.

[100] Despite Neugebauer's intolerances, he had a becoming self-deprecatory side. (See Neugebauer 1975 pp.vii & 1-2. See also his final top protégé Swerdlow's too-modest remark at *DIO 4.3* ‡13 §B8.) In his only conversation with DR (telephone, 1976/8/14), he said, regarding the reception of Neugebauer 1975: "I expect to be attacked on *all* sides." Neugebauer 1957 p.177 (p.170 of the 1952 ed): "In the 'Cloisters' of the Metropolitan Museum in New York [City] there hangs a magnificent tapestry which tells the tale of the Unicorn. At the end we see the miraculous animal captured, gracefully resigned to his fate, standing in an enclosure surrounded by a neat little fence. This picture may serve as a simile for what we have attempted here. We have artfully erected from small bits of evidence the fence inside which we hope to have enclosed what may appear as a possible, living creature. Reality, however, may be vastly different from the product of our imagination; perhaps it is vain to hope for anything more than a picture which is pleasing to the constructive mind when we try to restore the past." (Ultra-snob Thos.Hoving's *Making the Mummies Dance* NYC 1993 p.350 attempts a hilariously anachronistic projection of the fading modern Freudian fad upon medieval artists' mentalities, in order to impute something saleably salacious to this innocent work.) A fine

I The 800 Sidereal Year Eclipse Cycle & its Metonic Nest

I1 I have found that the smallest[101] number of years in which eclipses will return to the same sidereal point (i.e., will occur at the same star) is 800^y, on the nose. In equation form, this neat circumstance may be expressed thusly:

$$9895^{\mathrm{u}} = 800^{y} = 10738^{\mathrm{w}} + 5^{\circ} = 292205^{\mathrm{d}}1/4 \qquad (20)$$

I2 When first discovering eq. 20, I naturally wondered if the ancients were aware of the cycle (whether or not they knew it was the shortest). And I quickly found the humbling truth: yes, the 800^y eclipse cycle had been discovered by ancient scholars — 2000 years ago! To be specific: they certainly knew of it by the late 2^{nd} century AD, and most probably already had it by 62 AD. (See also fn 110.) [One-fifth of eq. 20 is cited at Geminos 8.40-41.]

I3 Unlike the 690 yr & 795 yr cycles discussed here previously (eqs. 8 & 10-11, respectively), the 800^y cycle does not comprise an integral number of anomalistic returns; thus, its appearance is diffused on either side of the 800^y-mark. Since the discrepancies (vs. the exact 800^y figure) are at 19 yr intervals, we will give this family of returns the name: "The 800 Year Metonic-Nest". Eclipse-pairs in this snug Nest can occur at the following intervals: 743^y, 762^y, 781^y, 800^y, 819^y, & 838^y. (Examples of the extremes:[102] a 743 yr pair, −878/9/19 & −135/9/24; an 838 yr pair, −967/6/17 & −129/6/22.)

I4 The earliest explicitly dated trio of eclipses whose records have come down to us is the Sargon-era Babylon threesome[103] of −720-719, whose times[104] were preserved for us by Ptolemy. The −719/3/8-9 midnight lunar eclipse observation was Ptolemy's favorite[105] early eclipse (see *Almajest* 4.6-9, 6.9). We're about to learn (& see fn 105) that this eclipse was likely central in ancient astronomers' secular reckonings, well before Ptolemy.

I5 We will now demonstrate that the −719/3/8-9 eclipse was probably (though see fn 110) used by the ancient scientist who discovered the perhaps-since-forgotten fact — highly convenient for gauging sidereal yearlength — that lunar eclipses return to the same star in eight centuries (§I1). This scholar (maybe Heron or, more likely, a contemporary)[106]

reproduction of this very tapestry-finale-image of the fenced (sitting) unicorn hung for many years in the apartment of my wife's late mother, Sylvia Linscott Reynolds (long of the Harvard Alumni Fund) and was overlooking her when she died (1994/9/12). It now hangs in our living room, over the Terry clock that for a century enhanced the mantelpiece of the stately old (since vanished) Dennis home, "Hampton" (a mile north of Urbana, MD), where I spent many a childhood summer, and where my wife & I later (1960/6/11) honeymooned.

[101] I of course refer to periods longer than a few 19^{y} Metonic cycles (eq. 30).

[102] The extremes occur not symmetrically about 800^y because the most exact synodic-draconitic commensurability fell not precisely on 800 yrs but rather somewhere between eqs. 20 (800 yrs) & 21 (781 yrs). (Nearer the latter, as we see by comparing the absolute magnitudes of draconitic remainders: eq. 21's 2° is less than eq. 20's 5°.) By contrast, the most exact sidereal-synodic commensurability was between 800 yrs & 819 yrs — much nearer the former, which is why I here refer, throughout, to 800 yrs as the basic interval of this crucial sidereal-return eclipse-cycle.

