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Free spirits will presumably be pleased (and certain archons will not be surprised) to learn that: at DIO, there is not the slightest fixed standard for writing style.

Potential contributors: send to the above address a spare photocopy of material (not to be returned) and phone DIO about 3 weeks later.

Each issue of DIO will be printed on paper which is certified acid-free. The ink isn’t.
Note added 2013. In 1996, the article commencing on the opposite page conspicuously launched a test of Muffia-enforced omerta, by presentation of its PRECISE Equation 31 solution to Ptolemy’s final astronomical relation. As of 17 years later (2013): Congratulations! Unsurprisingly, the entire history-of-astronomy rabbitariate has passed our test, by non-citation of such blemishlessness as to be the envy of less fear-ridden churches, additionally betraying years of failure of sycophants’ searches (www.dioi.org/prt.htm#eqs) for the alternate-solution grail that never materialized, frustration which we gratefully acknowledge could not more highly honor the thereby-reconfirmed validity of Equation 31.

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Mea Bloopa:

Back-issue DIO reprints (with stiff-covers) are incorporating the below-cited corrections and others (e.g., the list at DIO 2.2 p.54).

A. My wife Barbara (whose astonishing associative memory is legendary among us) caught a stupid DIO misattribution: the citation to Noel Coward, at DIO 2.1 §1 In 6, should instead be to Emry Iiggins (My Fair Lady). (Typically, B knew this without even checking.)

B. While usefully revealing how easy it is to observe Poisson Spots (the most convenient means of proving the wave theory of light to anyone: no lab equipment required), DIO 4.2 §9 To carelessly stated in many copies mailed out that the opaque specks (whose shadows one is observing) are on the cornea, rather than (Jearl Walker Sci.Am 246.4:150; 1982/4) intra-eyeball. This dumb error was snuffed partway during printing (1994/12/30-31); and the DIOs going to libraries at this time all received the corrected edition.1

1 At the time of her receipt (1995/5/8-7) of artist Natasha Mathias’ irresistible dinosaur-mobile, B expressed disapproval of our reference to Muffia-as-diner, at DIO 1.1 §5 fn 11 [see also DIO 4.1 §3 fn 3], which noted that, while R.Newton often admires the work of those he criticizes, typical Muffia output approaches R.Newton “as fundamentalists approach Darwin: the slightest slip is leapt upon, with tyrannosaurus gentility, as hard-proof that a hated general theory is entirely false and abhorrent.” Except for an obvious vulture analogy (DIO 1.2 fn 127), B feels this is unfair to ty-rx, who’s a cuddly puppy next to the flesh-eaters of modern academe. After all, there is no record of friendly as oth. than homo superbus contumax, which consciously aimed (DIO 4.3 §15 §13) at the EXTINCTION of a fellow species.

2 [Before DIO 6’s shipment to the great majority of our library subscribers, the 1997 printing has adopted slight improvements to the calculated ancient eclipses cited in §1. Though the several corrections entail less than the data’s empirical uncertainty and are far too small to affect the paper’s inductions, it seemed right to print a notice nonetheless.]

3 Since were-dogs don’t know right from wrong, punishing them would be senseless vengeance (DIO 4.2 §19 §2). OS matches the great SatNightLive civil rights pioneer, Emily Litella, in standing 4square against wool-abuse.

4 Just an upside-down version of the truth (first widely revealed by Trevor Hall) of Nobel chemist Wm.Crookes’ psychic “research” on his secret young lover Florence Cook.

5 Astrologists will note that the murders occurred not at lunisolar opposition (Full Moon) but at 90° and at 90° — and the verdict at time.

6 I hate to interrupt this romp with something true, but it is a fact that I have bumped into both OS and Kato quite by chance. I encountered a genial OS in LAX airport c.1980, where he was bobbing & weaving while dribbling an invisible basketball. (No one ever looked less like a murderer.) On 1995/6/4, at Camden Yards, my nephew John Charles Aviret and I spotted Kato nearby, settling into the best seat in the stadium. (Accompanied by a local 98 Rock deejay, whose hair had shared a bleach-windtunnel with Kato’s.) I went over to shake paws, and we chatted pleasantly for a minute. (An extremely likable fellow. Who will not resent DR pulling his leg & wagging his tale.)

7 In fact, the Were-Dog Hypothesis is more consistent than Johnny C’s shaky theories, which of course never did explain the cuts on OS’ hand as anything more than a spectacularly convenient coincidence.

