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 More¹ MuffieMyth MirthBalls

by Rawlins²

Princeton-Institute-Muffia³ History-of-Science Wisdom:

The conclusion⁴ 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.

---

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 (and 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: Muffenymythmirthballth . . . ?)

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 fn 20, Rawlins 1991W §B1, & DIO 4.2 §§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 §§C5, C7, & C12. See fn 5. Also “Black Affidavit” at DIO 1.3 §4. And “Casting Pearls Before Pyglets” at DIO 4.1 §4.


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 than has been generally believed by Historians of science (Hist.sci). The following paper bears critically upon this larger issue.

Muffia Omertà

Secret, 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, despite the Muffia’s worst efforts):


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 cover-up-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 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-Manlyacrobatics. Eq. 31 will add a further invigorating challenge to the cult’s dedicated 26 dirty-tricks 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 venemous 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.

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 saros11 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 (Almajest 4.2):

\[
4267^u = 4573^v = 345^d - T^o 1/2 = 4630^u 1/2 + 11^o = 126007^d 01^b
\]

(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], \(y\) = sidereal years, \(K\) = Kallippic years [§F1]. Degree-remainers merely signify 360ths.)

A2 Ptolemy said that this relation14 was the source of Hipparchos’ value for the length of the synodic (calendaric) month:

\[
M_A = 29^d 3150'^o 08"' 20''' 29^d 53059413580
\]

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 into 126007 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 1991§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].

---

6 E.g, Rawlins 1982G p.265 (& attendant correspondence with Jais). 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.
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, 29035407b 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): anon 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 Metonielly-defined “tropical” year (eq. 30: \(1^o = 235^o/19\), 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 superflous even were the last hexadecimal place accurate. Several other values (e.g., eqs. 8 & 13) are also rendered here in varying degrees of overprecision.
§5 traced to Hipparchos Babylon’s “System B” yearlength 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 ~620/242 and ~275/4/18 may have formed the particular ancient 345 yr-interval (see eq. 1) on which was founded 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 in 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 familiar22 equation:

\[ 251^u = 269^u \] (3)

B His Llardship Sweetens Yale’s Rep with Fudge-Babylonienne

B1 But there is a longstanding mystery about eq. 1: the ~7°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 clever21 discovery that the ~7°1/2 remainder in eq. 1 could almost be explained by assuming use of the

17 See fn 137. It is an indication of how highly Hipparchos was regarded in antiquity that his ~134 S.Sostice had become internationally accepted on a level with Meton’s revered ~431 observation.

18 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 1991W fn 9. (See DIO 4.1 fn 10, and “Casting Pearls Before Pyglets” at DIO 4.3 fn 24. Swerdlow’s most polished forced-math gem, cited at DIO 1.1 § 5 fn 7, debated 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 influence27 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:

26 For catalogs of other entertaining instances of Muffia (& Princeton Inst) fog, see “Black Affidavit” at DIO 1.3 §10, and “Casting Pearls Before Pyglets” at DIO 4.1 §4 (“Muffia-Catalog: the Incompetence-Chargers’ Competence”). But note also his distancing himself (see §37 & end of fn 29) from Aaboe’s egregious eq. 5 sleight.

27 Question: has a single case of unattributed Greek use of Babylonian astronomy ever been established? (Why not?)
force & artifice. (Princetinstitute-Muffia Omertà 1996 January DIO-J.HA 6 §1

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 & §B14) parallax-guesstimates would degrade the accuracy of apogee-determination, it is reasonable23 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°23′) used by genuine ancient astronomers is nowhere directly attested in extant works. (See Table 1 of Competence Held Hostage 42 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 apogee’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 trepidation7 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◦ relation — namely, 4573° ± 345° − 7°1/2 — be unacceptably hybrid & inconsistent if it used anomalistic returns for the Moon, but not for the Sun?

C9 However, in §9’s method might be used by either side of the helio-vs-geo-centrist controversy.

See Rawlins 1982E eq.28 fn 9 & fn 11

C10 Neugebauer 1975 pp.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 desires credit for showing (Neugebauer 1975 p.298) that the explanation for eq. 1’s −7°1/2 remainder could be Greek.

C11 Best ordmag-1000y synodic-anomalistic cycle: 16092° = 17246′ = 17463° − 9° or 1301° + 3° = 47529608h (double last cycle in fn 57). Evident nondisclosure 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° cycle’s variations than with those of eq. 1. fn 56. But averaging several 345° pair-intervals would (fn 18) produce comparable accuracy.)