[103] There was a total eclipse on −720/9/11-12 (before dawn at Babylon), a record of which has not come through to us. Though, it may have been available to ancient scholars; or, it may have been cloudy. (Another possibility: Ptolemy or an earlier scholar may simply have dropped this eclipse because the geometric proof at *Almajest* 4.6 required only 3 data, and Ptolemy was not big on overdetermination. [See Rawlins 1991W fn 224 & Rawlins 2002V.]) But: all three nearby events that survive (*Almajest* 4.6) started before midnight, while the −720/9/11-12 eclipse didn't. Does this circumstance hint (contra §H1) that predictions & anticipations were not yet regularized (fn 98): i.e., this was still back in an age when eclipses were noticed only if they happened to occur at a convenient time of day? (I doubt whether anticipation was this dormant in the 9th century BC — but feel obliged to note that possible interpretation, regardless.) Of the −832-830 tightquad proposed at fn 56, only the −831/9/9-10 grazer (§H4) was near dawn.

[104] I agree (contra R.Newton) with van der Waerden 1988 (p.269) that the times are probably real, while only Ptolemy's attendant computations are fudged.

[105] The −719/3/8-9 eclipse was certainly (*Almajest* 6.9) used by Hipparchos. Rawlins 1985S proposes that Aristarchos also used it.

[106] Note the wisdom of van der Waerden 1963 p.277 on G.Cantor's (familiarly Muffi0se) historical naïvete, regarding Heron's originality. [C.Truesdell's pioneering re-evaluation of da Vinci is equally perceptive.]

used the Heron 62/3/13-14 Alexandria midnight eclipse (Neugebauer 1975 p.846) with the −719/3/8-9 Babylon midnight eclipse (both at[107] the star 49 Vir,[108] whose latitude $\beta = -3°$), in order to found the equation:[109]

$$9660^{\mathrm{u}} = 781^{y} = 10483^{\mathrm{w}} - 2° = 285265^{\mathrm{d}}13^{\mathrm{h}} \qquad (21)$$

I6 Of course, it is possible that the relation was known even earlier, since 781^{y} pairs are common;[110] however, there are reasons for believing that this particular pair (or its associated fn 110 tightquad) is the prime basis for eq. 21 and thus our upcoming shocker, eq. 31: [a] Both eclipses are attested (§§I4&I5). [b] The −719/3/8-9 eclipse has been (fn 108) connected to a specific star (49 Vir). [c] The −719-to-62 pair is unusually neat; both mid-eclipses occurred at local midnight, thus the parallaxes were small (merely $+8'$ in longitude & ordmag $1'$ in rt.asc), and the differential parallax was trifling (fn 110). [d] The solar arc between mid-eclipses fell only 0°.3 short of precisely 281160° or 781 sidereal revolutions.

I7 Eq. 21 produces a value for the sidereal year Y'_{Y}, implicit in *PlanHyp*, of very nearly:

$$Y'_{\mathrm{Y}} = 9660M_{\mathrm{A}}/781 = 365^{\mathrm{d}}1/4 + 1/148 \qquad (22)$$

— virtually identical to the *Almajest* value (implicit in eqs. 16&26), which is about:

$$Y'_{\mathrm{J}} = 36000Y_{\mathrm{J}}/35999 = 365^{\mathrm{d}}1/4 + 1/147 \qquad (23)$$

[107] In both longitude λ & latitude β (whether topocentric or geocentric), both eclipses occurred nearer to the brighter star 51θ Vir (than to 49 Vir) but I adopt here the irresistible (fn 108) identification by Ptolemy (or his source) of 49 Vir as the star that both eclipses occurred at. In right ascension α there was in fact little to choose between the stars' proximity to the eclipses. (The two stars' α were only $33'$ apart in 720 BC; $38'$ apart in 62 AD. And lunar α parallax is null for an apparent-midnight eclipse. The −719/3/8-9 mid-eclipse's α virtually equalled 51θ Vir's α, while the 62/3/13-14 mid-eclipse's α was nearer 49 Vir's.) So the 49 Vir connection implies that 8th century BC Babylonians did not yet place events in the ecliptical system. Possible hypotheses for where the original record said mid-eclipse occurred: [a] on the nearly-north-south line between 49 Vir & 51θ Vir; [b] "above" 49 Vir.

[108] See fn 107. One must make explicit the implication here (on which a direct estimate of the sidereal year depends: fn 110), that some of the early Babylonian observers preserved not only the time & magnitude of an eclipse but also the identity of the star at which it occurred. (If not, then eq. 21 was rather in the nature of a lunar definition of the sidereal year: akin to eq. 30's better-known definition of the Metonic "tropical" year.) For further evidence consistent with this unsurprising hypothesis, see *DIO 2.3* ‡8 fn 20, which reveals that Ptolemy accepted that the −719/3/8-9 mid-eclipse occurred at 49 Vir. Ptolemy (*Almajest* 4.6) put the eclipse at 163°3/4. And he put 49 Vir there also: merely adding 8°1/2 (the probable Ptolemy rounding of 8°34′ — see, e.g., Toomer 1984 p.452 n.69) for §I10 precession gave 172°1/4, this star's longitude at *Almajest* 7.5 — with a quarter-degree ending that is found (*DIO 4.1* ‡3 fn 11) in only 5 of the Ptolemy star catalog's longitudes (less than 1/2 of one percent of the 1025 stars), all 5 of them associated with lunar or Venusian conjunctions: again, see *DIO 2.3* ‡8 fn 20. (Ptolemy took the reported −719/3/8-9 conjunction as ecliptical, though the 1st century AD discoverer of eq. 21 evidently realized it was equatorial.)