B Simpson as Near-Saviour: a Tragically-Wronged Hero

B1 So the truth is that, on the night of 1994/6/12, Kato — moonsruck in his own special way (more on that below: §B6) — came upon OS, Nicole, & Ron, at a tender moment, right as OS was amiably congratulating his former #1 punching-bag for now spending his hard-earned millions while she slept with men other than himself. In the midst of this loving familial scene, Kato (known to be a tail-wagging stalker of humans — a regular shadow) suddenly leapt upon the threesome, swiftly slashing the throats of the frailer Nicole & Ron — while powerful OS got his left hand cut while trying heroically but vainly to defend them. (OS at last succeeded in chasing Kato away before a feeding further desecrated the bodies. Little wonder OS is upset at the public’s ungrateful misunderstanding of his efforts.)

B2 Wakeup-question: why is the dog called Kato? Have you ever seen the dog and the Kato together that ferally-smiling OS-houseguest Kato (whose hairdo is a hairdon’t) is THE shaggiest of as shaggy as this story.

B3 Like were-wolves, were-dogs kill by fanging throats. (And where were both fatal wounds delivered? Think about it. Beginning to understand why no weapon was found?)

B4 So, DIO now scoops the truth behind the headlines: Saint OS is covering for someone else (just like F.Lea’s other too-kind client: §A4), namely, his wuvable woofwoof pal Kato — who has a pathetic Special-Problem. (Were-dogs just can’t help themselves.)

B5 Indeed, scientists OS & Nicole had for years been privately yearning to land a Nobel by becoming the first social workers ever to cure a were-dog. The project’s secrecy necessitated their ‘til-now-inexplicable eternal-houseguest cover-story” — transparently ridiculous, of course. But effective: no one has previously discovered its true significance.

B6 Since Kato is still on the loose, we at DIO believe the public deserves to be fully informed of the differences between were-wolves & were-dogs:

[a] A were-dog’s alter-animal is pooch, instead of wolf, which makes the were-dog all the more dangerous, because it seems as friendly as — well, as friendly as OSimpson.

[b] The were-dog kills not at Full Moon but at MoonSet. (The were-wolf’s smarter cousin prefers to kill in full darkness: night MoonSet being the very start of same, this leaves plenty of time for the business at hand. I.e., he doesn’t like to rush a meal.)
5 Hero & Doppelfanger: A Shaggy Were-Dog Story

Written 1995/10/5. [If you haven’t kept abreast of the OSimpson drama, then the following may make no sense. Likewise if you have.]

A What the Los Angeles Police Dep’t Didn’t Tell the Public

A1 As noted at §4 §C8, millions of OSimpson–trial-junkies take an ingenious position (which superficial minds too-quickly pass off as fantastic): [a] OS is innocent, and [b] his blood was at the murder scene without police planting. The following development is dedicated to these detectives, who will go otherwise unhelmed in the standard media.

A2 Conventional analysts have needlessly made the OS case much too complicated. Simple question: who was the only personality known to be at the scene, & wet with the victims’ fresh blood? Why was he busy howling at the setting Moon? Only someone almost as racist as certain celeb-athletes’ preferences in women, could suspect virtually-bloodless OS, when Kato the (strange-looking Akita-breed) dog was caught red-pawed, at the bodies.

A3 And, you want to talk LAPD cover-ups? According to a well-fueled Hungarian housekeeper (suspiciously vanished since), on a cop on the scene said — and I quote verbatim — he was certain that this was one very “weird dog”. As any student of the relevant Balkan lore is well aware, “were-dog” is awed-religious-fool’s Carpathian-dialect for “were-wolf” — first cousin to the better-known but equally-dreaded “were-wolf”, which is also now found mainly in suburban L.A.— on the devout film-lots of “Wholly-Weird”.1

A4 The case will now solve itself, once we add-in some background: [a] OS’s lawyer1 first entered bigtime celebhood (this was even before he became a feminist publisher) when he got murdered2 Sam Sheppard off by inducing Sam to suddenly recall3 that his initial story — he was altruisically holding back relevant information in order to protect another party. [b] OS, too, is a very selfless guy.