C12 Almajest 4.2 also speaks of eq. 14 as sidereal, which (fn 66) is not. (i.e., use of the sidereal year in eq. 14 will not produce the 10°2/3 remainder cited.) But it is 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.

C13 Neugebauer 1975 pp.293&298 suggests 2 different possible values for Hipparchos’ sid yr: 365d1/4 + 1144 & 365d1/4 + 1100; adding that Hipparchos may’ve worked with a slightly different constant. [ON who thythers’ 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′44″33′′36″. c.1000 times more precise than accurate. Same muffins (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.]

96 January  DIO-J.HA  6 1

C  Thus, let us test quantitatively the hypothesis38 that the ancients knew39 the Sun’s anomalistic motion; we start by proposing40 an accurate value \( G_s \) for the solar anomalistic year, rounded in typical ancient41 fashion:

\[
G_s = 365.25/4 + 1/100
\]

(6) and divide it into eq. 1:

\[
4261TA/G_s = 345.27 - 7°27' -
\]

(7) — where we recall (§A1) that superscript \( s \) for solar anomalistic years, rounded in typical ancient fashion:

C10 If we had (in eq. 7) instead used the actual42 (unrounded) synodic month & anomalistic year, then (fn 38), the deduced remainder would have been \(-7°28'\).

C11 There is no difficulty at all in believing that an ancient scientist rounded either result (eq. 7 or §C10) to \(-7°27'\). (We see that eq. 1 implies a \( G_s \) value accurate to ordmag \( 10^4 \), almost as accurate as Aristarchos’ sidereal year43.)

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&8). The coincidence of [a] & [b] may not be proof, but it is attractive. [Note added 2018. Papyrus *P* *foud* 267A bears a solar-motion column consistent with yearlength 365.25 + 1\( \frac{1}{102} \) (23): Rawlins 2018U [K5].]

C13 And this adds more credibility to the position that the famous geocentric astrolabists Hipparcos & Ptolemy, drew much of their astronomy from often-unnamed44 but able45 astronomers (probably heliocentrist46) — not politically well-connected — whose

38  Prolemy did not recognize the precession of the solar apex. (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 found the tropical year (fn 43): fixing the anomalistic year (actual value, for Phil 1-epoch, by Newcomb’s solar theory: 365.25 + 1\( \frac{1}{102} \) would be tougher yet. [ii] The ancients knew the Moon’s anomalistic motion only to a precision of c.1°44 per 1000’ (even though the Moon’s orbital eccentricity was more than three Earth’s), but this error is approximately the size of the difference between the precessions of the solar apex & the stars. [iii] Rawlins 1991W SNS5 estimates an error of nearly 5° in 300 BC astronomers’ estimates 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 finding the anomalistic year would have been very much simpler. [c] There is no difficulty at all in believing that an ancient scientist rounded either result (eq. 7 or §C10) to \(-7°27'\). (We see that eq. 1 implies a \( G_s \) value accurate to ordmag \( 10^4 \), almost as accurate as Aristarchos’ sidereal year.)

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

40  Rawlins 1991W fn 1.

41  Kalippos (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 \( \frac{c}{2} = 60° \), consistent (idem) with Sample A of the Ancient Star Catalog. (Though, van der Waerden 1988 p.38 makes an intelligent, but incorrect orbit was pretty accurate (\( \frac{C2}{2} = 62.7° \), & DIO 1.5 15 fn 13); indeed, its error-wave-amplitude appears to have been less than that of any of Hipparcos’ three successive solar orbits (ibid ibid) G10, K10, & K9).


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

44  Specifically: Hipparcian 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.

45  Kalippos (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 \( \frac{c}{2} = 60° \), consistent (idem) with Sample A of the Ancient Star Catalog. (Though, van der Waerden 1988 p.38 makes an intelligent, but incorrect orbit was pretty accurate (\( \frac{C2}{2} = 62.7° \), & DIO 1.5 15 fn 13); indeed, its error-wave-amplitude appears to have been less than that of any of Hipparcos’ three successive solar orbits (ibid ibid) G10, K10, & K9).

46  See, e.g., DIO 4.1 p2 (News Note A). Not that DR’s tidy, entirely novel (physical) solution of the problem is likely to cause the stade-scrunching-for-Eratosthenes tribe’s incurreable passion for the uncertain 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 was 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 & Poseidonions values, 250,000 & 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 2.5 §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; [ii] the figures are self-consistent; [iii] it explains one of the figures (eqs.23&24. This reconstructed orbit was pretty accurate (\( \frac{C2}{2} = 62.7° \), & DIO 1.5 15 fn 13); indeed, its error-wave-amplitude appears to have been less than that of any of Hipparcos’ three successive solar orbits (ibid ibid) G10, K10, & K9).