[109] Eq. 21 can be re-rendered: $781^{y} = 285265^{\mathrm{d}}1/3 = 9660^{\mathrm{u}} - 3°$, & eq. 20: $800^{y} = 292205^{\mathrm{d}}1/5 = 9895^{\mathrm{u}} - 0°$. Both equations are based upon the real mean periods for epoch Phil 1 (−323/11/12).

[110] And eclipses 800^{y} apart are also common. Indeed, the 800^{y} cycle could have been discovered by the same astronomer who found the 795 yr cycle, since the start of the −30/2/10 eclipse was easily visible in Alexandria (not Babylon), just 800 sidereal years after the −830/2/4 eclipse (§D2) presumably used for finding the 795 yr relation (eq. 8). Both the −830 & −30 eclipses occurred around 77σ Leo. Note that no Nest relation is very close to integral in anomalistic returns. (In this respect, the infrequent 743 yr cycle is the best of the lot: $9190^{\mathrm{u}} = 9849^{\mathrm{v}} + 14°$.) Eq. 21 (781 yrs) is poor in this regard ($9660^{\mathrm{u}} = 10353^{\mathrm{v}} - 92°$), but it was used anyway — and to good effect, since *the interval is so long* that even a few hours of anomalistic-nonreturn-caused error had little effect on a direct-division result: merely 1 part in ordmag a million. Note that, for gauging the sidereal (star) yearlength from the empirical data that went into eq. 21, one needed merely each mid-eclipse's: [a] time, & [b] position vis-à-vis 49 Vir. (The parallaxes virtually cancelled for this lovely pair: in fact, the parallaxes' difference was ordmag $1'$! See §I6.) If the data for [b] existed (fn 108), then the lunar anomaly — though useful for gauging synodic monthlength — would be unnecessary for estimating the sidereal year. Moreover, there were other (adjacent) 781 yr-pairs. Of the −720-719 eclipse-tightquad (see fn 103), all four mid-eclipses were visible in Babylon. And most had accessible matching eclipses in 61-62 AD — all were visible at mid-eclipse in Alexandria except the last, the end of which was visible around Persepolis & east thereof. The four pairs [intervals in brackets]: −720/3/19-20 & 61/3/24-25 [285264d23h], −720/9/11-12 & 61/9/17-18 [285266d02h], −719/3/8-9 & 62/3/13-14 [285265d01h], −719/9/1-2 & 62/9/7-8 [285265d20h]. Dividing just the 3rd interval by 781 would have given a sidereal year of 365d1/4+1/163; but averaging the four intervals or the two extreme cases would have produced a mean interval of 285265d12h. (Very near to 9660^{u} = 285265d13h: eq. 21.) Dividing this by 781 produces a sidereal year of 365d1/4 + 1/149, which agrees closely both with the truth (365d1/4 + 1/154) and with eq. 22. (See fn 54.)

I8 Eq. 21 provided ancients the number of sidereal months in 9660^{u}; thus, combining this information with eq. 2 gives us the length of the *PlanHyp* implicit sidereal month:

$$S_{\mathrm{Y}} = 9660M_{\mathrm{A}}/(9660 + 781) = 123234713^{\mathrm{d}}/4510512 = 27^{\mathrm{d}}.32166836048 \qquad (24)$$

This was accurate to 1 part in ordmag 10 million — and it agrees very closely (to 1 part in 57 million) with the *Almajest* implicit sidereal month:

$$S_{\mathrm{J}} = 1/[1/M_{\mathrm{A}} + 35999/(36000Y_{\mathrm{J}})] = 27^{\mathrm{d}}.32166858515- \qquad (25)$$

I9 Since we are about to reconstruct ancient transformation[111] of eq. 21's sidereal cycle into a tropical cycle, we will first set out the ancient geocentrists' standard (if inaccurate) relation between the sidereal year and the tropical year, as stated explicitly at Ptolemy's *PlanHyp* 1.1.5 (Heiberg 1907 pp.78-81 or Neugebauer 1975 pp.901-902 & eq.7):[112]

$$35999^{y} = 36000^{\mathrm{y}} \qquad (26)$$

I10 Now, using [a] the implicit precession of 1°/century (eq. 26 or *Almajest* 7.2-4),[113] & [b] conventional ancient degree-fraction rounding (which would express 7°.81 as 7°4/5), we convert eq. 21 into a relation between the length of the tropical year and the length of the synodic month:

$$781^{\mathrm{y}} + 7°4/5 = 9660^{\mathrm{u}} \qquad (27)$$

Eq. 27 permits an overprecise evaluation of the tropical year (1^{y}) in synodic months:

$$1^{\mathrm{y}} = 5796000^{\mathrm{u}}/468613 \qquad (28)$$

I11 Next, we look for a less cumbersome expression (for the tropical year) which is nonetheless sufficiently[114] agreeable. We start by displaying eq. 28 as a continued-fraction:

$$1^{\mathrm{y}} = 12^{\mathrm{u}} + \cfrac{1}{3 - \cfrac{1}{4 - \cfrac{1}{2 + \cfrac{1}{448 - \cfrac{1}{7 - \cfrac{1}{8}}}}}} \qquad (29)$$

Truncating eq. 29 will now reveal two important attested relations.