1 One is tempted to suppose that the werewolf is a West Hollywood creature, since there has only rarely been a female film-werewolf. (Though, see J.G.Melton Vampyre Book [speaking of doppelfangers] Detroit 1994 p.35.) Note that what’s fatal to the werewolf, a silver bullet, long had a strictly male connotation. (Until Coors recently found it could get both sexes fat&drunk on its formerly-male-appeal Silver Bullet beer.) But this apparent gender-bias may instead be merely an unintended side-effect of the US’ lucrative depillatory industry’s sexist veto-power over female imagery in films: after all, how saleably-charismatic would the public find a film werewolfess (Mad Magazine, are you ready for this?) — entirely covered with hair, excepting legs and armpits?

2 F.Lee Bailey (The Defense Never Rests 1971 pt.1 ch.5, re yet another homicidal chap he got off): “Would I defend a guilty man? . . . the question whether [a killer] should have been defended in every possible way is not personal or subjective. It is professional and legal. And any lawyer worth his license would answer it the same way.”

3 In the early 1970s, F.Lee Bailey launched a Playboy clone punningishly called Gallery.4

4 Popular history (controlled largely by F.LeeBee himself) has exonerated Sheppard, who was formerly the prominent police sewage of Bay Village (Cleveland OH suburb). His case has similarities to OS’ (even beyond FLB’s involvement): rich client, “whopping” legal fees (FLB’s 1971 pt.2 ch.2), fast marriage (Sheppard-divorce 1st discussed only 3 weeks before murder: ibid pt.2 ch.1), suspect’s indelity, no time-alibi when wife killed, didn’t testify before jury that released him, “real killer” never apprehended, police-compotence/integrity put on ferocious trial. Sheppard’s story: he saw an unrecognizable assailant (of indeterminate gender?) who got into his bedroom and killed his wife but merely injured Sam. (Jeff McDonald wife-murder: close copy. Chas. Stuart similar.) Curious “real” murderer: killed wife but permitted Sam to live because he knew he had hit Sam just hard enough to destroy the precision of Sam’s memory (of someone of allegedly “white form” or in “white garment”: idem).

5 See S.Sheppard Endure & Conquer 1966 pp.299-300, 310-311, 318. He promised (FLB 1971 pt.2 ch.2) to work for 10$ after release, to pay off FLB’s fees but (after converting from book-author to pro wrestler) he escaped by dying of alcohol & pills at age 46, only 3 years of “freedom”.

1 §1 Testing Princetitute-Muffia Omertà

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! MuffieMyth MirthBalls

by Rawlins

Princeton-Institute-Muffia\textsuperscript{1} History-of-Science Wisdom:

The conclusion\textsuperscript{1} of the Muffia’s late don-of-dons, Otto Neugebauer of the Princeton Institute for Advanced Study,\textsuperscript{2} 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-JHA-followers, looking for the dependably entertaining math-antics of their favorite modern imps, will not be disappointed by the Muffa-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: Muffienythmirthballth . . . )

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 §20 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 supernerdludes, was introduced to DIO readers at DIO 1.1 §1 | §C5, C7, & C12. See fn 5. Also “Black Alfdavit” at DIO 1.1 §10. And “Casting Pearls Before Pyglets” at DIO 4.1 §4.


5 Due to its long association with O.Neugebauer, the Princeton Institute continues, uncomplainingly & unqualifi- edly, to confer prestige and funding upon the skewed-and-or outright-censorial output of the nest of Neugebauer- clones whom DIO has reverently dubbed: the Muffia. See, e.g., fn 139, §M5, and Rawlins 1991W fn 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,7 typically-suppressive 1993 pre-publication advice from the unfalsifiable Muff-
ia, warning against a Cambridge University-trained mathematician’s repeated citation (in
a 1944 Springer book) of DR’s finding of the first evidence of Greek influence upon Babyl-
onian astronomy (discovery now widely-accepted,8 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 another9 fist-sized hole into the Muffia’s
ever-straining omertà-dike, the shoring-up of which continues to squander Hist.sci credi-
ocability & 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
dirty-tricks10 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 Mathe-
ematics 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.

19 A previous sample (early 1995) found that the actual fraction of blacks who thought OS was guilty was 8%. I
see no evidence that this fraction is less accurate than ABC-TV’s ‘news’ reported 18%. In short, the racial split on
the verdict may be even wider than now given out.

20 And 83% of blacks agreed with the verdict. Since the sum is 101%, evidently 1% thought he was guilty but
weren’t sure enough to vote conviction. The numbers for whites: 70% guilty, 37% agreed with verdict. The one
white juror just-so-happened to be in the small 7% not-sure-enough subsample. Or felt isolation-intimidated.