47  Both magnitudes are DIO calculations, as are the following. The — 830/24 mid-eclipse was at 20:57 Babylon mean time (20:39 Babylon apparent time), at \( \lambda = 129° \& \beta = -1°, \) near 77° Leo. (DR’s calculations of eclipse times here & at, e.g., fn 64 are subject to c.1/4 hr uncertainty.) The — 140/27 mid-eclipse was at 21:58 Rhodes mean time (21:42 Rhodes apparent time), at \( \lambda = 125° \& \beta = +0°7, \) less than 3° east of Regulus. Both positions toponcentric and E&E= of date — both \( \lambda \) 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.

48  Actually, whereas the — 140 eclipse is only 1° from perigee (fn 65), the — 719 eclipse (which Almagest 6.9 says Hipparchos paired with the other for his 716B cycle: fn 52 & fn 59) was 14° short of apogee. Ptolemy correctly notes Almagest 6.9 the consequent serious effect upon the equation of center.
Moon's draconitic motion (eq. 19). Note: Ptolemy's idem criticism of this method is valid — his own Almajest 4.9 method is superior to Hipparchos' use of a 7160 u cycle. (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: §46) 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:

\[ V_{\text{H}} = 8534 \text{MA}/9146 = 251 \text{MA}/269 = 192123683^4 (6972480 = 27^2, 554569) \]  

(8)

which was correct to about 1 timesec! To be precise: the mean error (of eq. 8) during the centuries discussed in this paper was \(-1.3^\circ\pm0.1^\circ.\) (Understand: empirically determining anomalistic motion is an ordmug more difficult than determining synodic motion.) The eq. 8 anomalistic monthlength \( V_{\text{H}} \) is the basis of the (evidently Hipparchan) daily motion given at Almajest 4.3&4 (based on eqs. 2&3): 

\[ v_{\text{H}} = 360^\circ/V_{\text{H}} = (360^\circ/\text{MA})269/251 = 130^\circ35^\prime5^\prime\prime6^\prime\prime\prime38^\prime\prime\prime\prime38^\prime\prime\prime\prime (9) \]

And Ptolemy's Almajest 4.3&7 value (the basis of his Almajest 4.4 anomalistic motion tables) differs from eq. 9 by merely \(-12^\prime\prime\).
Had the eclipse of −149/1/7 been visible to Hipparchos, he could have paired it with that of −719/9/1-2 (Almajer 4.6), except that the later one occurred below his horizon.60

E3

Returning to the attested 3277\textdegree relation: if (analogously to \S5) we combine eq. 10 or eq. 11 with eq. 2, then we have:

\[ V_Y = 9831 M_A /10536 = 3277 M_A /3512 = 27.554600508 \] (13)

E4

The error in eq. 13 was \(+1.4\pm0.1\) — 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 1991 fn 1.) So the mean of eq. 3 & eq. 10 was just about right, and it is a credit\textsuperscript{61} to Ptolemy’s judgement\textsuperscript{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 anomalous month was almost exactly accurate: error ordmag 1\textsuperscript{\circloose} /100.

E5

Again (as with the 690 yr cycle noted at \S1), we find that this 795 yr cycle’s number of draconitic returns exceeds (eq. 11) a half-integral value by an amount (22\textsuperscript{\circloose}) which is just short of the outer limit (25\textsuperscript{\circloose}) 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 \S11.) 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 was: −830/2/4 & −36/12/7. The latter eclipse (−36) is just one 345\textdegree cycle after the −38/12/12 eclipse (which is attested at Almajer 4.11).\textsuperscript{64} and the former (−830) is the very eclipse we already suggested (at \S2) Hipparchos might have used for the 690 yr cycle.

E7

Note: the actual interval between the 2 eclipses of \S6 was 290315\textsuperscript{02}\textsuperscript{b} (5\textsuperscript{\circloose} shorter than 9831\textsuperscript{0} in eq. 11). Division by 10536\textsuperscript{(eq. 11)} produces \( V = 27.554583, \) ordmag

60 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 §11’s alternative 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., §D’s 716\textsuperscript{0} \( \approx \) 7770\textsuperscript{0} \( \approx \) 7675\textsuperscript{1/2} \( \approx \) 579\textsuperscript{0}, at Almajer 6.9) to find the empirical basis for eq. 19, so that (contrary to the suggestion of Rawlins 1985S) eq. 19 was not found from eclipses separated by 5458\textsuperscript{0} (eq. 18’s 2729\textsuperscript{0} or its triple [662 yrs], which has a better lunar anomalous return but a remainder of 40\textsuperscript{0}, nonetheless) but by continued-fractions analysis. (Eq. 19, Almajer 4.12, but not an eclipsi-pair 5458\textsuperscript{0} apart.) Note that if eq. 19 was derived from continued-fractions (and its prominence by Hipparchos’ era is likely related to mathematician-rediscovered investigations), then we will probably not be able to trace its ultimate empirical foundation (see Neugebauer 1975 p.160, partially cited here at the outset: fn 4) — especially if it is not built upon a specific period relation, as eqs. 11 \& 13 each were.