[111] This is the very same sidereal→tropical transformation-procedure which was central to Rawlins 1985S and Rawlins 1987 p.237 & n.27.

[112] See *DIO 2.1* ‡3 fn 18. [**NB: eq. 26 appears in the Ptolemy paragraph just preceding that containing eq. 31.**]

[113] If we simply add 1°/century to convert sidereal years to tropical years, we are effectively converting eq. 26 into 36000 sidereal yrs = 36001 tropical years. The error caused by this procedure is about 1 part in 36000 squared — or less than 1 part in a billion.

[114] Eq. 31 approximates eq. 27 to a precision of 1 part in ordmag 10 billion. [Implicit cont'd-fract precision: a billionth.] And the tiny rounding at §I10 [b] (which converted eq. 21 into eq. 27) affected our math by barely 1 part in 30 million (& see fn 113). (Muffiosi who've accepted the false, nontrivial, atypical and-or unknown roundings which are exposed at §§B6&B7 and eq. 33, will have difficulty consistently objecting to our perfectly ordinary & trifling ancient rounding here.) In sum: all the approximations, leading from the original empirical 781 yr cycle equation (eq. 21) to the attested result (eq. 31), corrupt the original ratio by less than 4 parts in 100 million. (These degradations were trivial compared to eq. 31's empirical error, which was roughly 1 part in a million: see fn 110.)

I12 Truncation[115] after the 3rd fractional term (the 2) will produce the famous[116] 19^y Metonic[117] cycle (a valuable artificial[118] identity still used to compute the date of Easter):

$$19^{\mathrm{y}} = 235^{\mathrm{u}} \tag{30}$$

I13 But, truncating after the next term (the 448) yields a far, far more precise expression:

$$8523^{\mathrm{y}} = 105416^{\mathrm{u}} \tag{31}$$

I14 Given the size of the components in our eq. 31, it can hardly be an accident that **precisely this equation is propounded in the final extant astronomical work of Ptolemy** (late 2nd century AD), at *PlanHyp* 1.1.6 (Heiberg 1907 pp.78-79 or Neugebauer 1975 p.901 eq.3). Thus, during our above development (eqs. 21, 26, 27, & eq. 29 → eq. 31), we have been walking in the very math-steps of eq. 31's ancient discoverer. (Inducing ancient realities is a refined pleasure. Which very seldom gets this delightful.) For probability-context: this is the only place in Ptolemy's works where he explicitly[119] provides the ratio of the tropical yearlength to the synodic monthlength. (Another comment in passing: Muffiosi will reflexively attempt to ignore or[120] damn eq. 31 by claiming that the historical context — read: *their* idea of same — does not support any connexion with eq. 21. It will not occur to Muffiosi to ask: when is a discovery so powerful & central that it forces re-evaluation of one's perception of context? See, e.g., fn 137 & Rawlins 1991H §§A7&B12.)

I15 Note also a few other related coincidences: [a] The 781^y interval is the only one of the six members of the 800^y Metonic nest (§I3) that yields eq. 31. [b] It is also the only member of this sextet which we know was observed (§I6 item [a]). [c] And one of the two eclipses, on which we are proposing that this star-year relation (eq. 21) was founded, has been precisely related[121] to a (*very* unusually-rounded)[122] conjunctive star in the Ptolemy catalog. [d] Finally, do not miss the provocative fact that the main two ancient cycles recovered[123] in this paper, 781 yrs (eq. 21) & 795 yrs (eq. 11) — interval-lengths agreeing to within 2% — are both (as might be expected, if we are on the right track) a little less than the c.900 yr time-interval (fn 51) from the start of regular Babylonian records (§G1) down to Ptolemy, whose corpus contains both cycles *in the same paragraph of the same work*. (Note: if lasting Babylonian records actually began with the −832-830 trio, then the first ss ever tracked from start to finish could have been the 974^y series of 55 eclipses lasting from −831/9/9-10 to +143/4/16-17. The final event, m = 0.5 digits and visible in Alexandria, occurred while Ptolemy was compiling his output. In fact, Ptolemy relays, at *Almajest* 4.9, a report of this very ss' next-last eclipse, +125/4/5, m = 1.8 digits.)

[115] Similar ancient continued-fraction truncation (explaining Eratosthenes' famous obliquity) at Rawlins 1982G p.262. [Theory initially proposed by Neugebauer in 1943: see *DIO 2.1* ‡3 fn 26.]

[116] See fn 79 and van der Waerden 1974 pp.103-105, 246-247.

[117] The 19^y cycle-calendar's inventor, Meton, was portrayed as a fake by his conservative contemporary, Aristophanes: *The Birds* kicks Meton off the stage, and *The Clouds* accuses newfangled-calendar reformers (like Meton) of starving the gods by confusing them about the dates of their eats-festivals.

[118] See fn 79 & Rawlins 1991H fn 1.