21 Face the Nation (1995/10/8). (Empth entirely hers.) Similarly schiz editorial by MS Editor-in-Chief M.Gillespie

22 [This reasoning could (just as fallaciously) alibi the occasional white juror that (for racist instead of evidential
reasons) unjustly convicts a black for a mugger he did not commit. Would the US media broadcast, nationally
(without obligatory immediate-followup pundit-horror-at-crimethought), cultist suggestions that such an injustice
should stand, simply because the white juror was basing its verdict upon a legitimate-experience of disproportionately
high black-on-white (street)crime rates?]


24 See the astute comments of Jos. Agassi (Centaurus 37:4-349 [1994]) & Paul Feyerabend (Studies Philos & Hist

a genuine hero, Ron Goldman.) I criticize Liberals for projecting their mentalities onto others. But, in this case, I (more than once) did the very same thing. DR’s most persistent optimism to keep looking (Diogenes-like) for an exception to my own cynicism — because that is what I myself strive to be.

C OS Doubletakes

C1 Well, that’s a comfort: When OS friend M.Slotkin was asked what effect an acquittal would have on OS, the reply was, “I don’t think he’s going to change much . . .”

C2 Jo-Ellan Dimitrius, OS’ charming-fox hiring-jury-slanter, slipped & told a truth: “I’m a true believer in the jury system in the United States.”

C3 It was obvious from the outset that the two OS murders would lead to a total incarceration of at least ten years. For the jury.

C4 Even while US establishment-volk ritualistically condemn racism, they simultaneously reacted (privately) to the OS case with [i] a judgement & [ii] a prediction. These are worth placing side-by-side: [i] Only an idiot can believe OS is innocent. [ii] A lot of the [mostly black] jury will vote OS innocent.

C5 But the worst (& most doubletake-worthy) prediction was by DR, who — neglecting Mencken’s dictum [no one ever lost a buck understimating the US public’s intelligence] — ripply supposed (DIO 4.2 §9 [F3]) that even a cheap idiot-prosecutor couldn’t lose all 12 jurors. (The suggestion’s only plus: same verdict for less public cost.)

C6 The actual outcome spat in the face of public trust in decency and intelligent democracy. It is a victory for truth-manipulating lawyers, KKK predictions, & cynics everywhere — and for the repeatedly vindicated theory that (as with our elections): when millions of dollars are on the loose, under-the-table, sometings will always go wrong with a rationally-intended process.

14 E.g., fn 26. The irony is that the admirably decent Liberal publisher Chas. Peters was all along far wiser than DR on the trial’s outcome. E.g., he led off the 1964 issue of his Wash Monthly (over a year before the verdict) with a note he’d received from L.A., describing its writer’s black friend (caps in org): “This guy is middle class or better, definitely a solid citizen. He told me that he knew O.J. was guilty — just by the way he looked in court when they showed the pictures of the bodies, and by the fact that he took him a month to mount a campaign to hunt for the killers of his children’s mother — but that even so . . . that he would NOT GUilty. When I asked why, he replied, ‘If it was good enough for the Menendez brothers, it’s good enough for [Simpson]. And besides, I know the LAPD isn’t above framing a suspect.’ [DR: note that this last point’s relevance to the OS case is contradicted by his previous (cop-independent) reasons for realizing OS’ guilt: ‘I can’t stress enough that this was a very level-headed guy saying this. The prosecution is doomed, I think.” On 1995/10/13, my black (defense lawyer) neighbor said she was delighted at the OS verdict regardless of his guilt, because OS' conviction would have enforced the “stereotype” of blacks as criminals. (If a white mob cheered the release of a white killer for similar reasons, this would not be the case.)

15 E.g., I believe that there are Mufosis who are quietly embarrassed by their own cult’s misbehavior (11 fn 3&5), but I expect no public confirmation of that DR optimism.

16 Dimitrius used potential jurors’ handwriting (Newswear 1995/10/30 p.84) when selecting acquitters. (One can see where examining handwriting might be useful to someone looking for low-education jurors.) Was it accidental that the jury ended up containing virtually no one who’d been to college? (Is popular promotion of “graphology” going to serve as a cover while wealthy criminals’ jury-lurkers systematically dummyfy US juries?)