61 Aristyleus may have had the opportunity of discovering the 569 yr cycle from the eclipse pair: −831/9/9-10 \& −26/11/15/16 (interval 207954d16h). See also Ptolemy’s draconic reversion: fn 55.

62 For our similar 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 \& −257/9/30. (Neither of these two eclipses was visible in Europe or Babylon. Of this series, the first pair visible in Babylon was −1029/10/8 \& −234/10/11.) Note, however, that this series of 795 yr-cycle pairs was not the only one that ended eclipses in Hipparchos’ time. Pairs which ended other such series: −935/12/6 \& −140/12/7 (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 \( v = 113\textsuperscript{1/2} \& 112\textsuperscript{1/2} \), 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\textsuperscript{0} pairs are from saros-series whose Mees-Mucke numbers differ by 53.] If, despite its large solar-anomaly remainder (−65\textsuperscript{0}), 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 A = 74\textsuperscript{0}7.6 \& B = −0.9 (topocentric); its magnitude \( m = 6.9 \) (N limp.).
but in fact very few (about 1/12th) are in this “mean” range, most ss being nearer the extremes: about 70% are either between 7.88 centuries or between 9.10 centuries. 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. 1 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 near eclipse period-relations (eqs. 17&18) establish what I will call the “PBT”: (1) the Precessing ss-Bound anomalistic-Triangle, governing ss-starts&ends; and this triangle’s slow-motion precession in turn explains the long gap (in the occurrence of 795 yr-cycle eclipse-pairs) following the −36 eclipse.

F7 Two relations underlying the PBT are: the well-known76 29 yr cycle,

\[
358° = 388° 2/3 + 2° = 388° 2/3 + 29° - 20° = 1057° - 1°
\]

and the 221 yr cycle

\[
272°9 = 292° 4/3 + 13° = 296° 1° 1/2 = 221° - 131° = 8058°.
\]

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 1.16 (Heiberg 1907 pp.78-79):

\[
5458° = 5923°
\]

as a near-perfect synodic-draconitic return. (See also Neugebauer 1975 p.310.) And how well did the ancients do, when choosing eq. 19 (= 111719° = 441° 1 - 97°) 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.

77 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-ss-eclipse is 38 saros periods long.)

78 The 3rd sure post-Ptolemy 795 yr-cycle eclipse pair was −540/19/10-12 + 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 236/10/11-12 & 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.)

79 See §H4 & §J1.

80 Partial history at van den Bergh 1955 pp.22-23.
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°) — and these “points” will diffuse only ordmag 10° during that time. This leaves a lot of anomalous (space in the 360° possible values of anomaly) in which no ss-bound eclipse will occur for centuries on end. Since ss-bound anomaly is critical (§6) 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. 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 seen52 the latter member (both were part of the same ss, ending at −36/12/7), namely, −957/11/20-21 & −1629/22-23 and −921/12/13-14 & −1260/10/14-15. On the other hand: [a] No attested Hipparchos eclipse observation is part of a visible55 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 10th century BC material is unnecessary, since later Ptolemy (§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: 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 (2nd century BC) & Ptolemy (2nd century AD), a time whose high science has hitherto been a virtual blank56 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);57 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 whose name can for now only be guessed at. Possibilities include (Neugebauer 1975 p.575): [a] Aserion, who is the earliest figure cited (fn 32) as a compiler of tables for equation of time (which indicates the existence of sph

85 This c.120° has no relation to the well-known c.120° (due to the 13/3 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.

86 Ptolemy seems to be implying that spottier records existed before that time. And his Almajest 4.6 statement that the −720/119 era was “selected from the Babylonian records of that era” also implies more. (See fn 59.)

87 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 (ms = 1.7), the setting Moon’s altitude (above Babylon’s dawntilt horizon) was tiny at best.

88 Hipparchos’ −1401/127 eclipse is part of a 795 yr pair: it matches the −935/326 eclipse. But the −935 event was not visible at Babylon (or Europe).

89 Neugebauer 1957 p.55: “Early Greek astronomy from its beginnings about 400 B.C. [200 B.C. in 1st ed1] 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.