[119] Of course, the Hipparchos-Ptolemy values for the yearlength (eq. 16) & monthlength (eq. 2) imply a ratio (which agrees with eq. 31 to better than 1 part in 10 million). Though Ptolemy is our source for eq. 31, there are reasons for doubting that he (who was not a scientist) discovered its basis (eq. 21). (E.g., the Heron eclipse was about a century before Ptolemy; and see fn 108.) Heron or Menelaos are more likely figures. (Even more likely: Anonymous.) However, the precise expression eq. 31 may well be mathematician Ptolemy's own creation.

[120] Wide range of Muffia sleights explored at Rawlins 1991W §H2 & *DIO 2.3* §C.

[121] At §I6 item [b].

[122] See fn 108 & *DIO 2.3* ‡8 fn 20.

[123] Neither of these two cycles (781 yrs & 795 yrs) is recognized in van den Bergh 1955 (nor is that of fn 30). Indeed, both cycles have apparently lain unknown for the nearly two millennia from antiquity to the present paper.

J Testing History-of-Silence's Archons

J1 But, for reasons given previously,[124] the Princetitute-Muffia-O.Gingerich [PRIMO] Hist.sci cult cannot own that DR has contributed to "their" field.

J2 Therefore, I offer the foregoing reconstruction (eqs. 21&26→eq. 31) to the PRIMO cult, as a simple integrity-test: Is any member (§J4) of this clique, ANY member, willing merely to agree that the relation of eq. 31 to eq. 21 is sufficiently likely (to be a real not chance connection) that it should be cited as merely of *possible* historical value? (Or should Muffiosi continue to recognize the merit only of misarithmetic that confirms its own cult's totally-flawesome prejudices? — e.g., eq. 5, Rawlins 1991W §C7, & *DIO 4.1* ‡4 §A.)

J3 No matter how sensible this request may seem (to the uninitiated), it cannot be acceded to — because Muffia-reaction-to-DR is a rigid proscription-prescription (fn 7). Reason: if a single R.Newton-DR discovery is admitted by Muffiosi to be of potential worth — an epochal event yet breathlessly awaited by *DIO* readers — then it becomes impossibly difficult to justify a continuation of two decades of religious (fn 124) Muffia condemnation of RN-DR. Better to try pretending (despite multi-layer contextual consistency: §I15) that eq. 31's exact match to Ptolemy is just a fluke — and not just *probably* but *certainly* a fluke. (How else justify *total* nonacknowledgement of RN-DR contribution? And, an increasingly dominant factor: how else avoid admitting a 1/4-century of false & not-excessively-competent[125] Muffia slander against important contributions to the very field where Muffiosi pretend to ownership and exclusive expertise?) Note well the cult's inflexible rule (fn 7): the Muffia doesn't say that DR is *almost*-never convincing. It says unqualifiedly and all-encompassingly (as past & future are revealed with equal clarity to the most wise): NEVER convincing. The omniscient for-all-time surety is nothing short of a mental & spiritual miracle (revealing why DR so earnestly promotes[126] solicitous, *packed-in-cotton* preservation of Muffiosi). *DIO*'s readers are invited to join us in awed obeisance:
where outside the Princeton Institute-Muffia can such god-like intelligence be found?

J4 Copies of this *DIO* will be sent to: O.Gingerich, N.Swerdlow, G.Toomer, A.Aaboe, B.Goldstein — the usual unusuals — with *DIO*'s ever-so-humble request here that they take the §J2 test. (I.e., can Hist.sci archons attain to the calmness & impersonal fairness required to recognize merit *even in a scholar they despise & a journal they fear*?) Surely, mailing *DIO* the word "Yes" or the word "No" should not tax the literacy-limits of the ancient astronomy field's Ivy League deities. [Note updated 2013: Muffia-circle reaction to the astonishing eq. 31 match? No replies. No citations. More succinctly: no surprises.]

J5 OK, it's not that I have much confidence that any PRIMO scholar possesses the intelligence (primarily a feel for probability)[127] and the integrity that would be required to pass this test. However, [a] There's no harm in trying. (Yet again.) [b] *DIO* wishes to leave behind as clear a record as possible of proud academic archondum's antibody-rich ability to remain unifiedly immune to evidence, logic, & equity — even for decades on end. [Muffia-vetted, *DIO*-nonciting B.Goldstein 2003 p.70 tests (fn 119) Hipparchos vs eq.31, but won't ask: how (eqs.26-31) *did 105416&8523 occur in the 1st place*? Pass the cotton!]

J6 The operative principle here at §J3 is a solid piece of academic-climber-reasoning: in any controversy, the scholar who does business (and soirées) with the most archons, is the one who's right. No exceptions to this rule can be admitted without implicitly defiling archonal majesty, most dangerously by creating impious infirmity (even skepticism) about whether academe REALLY NEEDS ARCHONS. Remember *On the Waterfront*'s labor-gangster-boss, Johnny Friendly, reacting to the horror of just one person's defiance of his fiscal control of commerce on the docks: "First, [this guy] crosses me in public and gets away with it, then the next joker — pretty soon, I'm just another fella around here."

[124] E.g., fn 120 and *DIO 2.3* ‡6 §F4 & *DIO 4.1* ‡4 §B2.