17 Total including rejects & alternates.

18 But the Clark-Darden experiment was better: mass-racist black-cheering of a wifebeating killer’s release was an invaluable wake-up-shock to even the densest traditional civil-rightsful Lib. It may assume the same rˆole which N.Chamberlin’s 1938 peace-in-our-time naivete served, for 1938-1940 observers (who might have blamed England for war, had hostilities started in 1938): when give-em-enough-ropes mentality goes far beyond rationality, it can ultimately help enlighten the very slowest learners.

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 cycle11 (Almajest 4.2):

\[
4267\cdot y_{m} = 4573\cdot y_{m} - 71.75/12 = 4630\cdot y_{m}/12 + 11\cdot y_{m} = 126007^{01b}
\]  \hspace{1cm} (1)

(Apologies here & below: d = days, h = hours, m = timeminutes. Lunar: u = synodic — Hippantly supposed (x5) that even a cheap idiot-prosecutor couldn’t lose all 12 jurors. (The suggestion’s only plus: same verdict for less public cost.)  But the Clark-Darden experiment was better: mass-racist black-cheering of a wifebeating killer’s release was an invaluable wake-up-shock to even the densest traditional civil-rightsful Lib. It may assume the same rˆole which N.Chamberlin’s 1938 peace-in-our-time naivete served, for 1938-1940 observers (who might have blamed England for war, had hostilities started in 1938): when give-em-enough-ropes mentality goes far beyond rationality, it can ultimately help enlighten the very slowest learners.

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

\[
M_{A} = 29^{d}31^{m}50^{s}08^{"}20^{"'} = 29^{d}53059413580
\]  \hspace{1cm} (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 into 126007^{01b} 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.)

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/ seconds or better. The Babylonian part of the empirical base of this Greek math is shown (eq. 11 & §6) 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.

11 In this paper, I use the word “saros” to signify the interval of eq. 14; and “saros-ss”, 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 equal-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, 20031507th is found by multiplying 1 = 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 — 65360). Other real lunar periods for Phil (1 — 323/11/12): anom mO = 27d.554584, draconic mO = 27d.212222, sidereal mO = 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 (J2) or the Metonically-defined “tropical” year (eq. 30: 1 = 323/11/12): anom mo = 333/11/12: dracon mo = 333/11/12: sid mo = 333/11/12. In (these equations [also in, e.g., eqs. 22&23], equality is not meant to be exact: it holds only to the precision displayed.)

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 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 −6204/22 and −2754/18 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. In 1991, 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 equation:

\[ 251^u = 269^u \]

B His Lardship 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.10

B2 At Yale, length University’s A.Aaboee 1955 (pp.123-124) made the clever11 discovery that the −7°1/2 remainder in eq. 1 could almost be explained by assuming use of the

---

15 See fn 137. It is an indication of how highly Hipparchos was regarded in antiquity that his −134 S.Solstice became 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].

17 The −6204/22 eclipse is one of only four pre-600 BC eclipses preserved by Ptolemy (Almajest 5.14). The middle of the −2754/18 total eclipse was below the horizon at Alexandria, but the eclipse started about 18° above the horizon, at AAM Local Mean Time. Just adding a rough total eclipse semi-duration of 110° or 120° would provide a time interval of mid-eclipse to within about 10°. (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: −4904/25-26 (Almajest 4.9) & −1454/21-22 (Almajest 3.1). Though eq. 3 may already have been known, the discovery of eq. 1’s 345 yr interval would show its superiority vs. other multiples (fn 19) of eq. 3.

19 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 to that of my long discouragement at black culture’s stereotype-breakers: “other side” in the OS case. So, the media regularly paraded before the public a succession of soap-saga: Tonya–Nancy — to spur ratings with sportsbiz-style gotta-watch cliffhangerhood.

20 Even the Mufa’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].

21 At least since Manitius 1912-3 1:196 note b. Taking Phil 1 as epoch: for 4267 real synodic months, eq. 1’s mean sidereal period is 345.017 yrs. This is quicker than Ptolemy’s 365d1/4 = 365.2425. This is quicker than Ptolemy’s Almajest 3.4 math, & the results are identical. (See also fn 44.)
converges on him, but: the very instant he puts a gun to his head, the genius L.A. cops
fundamental Babylonian ratio of yearlength\textsuperscript{22} to monthlength:

\[ 1^\circ = 12^\circ 22'08'' \]  

(4)

Almost.