80 See Toomer 1984 p.1’s excellent edition of the invaluable Ptolemy king-list.

81 All modern calculations agree closely on the −830/2/4 eclipse’s magnitude: Oppolzer 1887, m = 0.4; Mees & Mucke 1992, m = 0.4; Liu & Fiala 1992, m = 0.5; DIO, m = 0.5.

82 And similarly, Almajest 6.6 of the superficially-plausible-but-unfortunately-false Ptolemy-Princetitude proposition53 [emphasis added]: “An interval of five synodic months is possible for...”

83 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 = 0.4 for the former.)

84 The −848/11/25-26 eclipse occurred well below the horizon at Babylon. Its m = 1.0 digits.

85 See Geminos 18.5 (Anajv ed., p.94), cited by Pedersen 1974 p.163 n.3.

86 My impression of Babylonian astronomical sophistication is consistent with either non-direct explanation.

87 Perhaps ultimately due to Hipparchos: Pliny 2.57. (One would suppose that the Princetitude could improve upon 2 millennia-stale math. But, then, see Rawlins 1987 n.30 on the Almajest 9.3-4 mean motions: DIO 4.3 151 §11.) If so, then neither he nor Ptolemy had checked the −830/2/4 (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 147.3, or less than 29.5/month, which is shorter than the average 29.5306 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 §13]. 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 9th 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 and both were visible in Babylon. So, this very rare short observable interval may 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 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.)

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] op5 to a 1000 year religious marriage ([H6]) to the ss?

Our final speculation is certainly the grandest — and (since it goes against my own historical expectations) the most enjoyable: long before the Seleucid era’s plague of astrologers (whose indoor-tablets so enthral modern historians), did early Babylonian lunar priests keep (now-lost)70 records of the eclipses of entire ss?

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

We may never know the truth. But merely savouring the possibilities is itself a pleasure. (See the beautiful and attractively overmodest conclusion100 of Neugebauer 1957.)

106 DR’s spotting of the −830/2/4 eclipse (at, e.g., Dio 2.3 p.90) occurred long before its 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).

107 The very occurrence of the −831/9/9-10 dawn eclipse (m = 0.6 digits by Oppolzer 1887, & 0.1 digits by Meers & 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.

108 See van der Waerden 1974 pp.115-120 for an argument favoring the short-term option. Further support here at fn. 30 and at §G2G3 & 115 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 cycle = 456.)

109 The lack of records is the most obvious conservative argument against DR’s §H6 speculation. So, in the absence of better clear evidence, we must here side with conservatism.

110 Despite Neugebauer’s intollerances, he had becoming a self-deprecatory side. (See Neugebauer 1975 pp.vii & 1.2. See also his final top protegé 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.” (Strangely, in 1975 p.177 (p.170 of the 1952 ed): “In the ‘Cholster’ of the Metropolitans’ annual of The New York [City] there hangs a magnific 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 mean much to me, but what have we 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 Museum Dance NYC 1993 p.350 attempts a hallucinatosion projection of the fading modern Freudian fascination upon medieval artists’ mentalities, in order to impute something saleably sacriligious to this innocent work.) A fine reproduction of this very tapestry-finale-image of the fenced (sitting) unicorn hung for many years in the apartment above my wife & I later (1960/6/11) honeymooned. 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)110 liked central in ancient astronomers’ secular reckonings, well before Ptolemy. 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)110

I The 800 Sidereal Year Eclipse Cycle & its Metonic Nest

I1 I have found that the smallest101 number of years in which eclipses will return to the same sidereal point (i.e., will occur at the same star) is 800, on the nose. In equation form, this neat circumstance may be expressed thusly:

\[ 9895^u = 800^u = 10738^w + 5^w = 292205^z/4 \]

101 DR’s quotation of the 830/2/4 eclipse (at, e.g., Dio 2.3, p.90) occurred long before its 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).

110 The lack of records is the most obvious conservative argument against DR’s §H6 speculation. So, in the absence of better clear evidence, we must here side with conservatism.