[125] See ‡3 §B1 and-or *DIO 4.1* ‡4 §B4.

[126] E.g., fn 141. Also: Rawlins 1991W §H3 & *DIO 3* §L8.

[127] Rawlins 1991W §H3 & *DIO 2.1* ‡3 fn 23.

K How Toppe Scholars Discover a Lunisolar Cycle

K1 *DIO*'s silly recourse, to deriving Greek lunisolar cycles from *Greek* methods and actual lunisolar observations, merely shows how amateurish non-Muffia scholarship can get. For a lesson from The Experts (who regularly declare that those who disagree with them are incompetents: §B5 & *DIO 2.3* ‡6 §E2), we turn to the paper of Muffia genii B.Goldstein (Muffia capo) & A.Bowen (Inst Res Class Philos & Sci, Princeton — publisher of Princetitute-supported Britton 1992) in the 1995 May *Journal for the History of Astronomy*. (The paper is a jollygood joke. One trusts the authors knew this.)

K2 Pliny 2.53-54 includes a famous passage on Hipparchos' 600^{y} of eclipse calculations. G&B propose that everybody else has misunderstood this to mean an interval, when it really (fn 129) means a lunisolar cycle. Thus, their paper's title is: "Pliny & Hipparchus's 600-Year Cycle". Since Muffiosi regard Hipparchos as "virtually a closet Babylonian in Greek drag" (Rawlins 1991W §E1), the 600^{y} period must (fn 27) have a Babylonian[128] origin. (Never has adding needless-to-add been so: needless.) Thus, borrowing a notion of N.Swerdlow, G&B start with the well-known Babylonian version of our eq. 30, namely:

$$1^{\mathrm{y}} = 12^{\mathrm{u}}22'06''1/3 \tag{32}$$

G&B then round this to the unattested expression

$$1^{\mathrm{y}} = 12^{\mathrm{u}}22'06'' = 7421^{\mathrm{u}}/600 \tag{33}$$

This rounding (which, were it RN-DR's, would be scorned by G&B as "fiction":[129] §K6) is then converted (B.Goldstein & Bowen 1995 p.157) into the equally unheard-of "cycle":

$$600^{\mathrm{y}} = 7421^{\mathrm{u}} \tag{34}$$

K3 The trifling inconvenience that 7421^{u} is not an eclipse cycle is handled in the most artfully Muffiose fashion: it isn't mentioned. (This, even though the Pliny 2.43-57 context is: eclipses.) Nor is 7421^{u} an anomalistic cycle. In fact, 7421^{u} doesn't equal anything recognizable, other than roughly 600^{y} — and even that equality isn't exact enough (as we'll see in §K4) to be worth the slightest notice.

K4 Compare to our genuine & remarkably precise 800^{y} cycle, eq. 20. (See fn 109.) That is, $800^{y} = 9895^{\mathrm{u}} - 0^{\circ}.4$; by pathetic contrast, $600^{y} = 7421^{\mathrm{u}} + 90^{\circ}$ and $600^{\mathrm{y}} = 7421^{\mathrm{u}} - 13^{\circ}$. As for draconitic commensurabilities: $9895^{\mathrm{u}} = 10738^{\mathrm{w}} + 5^{\circ}$, but $7421^{\mathrm{u}} = 8053^{\mathrm{w}} + 86^{\circ}$.

K5 However, now that the Muffia has opted for its 600^{y} cycle, not for DR's 800^{y} cycle, the former will be uniformly regarded as superior and will be the only one cited. Just modern academe's standard business-ethic priorities talking.

K6 Further comments: [a] The *JHA* actually deemed G&B's bit of creative number-juggling to be worth four pages of article-space. [b] Meanwhile, the solid (if *JHA*-embarrassing) math of H.Thurston (*JHA 26.2*:164) was merely a little "Note" in the same issue. [c] On 1994/5/8, G&B called all DR's work "fiction", since it was (allegedly) reconstruction. (Similar attack by Bowen 1995 against Thurston: *DIO 4.3* ‡15 §E3.) So, I leave it to Muffia lawyers to explain why G&B's 600^{y} "cycle" isn't thus also to be classed as Fiction — by *their own* on-again-off-again[130] anti-reconstruction criterion.

L The Long View

Summing up the ancient-astronomy revelations of §A-§I:

L1 We can now fully appreciate the cleverness of pre-100 AD Greek astronomers' exploitation of the invaluable treasure of ancient Babylonian eclipse observations.

L2 The antiquity of the data used by these scientists has here been shown to be at least a century older than the earliest data used by Ptolemy: the 721-720 BC trio.

L3 The Greeks' resulting awareness of far longer eclipse cycles than previously suspected has been demonstrated beyond reasonable doubt.

L4 However, there is an implication in all of the foregoing that may escape notice if not highlighted here. A striking aspect of what we have been learning — both above & in Rawlins 1985S — about the Greeks' discovery of lunar period-relations, is that the same few oldtime eclipses keep turning up (e.g., fn 52): −830, −720-719 (trio), & −620. This suggests that either: [a] the Babylonian records available, to Greek scientists of the 3rd-1st centuries BC, were alot less complete than Ptolemy indicates (§G1); or, [b] the records were then pretty full (fn 79 [iii]), but the eclipses not selected for analysis & publication at this time were lost[131] to later investigators (fn 133).