B3. The charmingly plausible Aaboe, an enormous mathematical talent, has since be-

come the venerable loving-papa, guru-mentor, & shepherd of current lamb-brained Muf-

fiadum; an above-if-all, Primarily architect of its public-noncitizen-cum-private-slave strategy\textsuperscript{24} toward heretics. For openmindedness on central sacred tenets, Aaboe can match the

real pope.

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

\[ 4267''/(12^\circ 22'08''/1') = 343^4558'42'' \ldots = 345^9^0'01'17'' \ldots = 345^9-7^0'42'' \]  

and he then rounded the remainder to the nearest 1\(^\circ\)/2 to find \(-7^\circ1/2\). 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-protége N.Swerdlov’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 outrageous at nonculutches’ alleged academic dishonesty!) Why do Muffios have such a magic-touch pen\textsuperscript{26} for this sort of thing? (Note that MacArthur Fellow [see DIO 4.3 [15 fn 24]] Swerdlov’s most polished forced-math gem, cited at DIO 1.1 §f 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\textsuperscript{27} in Greek astronomy as their chief hero Pholomery was determined to discern his own theories embedded in recalcitrant reality — so, when the need arises, they will resort to the same math methods:


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 Klausowitz to anticipate that academic’s vaunted archonts 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-education. So, unsurprisingly, the banned parties occasionally react anew in extra-rational avenues. E.g., DIO 4.3 [12 fn 21]...)  

25 For catalogs of other entertaining instances of Muffa (& Princeton Inst) fudge, see “Black Afdavit” at DIO 4.4 (\textsuperscript{4}“Muffa 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 Afdavit” at DIO 1.3 §10, and “Casting Pearls Before Pgylets” at DIO 4.14. (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.) Mean-
time, 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 232) to reveal the purportedly enormous List-Here-in-My-Hand of actual heretics’ errors, Muffos 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 Geminus 18.9. See also Neugebauer 1975 p.601. But all of this is clearly cited. So, why do Muffios assume that large unattributed borrowings went on? For the hyperwispy, sometimes misconceived nature of the few alleged Muffia proofs of said borrowings, see, e.g., \$B5 and Rawlins 1991W §[02-09] \& fn 73, and below here at \$K. (Note contrast to DB’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. (Princeton-funded Britton 1992 repeatedly lauds the Mufa’s: [a] attack on RN’s honesty28 [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 (1976/39) called R.Newton’s research: “incompetent work in my realm”. (See DIO 4.3 §15 §G9.) For the Mufa’s own dazzling competence here (which accounts for this paper’s J.Hyst.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°).25

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 fn 198&57.)26

C2 It seems that the solar apogee A was placed near 60° (fn 44) by Kalippicos (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 excellent1 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.1′ century BC). (The author2 & 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.}

24 Eg., Rawlins 1991W fn 252 & DIO 1.1 §3 (D3).
25 Neugebauer 1975 p.312 sees solake 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 1′s −7°1/2 remainder could be Greek.
26 Best ordmag-1000′ syndodic-anomalistic cycle: 16092° = 17246′ = 17463° − 9° = 1301° + 3° = 47520608h (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 [H]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 315′ pair-intervals would [in 18] produce comparable accuracy.)
27 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.)
continuous-function solutions fit all the 3 trios of Almajest data under discussion in the JHA paper and (§D5) that all 3 of these orbit-solutions are derived & presented in DIO 1.2-3.

Adding to these credits, the Journal for-the-History-of-Astronomy has also refused even to inform its readership of the existence of DIO 3’s 1993 Tycho star catalog, published by DR, with whom Hoskin still insists on total noncommunication — a unilateral breach that persists (it’s been over 12 years) only & entirely because Hoskin is afraid that ending it under criticism might create a Wicked-Witch-style meltdown of a long-nurtured image of: Bigness. He remains sadly short of understanding the deeper meaning of the world.

It’s inspirational to realize that (contra the naïve mild optimism of §A5), despite the daunting challenges presented by the JHA’s ghastly “Muffia Orbatuary” (DIO 1.2-3) episode, nonetheless, that curious journal’s degree of devotion to integrity, impersonal equity, and astronomical history has overcome all — and thus (§E3) miraculously emerged essentially intact.