110 Despite Neugebauer’s intollerances, he had becoming a self-deprecatory side. (See Neugebauer 1975 pp.vii & 1.2. See also his final top protegé 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.” (Strangely, in 1975 p.177 (p.170 of the 1952 ed): “In the ‘Cholster’ of the Metropolitans’ annual of The New York [City] there hangs a magnific 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 mean much to me, but what have we 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 Museum Dance NYC 1993 p.350 attempts a hallucinatosion projection of the fading modern Freudian fascination upon medieval artists’ mentalities, in order to impute something saleably sacriligious to this innocent work.) A fine reproduction of this very tapestry-finale-image of the fenced (sitting) unicorn hung for many years in the apartment above my wife & I later (1960/6/11) honeymooned. 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)110

I1 I have found that the smallest101 number of years in which eclipses will return to the same sidereal point (i.e., will occur at the same star) is 800, on the nose. In equation form, this neat circumstance may be expressed thusly:

\[ 9895^u = 800^u = 10738^w + 5^w = 292205^z/4 \]
used the Heron 62/3/13-14 Alexandria midnight eclipse (Neugebauer 1975 p.846) with the −719/3/8-9 Babylon midnight eclipse (both a\textsuperscript{109} the star 49 Vir, \textsuperscript{108} whose latitude \( \beta = -3^\circ \), in order to found the equation:\textsuperscript{109}

\[
9660^y = 781^y - 10483^w - 2^s = 285265^13^b
\]  
(21)

\textbf{I6} Of course, it is possible that the relation was known even earlier, since \( 781^w \) pairs are common;\textsuperscript{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 following shcker, eq. 31: [a] Both eclipses are attested ([§34k15]. [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 & omag I' in rt.asc.), and the differential parallax was triling (fn 110). [d] The solar arc between mid-eclipses fell only 0.3^\circ short of precisely 281160\* or 781 sidereal revolutions.

\textbf{I7} Eq. 21 provides a value for the sidereal year \( Y_V \), implicit in PlanHyp, of very nearly:

\[
Y_V' = 9660MA/781 = 365^{1}/4 + 1/148
\]  
(22)

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

\[
Y_V'' = 36000Y_V'/35999 = 365^{1}/4 + 1/147
\]  
(23)

\textsuperscript{109} In both longitude & latitude \( \beta \) (whether topocentric or geocentric), both eclipses occurred nearer to the brighter star 519 Vir (than to 49 Vir) but I adopt here the irreissible (fn 108) identification by Ptolemy (or his source) of 49 Vir as the star that both eclipses occurred at. In right ascension \( \alpha \) there was in fact little to choose between the stars’ proximity to the eclipses. (The two stars’ \( \alpha \) were only 33' apart in 720 BC; 38' apart in 62 AD. And lunar \( \alpha \) parallax is null for an apparent-midnight eclipse. The −719/3/8-9 mid-eclipse’s \( \alpha \) virtually equaled 519 Vir’s \( \alpha \), while the 62/3/14 mid-eclipse’s \( \alpha \) was nearer 49 Vir’s.)

\textsuperscript{110} See fn 107. One must make explicit the implication here (on which a direct estimate of the sidereal year depends: fn 20. & [b] All Babylonians who observe preserve 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 \( \alpha \) in 20, which reveals that Ptolemy accepted that the −719/3/8-9 mid-eclipse’s \( \alpha \) was 4.06. He put 49 Vir there too: merely adding 8\*1/2 (the probable Ptolemy rounding of 8\*3.4 - see, e.g., Toomer 1984 p.452 n.69) for 310 precession gave 172 1/4, this star’s longitude at Almajest 7.5 — with a quarter-degree ending that is found (DIO 4.1 \( \alpha \) 11 fn 1) 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 \( \alpha \) in 20. (Ptolemy took the reported −719/3-8-9 conjunction as eclipsal, though the 1st century AD discoverer of eq. 21 evidently realized it was equatorial.)

\textsuperscript{111} Eq. 21 can be re-rendered: 781\* = 2826551/3\* = 9660\* = 365\* − 3\*, & eq. 20: 800\* = 2922051/5\* = 9859\* = 0\*. (Babylonian and eclipse 800\* apart are also common. Indeed, the 800\* 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 −830/24 eclipse (§22) presumably used for finding the 795 yr relation (eq. 19). Both the −830 & −30 eclipses occurred around 777 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: 9100' = 9849' + 14.5\*.) Eq. 21 (781 yrs) is poor in this regard (9660' = 10353' = 92\*), but it was used anyway — and to good effect, since the interval is so long that even a few fractions of anomalistic-nondecrement 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 fn 108.) If the data for [b] existed (fn 108), then the lunar anomaly — though useful for gauging synodic monthlength — was unnecessary for estimating the sidereal year. Moreover, there were other (adjacent) 781 yr epochs. 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 Persopolis & east thereof. The four pairs [intervals in brackets]: −720/3/19-20 & 61/3/24-25 [285265d23h], −720/9/11-12 & 61/9/17-18 [285265d20h], −719/5/8 & 62/3/13-14 [285265d10h], −719/1/2 & 62/9/7-8 [285265d20h]. Dividing just the 3rd interval by 781 would have produced a sidereal year of 365d14/1+163; but averaging the four years or the two extreme cases would have produced a mean interval of 2855265d12h. (Very near to 1479 = 285265d13h, eq. 21.) Dividing this by 781 produces a sidereal year of 365d14/1+149, which agrees closely both with the truth (365d14/1+154) and with eq. 22. (See fn 54.)
I12 Truncation\textsuperscript{115} after the 3\textsuperscript{rd} fractional term (the 2) will produce the famous\textsuperscript{116} 19\textsuperscript{th} Metonic\textsuperscript{117} cycle (a valuable artificial\textsuperscript{118} identity still used to compute the date of Easter):
\[ 19^\circ = 235^\circ \] (30)