L5 Further, it seems that the 569 yr-cycle (eq. 12) was never discovered in antiquity. Was that oversight just a piece of bad luck? (See §E2.) It is a peculiar omission, especially since the 569 yr-cycle is (vs. the 795 yr-cycle, which *was* discovered [eqs. 10&11]): [a] more accurate, [b] over 200 yrs shorter, and [c] much more frequent.[132]

L6 Again (§L4), the most obvious explanation is that there were very few 8th-6th century BC data available to (classical-era) Greeks.

L7 Another potential implication: little valid new lunar period-relation research occurred as late as Ptolemy's day, when 569 yr-cycle pairs could have been isolated by using older data from as late as c.400 BC — by which time it is generally assumed[133] that Babylonian data were plentifully available. In Greece: we have the Thales 6th century BC legend; and astronomers Meton[134] & Euktemon were already at work before 400 BC. Yet, Ptolemy

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[128] Bowen 1995 takes it for granted that Geminos & Pliny (emph added): "undertook to assimilate Babylonian celestial *science* in a cognitive structure that adhered to Greco-Latin requirements of what counted as proper science." How many *Isis* readers will know that this presumptive (R.Newton 1991 §D14) evaluation is merely a [bedrock] fantasy (Rawlins 1991W §E4) of the Muffia cult? — lacking the very "independent confirmation deriving from the times in question" which the same review (Bowen 1995) requires of nonMuffiosi. See, e.g., the learned analyses of Dicks 1994. Note: [a] No extant ancient Babylonian text explains Bowen's alleged Babylonian "science" of the heavens — no discussion of orbits or instruments. (See fn 27.) [b] What sort of *scientists* would (Rawlins 1991W §E3) order the planets as Babylon did, namely, astrologically good-to-bad (Jupiter-Venus-Mercury-Saturn-Mars: Neugebauer 1957 p.169)? — instead of physically, as the Greeks did (Mercury-Venus-Mars-Jupiter-Saturn).

[129] See, e.g., the gotta-have-ancient-attestation ploy in Bowen 1995, cited at §K6 & *DIO 4.3* ‡15 §E3. (Bowen 1995 says Thurston ignores *real* Hist.sci scholars' "rigorous demand for independent confirmation deriving from the times in question" and instead lets "reconstructions . . . supplant, or be confused with, the data reconstructed.") Evidently, G&B consider the explicit 600 yr figure the sort of "rigorous" attestation which lesser scholars lack! — even though the 600 yr interval has long been rightly (*DIO 1.3* fn 211) recognized by the Muffia's saner Neugebauer & Toomer as (not G&B's sexagesimal-expression by-product but) simply the time-span from the famous epoch Nab 1 down to Hipparchos' epoch. In any case, we thank the *JHA* for publishing yet another precious canard, which so efficiently demonstrates (better than our own *J.HA* could) the risible inductive-sterility of G&B's much-touted historical method. Incidentally, assuming that the catfight I witnessed at the end of the 1994/5/8 Dibner Inst conference was real, it would seem that this avenue is considered too extreme even by the central Muffia (which has given us some important reconstructions, e.g., *DIO 1.3* fnn 277&280). Another passing comment: those most drawn to the anti-math-reconstruction position are, by an inexplicable coincidence, the least mathematically trained. However, they do occasionally make their own sorts of contributions. (See, e.g., my own vital debt to B.Goldstein: cited *DIO 3* fn 93.) So, I am grateful that they continue to be active. (It would be pleasant someday to encounter similar tolerance on their side.)

[130] For similar double-standard act (and quotes from the Bowen 1995 review), see fn 129.

[131] Since the Enlightenment, a primary criticism of Ptolemy has been similar: he should have published more data than theories. The suggestion here (at [b]) is that he was not the only ancient (or modern) guilty of this oversight.

[132] The reason that 569 yr-cycle eclipse-pairs occur more often than 795 yr-cycle pairs is that the 18° draconitic remainder in eq. 12 is critically less than the 22° remainder in eq. 11. (Because the latter remainder is so near the 25°– outer limit of pair-possibility, 795 yr-cycle eclipse-pairs tend to occur near perigee and are impossible near apogee: §D1. By comparison, since a remainder less than 19° will permit eclipse-pairs for any anomaly, this generous condition applies to 569 yr-cycle pairs (given eq. 12's 18° remainder). Thus, there will be no centuries-long periods when such pairs are virtually nonexistent, as we found was true for 795 yr-cycle pairs: §F11.)

[133] However, it is possible that whatever old Babylonian data still survived were (by Ptolemy's time) mere hand-me-downs (§L4), effectively selected-pruned centuries ago by the publications of scholars in the era of high ancient science, whose research-tradition was no longer carried on. (I.e., the original Babylonian data may not have survived except in later works' sparse secondary citations of them.)

[134] No explicitly Hellenistic eclipse observation is extant earlier than the time of Kallippos: the −330/9/20-21 Arbela eclipse (observed by Alexander's army) reported at, e.g., Pliny 2.180 & *GD* 1.4.2. However, Meton's luni-solar cycle (epoch −431) must have been based upon eclipse data.

does not use (or cite) any eclipse-observations between −490 (*Almajest* 4.9) and −382 (*Almajest* 4.11) — and both of these reports are crude (fn 78) & Babylonian.

M Greek, Babylonian, & Princetitution Foundations

M1 Which segues us to a Babylonian-vs-Greek contrast that needs to be made explicit.

M2 There is a cult of modern scholars (Muffia&O.Gingerich) who impute serious sophistication[135] to late Babylonian astronomy, and who have thus for decades intermittently hoped[136] to find connexions between Babylonian tables and empirical sources. However, nearly a century after Kugler 1900 launched this idea (i.e., nearly 5% of the vast timespan since the end of Babylon!), not one Babylonian astronomical parameter or ephemeris (of hundreds) has been successfully related to any specific, dated Babylonian observation.

M3 Indeed, to Muffia catatonic horror, the only Babylonian parameter ever precisely connected to anybody's empirical data is based entirely upon Greek[137] observations!

M4 Two comments: [a] It is obvious that the Muffia's energetic Babylonian advocates have nothing at all to compare with the precise connexions exhibited, e.g., in the present paper, showing Greek use of empirical data. (Which is why funny arithmetic so often enlivens briefs for sacred Muffia viewpoints: e.g., §B5 & §J2.) [b] Babylonian-obsessed Muffiosi's *uniform* unwillingness (even while fitfully conjuring up vaporous[138] speculations of Babylonian empiricism), to acknowledge the simplest evidence of §M3 (fn 137), betrays such truly pathetic intellectual-shock paralysis[139] (how-do-we-get-out-of-*this*-one?), and such a hilariously inverted sense of what has and what has not been established (by modern investigations of ancient science), that it is now obviously long past time that the more fossilized members of this strange cult be relieved of the power to determine[140] who does and who doesn't get funding[141] in the History of science community, a power which: [i] is the primary reason Muffia follies are catered to by young scholars (who are thus forced into accommodation with that debate-shy cult's traditional hide&suck approach to the daunting task of achieving political advancement without intellectual advancement), & [ii] has been so arrogantly misused that it's now just an ongoing embarrassment to modern academe.

M5 Who would ever have predicted that the Princeton Institute — the last intellectual home of Albert Einstein — would become involved in promoting[142] idée-fixe kookery, while blatantly going for suppression[143] of legitimate & highly-recommended scholarship?

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[135] See Rawlins 1991W §E3, fn 87, §G3, & fn 266.

[136] E.g., contrast the noble aim expressed at Brack-Bernsen & Schmidt 1994 p.187, with the despair of Neugebauer quoted in this paper's opening text-for-the-day (fn 4).

[137] The −431 & −135 solstices of Meton & Hipparchos, resp. See Rawlins 1991H §A6 (& eq.6) — and (despite attempted Muffia suppression: fn 7) its acceptance by, e.g., van der Waerden (Rawlins 1991H fn 4), Moesgaard (*DIO 2.1* ‡2 §D2), Thurston 1994E pp.123&128, & Dicks 1994 fn 37. See also fnn 15&16 here.

[138] Needless to say, §H here is plenty speculative. But it's hard granite compared to some of the Muffia's gas about, e.g., Hipparchos' nonexistent Babylonian confabs. (See skeptical critiques by: Dicks 1994 §C2 and *DIO 4.2* ‡9 §K9.)

[139] Which explains the seemingly wild §I14 DR prediction that the Muffia will probably refuse even to admit the *possible* worth of the current paper's extraordinary fit at eq. 31. DR's long acquaintance with evidence-immune cultists (in&out of the academic establishment) accounts for this paper's title — as does the Princetitute's continuing effective-endorsement of Muffia arrogance, suppression, cult-fundamentalism, mismath, & false slander (e.g., §B5, fn 5, & *DIO 4.3* ‡15 §I3). Other obviously-valid *DIO* findings Muffiosi still noncite: fn 137, *DIO 3* fn 54 [Toomer vs. Manitius or DR], *DIO 4.2 Competence Held Hostage #2* Table 1, & Rawlins 1991W eqs.23&24.

[140] If such power were turned over to younger scholars, these would include budding Muffies — which would permit testing the optimistic hypothesis that the rigid behavior of some young Mufflets is simply due to fear (*DIO 4.3* ‡15 §G14) of losing funding now controlled by petrified-brain archons.

[141] As is clear from Rawlins 1991W fn 266, I am not trying to starve enemies. (That's the Muffia's tactic: *ibid* fn 16 & *DIO 4.2* ‡7 §B10.) I hope that Muffia research will continue (fn 126) — but without the usual censorial (fn 7) arrogance, and without fiscal threat (*DIO 4.3* ‡15 §G14) against even the *consideration* of dissent.

[142] Stark example at *DIO 4.2*'s *Competence Held Hostage #2* (Table 1 at p.56). And see here at fn 5 & fn 27.

[143] See, e.g., fn 7, §J2, & Rawlins 1991W fn 170.

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