I13 But, truncating after the next term (the 448) yields a far, far more precise expression:
\[ 852^3 = 105416^6 \] (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 2\textsuperscript{nd} century AD), at Plan-Hyp 1.1.6 (Heiberg 1907 pp.77-79 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 provides the ratio of the tropical yearlength to the synodic monthlength. (Another comment in passing: Muffiosi will reflexively attempt to ignore of\textsuperscript{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 1991W §§A7&B12.)

I15 Note also a few other related coincidences: [a] The 781\textsuperscript{st} interval is the only one of the six members of the 800\textsuperscript{th} Metonic nest (§33) 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\textsuperscript{121} to a (very unusually-rounded)\textsuperscript{122} conjunctive star in the Ptolemy catalog. [d] Finally, do not miss the provocative fact that the main two ancient cycles recovered\textsuperscript{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\textsuperscript{st} 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 Almagest 4.9, a report of this very ss’ next-last cycle, +1245/4, m = 1.8 digits.)

\begin{itemize}
\item \textsuperscript{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.)
\item \textsuperscript{116} See in 79 & van der Waarden 1974 pp.103-105, 246-247.
\item \textsuperscript{117} The 19\textsuperscript{th} cycle-calender’s inventor, Meton, was portrayed as a fake by his conservative contemporary, Aristophanes: The Birds kicks Meton off the stage, and The Clouds accuses new-fangled-calendar reformers (like Meton) of starving the gods by confusing them about the dates of their eats-festivals.
\item \textsuperscript{118} See fn 79 & Rawlins 1991W fn 1.
\item \textsuperscript{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 Menelaus are more likely figures. (Even more likely: Anonymous.) However, the precise expression eq. 31 may well be mathematician Ptolemy’s own creation.
\item \textsuperscript{120} Wide range of Muffia Sleights explored at Rawlins 1991W §H2 & DIO 2.3 §C.
\item \textsuperscript{121} At §I6 item [b].
\item \textsuperscript{122} See fn 108 & DIO 2.3 §8 fn 20.
\item \textsuperscript{123} Neither of these two cycles (781 yrs & 795 yrs) is recognized in van den Bergh 1955 (nor is that of fn 30).
\end{itemize}

\begin{itemize}
\item \textsuperscript{125} Muffia slander against important contributions to the very field (late 2 \textsuperscript{nd} century AD) at Plan-Hyp 1.1.6 (Heiberg 1907 pp.77-79 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 provides the ratio of the tropical yearlength to the synodic monthlength. (Another comment in passing: Muffiosi will reflexively attempt to ignore of\textsuperscript{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 1991W §§A7&B12.)
\item \textsuperscript{126} Note also a few other related coincidences: [a] The 781\textsuperscript{st} interval is the only one of the six members of the 800\textsuperscript{th} Metonic nest (§33) 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\textsuperscript{121} to a (very unusually-rounded)\textsuperscript{122} conjunctive star in the Ptolemy catalog. [d] Finally, do not miss the provocative fact that the main two ancient cycles recovered\textsuperscript{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\textsuperscript{st} 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 Almagest 4.9, a report of this very ss’ next-last cycle, +1245/4, m = 1.8 digits.)
\item \textsuperscript{127} Neither of these two cycles (781 yrs & 795 yrs) is recognized in van den Bergh 1955 (nor is that of fn 30).
\item \textsuperscript{128} Indeed, both cycles have apparently lain unknown for the nearly two millennia from antiquity to the present paper.
\end{itemize}
K How Topoche 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-Muffio scholarship can get. For a lesson from The Experts (who regularly declare that those who disagree with them are incompetents: §BS & DIO 2.3 §6 [SE2]), we turn to the paper of Muffio genii B.Goldstein (Muffia cappo) & A.Bowen (Inst Res Class Philos & Sci, Princeton - publisher of Princettito-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’ cycle 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 & Hipparchos’ 600-Year Cycle”. Since Muffiosi regard Hipparchos as “virtually a clozet Babylonian in Greek drag” (Rawlins 1991W §E1, the 600’ period must (fn 27) have a Babylonian-[130 origin. (Origin has adding needless-to-add been so: needless.) Thus, borrowing of N.Swerdlow, G&B start with the well-known Babylonian version of our eq. 30, namely:

\[ 1° = 12^{22.06'}/13 \]  

(32)

G&B then round this to the unattested expression

\[ 1° = 12^{22.06'} = 7421'\overline{4}/600 \]  

(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 unheared-of “cycle”:

\[ 600' = 7421' \]  

(34)

K3 The trifling inconvenience that 7421’ 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’ an anomalous cycle. In fact, 7421’ doesn’t equal anything recognizable, other than roughly 600 — 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’ cycle, eq. 20. (See fn 109.) That is, 800’ = 9895° — 0°.4; by pathetic contrast, 600’ = 7421° + 90° and 600’ = 7421° − 13°. As for draconic commensurabilities: 9895° = 10738° + 5°, but 7421° = 8053° + 86°.

128 Bowen 1995 takes it for granted that Geminus & Pliny (emb add): “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 [JE] of the Muffia cult? — lacking the very “independent confirmation deriving from the times in question” which one review (Bowen 1995) requires of non-Muffiosi. 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’ figure the sort of “rigorous” attestation which lesser scholars lack! — even though the 600’ 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 precocious, which so efficiently demonstrates (better than our own JHA could) the risible inductivity-stertor of G&B’s much-touted historical method. Incidentally, assuming that the catfght I witnessed at the end of the 1994/5/8 Dibner Inst conference was real, it would seem that this avenue of research is more promising than others considered too extreme even 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.)

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 commensurabilities (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 precocious, which so efficiently demonstrates (better than our own JHA could) the risible inductivity-stertor of G&B’s much-touted historical method. Incidentally, assuming that the catfght I witnessed at the end of the 1994/5/8 Dibner Inst conference was real, it would seem that this avenue of research is more promising than others considered too extreme even 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.)

133 No explicitly Hellenistic eclipse observation is extant earlier than the time of Kallippos: the 25-yr limit of pair-possibility, 795 yr-cycle eclipse-pairs tend to occur near perigee and are impossible near apogee. 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: [FSL!]

134 However, it is possible that whatever old Babylonian data still survived were (by Ptolemy’s time) merely hamp-mered down (1/LA), effectively selected preferred centuries considered too extreme even 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.)

135 No explicitly Hellenistic eclipse observation is extant earlier than the time of Kallippos: the –3309/20/21 (Arbela observed by Alexander’s army) reported at, e.g., Pliny 2.180 & GD 1.4.2. However, Meton’s lunisolar 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, & Princetitute 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&Gingerich) who impute serious sophistica¬tion136 to late Babylonian astronomy, and who have thus for decades intermittently hoped136 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 timespans since the end of Babylon!), not one Babylonian astronomical parameter or ephememeris (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 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 enliven briefs for sacred Muffia viewpoints: e.g., §B5 & §J2.) [b] Babylonian-obsessed Muffiosis’s uniform unwillingness (even while futilely conjuring up vapid & vapid speculations of Babylonian empiricism), to acknowledge the simplest evidence of §M3 (fn 137), betrays such truly pathetic intellectual-shock paral¬ysis136 (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 most fossilized members of this strange cult be relieved of the power to determine140 who does and who doesn’t get funding141 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&such 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 idee-fixe kookery, while bluntly going for suppression of legitimate & highly-recommended scholarship?

References

David Hicks 1994. DIO 4.1 11.
Thos.Heath 1913. Aristarchus of Samos, Oxford U.
Franz Kugler 1900. Babylonische Mondrechnung, Freiburg im Breisgau.
D.Menzel & O.Gingerich 1962, Pref. to Oppolzer 1887, NYC (Dover). C.Müller 1883&1901.
M2 Indeed, to 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 enliven briefs for sacred Muffia viewpoints: e.g., §B5 & §J2.) [b] Babylonian-obsessed Muffiosis’s uniform unwillingness (even while futilely conjuring up vapid speculations of Babylonian empiricism), to acknowledge the simplest evidence of §M3 (fn 137), betrays such truly pathetic intellectual-shock paralysis (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 most fossilized members of this strange cult be relieved of the power to determine who does and who doesn’t get funding 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&such 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 idee-fixe kookery, while bluntly going for suppression of legitimate & highly-recommended scholarship?