I Postscript C: Priorities & Sentences

 Hoskin’s last letter (1983/3/3) before condemning DR (3/21) to an indeterminate sentence of exile:

... I devoutly hope that in future you will honour other editors with your contributions. Your undoubted talents are bought at too high a price.

I urge that Hoskin issue a public (not [typically] behind-the-back, thus uncheckable) explanation of the mysterious priorities underlying that revealing final sentence.

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 geocentrist’s large (conveniently) Sun-shrinking: Rawlins 1991W fn 280 & §R14 parallel-guettestimates would degrade the accuracy of apogee-determination, it is reasonable to ascribe to ancient heliocentrist the here-proposed discovery of correct solar apogee-precession.

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°2114°) parallax-guesstimates would degrade the accuracy of apogee-determination, it is reasonable to ascribe to ancient heliocentrist the here-proposed discovery of correct solar apogee-precession. [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) 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.

Almajest 4.2 treats the −7°1/2 remainder in eq. 1 as if it is longitudinal; not only longitudinally 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.

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., 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.

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.)

We do not know. 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 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?

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

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

35 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 says 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, 365d2.2964134 (≈ 365d1/4 + 1/100), decimally to 365d.2596 & then expressing it sexagesimally as 365d15°34′33″36′″ = 1,000 times more precise than accurate. Same muffs (iden) 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.]

36 See fn 36, Neugebauer 1975 p.298, & DIO 3 fn 29.
Thus, let us test quantitatively the hypothesis\textsuperscript{38} that the ancients knew\textsuperscript{39} the Sun’s anomalistic motion; we start by proposing\textsuperscript{40} an accurate value $G_s$ for the solar anomalistic year, rounded in typical ancient\textsuperscript{41} fashion:

$$G_s = 365\frac{5}{4} + 1/100$$

and divide it into eq. 1:

$$4267MA/G_s = 345\frac{5}{7} - 7\frac{3}{4} - \ldots$$

— where we recall (§A1) that superscript $g$ = solar anomalistic years.\hfill C10

If we had (in eq. 7) instead used the actual\textsuperscript{42} (unrounded) synodic month & anomalistic year, then (fn 38), the deduced remainder would have been $-7\frac{28}{3}$.\hfill C11

Difficulty in believing that an ancient scientist rounded either result (eq. 7 or §C10) to $-7\frac{1}{2}$. (We see that eq. 1 implies a $G_s$ value accurate to orbitmag 10\textsuperscript{4}, almost as accurate as Aristarchos’ sidereal\textsuperscript{43} year.)

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\textsuperscript{5}/7 + 1/102 (23): Rawlins 2018U §K5.]

And this adds more credibly to the position that the famous geocentrist astrol–ogers, Hipparchos & Ptolemy, drew much of their astronomy from often-unnamed but able\textsuperscript{44} astronomers (probably heliocentrist\textsuperscript{45}) — not politically well-connected — whose achievements I.e., the most vital shortcoming here is not of math but of character.

Ptolemy did not recognize the precession of the solar apse. (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 ancient astronomers made the tropical year (fn 43); fixed the anomalistic year (actual value, for Phil 1–epoch, by Newcomb’s solar theory: 365\textsuperscript{1/4} + 1/102) would be tougher yet. [ii] The ancients knew the Moon’s anomalistic motion only to a precision of c.1\textsuperscript{7} per 100\textsuperscript{4} 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 apse & the stars. [iii] Rawlins 1991W SN5 estimates an error of nearly 5\textsuperscript{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 its eq. 43; but such an unequivocal choice was relevant to either the anomalistic year or the sidereal year, which were of no popular interest whatever. This contrast — and the known high accuracy of ancient’s 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 observed 100 times better than his anomalistic yearlength. fn 43.) [c] A hypothetical ancient scientist who determined solar anomalistic motion need not have known 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.

If the “or [ ]” at the end of the table of yearlengths relayed at Neugebauer 1975 p.601 is actually a fragment of an otherwise lost work, then the yearlength cited equals eq. 6.

Specifically: Hipparchos 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).

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


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 $A = c.600\textsuperscript{7}$, consistent (idem) with Sample A’ of the Ancient Star Catalog. (Though, van der Waerden 1988 eq.84 makes an intelligent case for $A = 750\textsuperscript{7}$.) This reconstructed orbit was pretty accurate ($\text{C2}, \text{Neugebauer 1975$ s.627, & DIO 1.1(5)}$ fn 15; indeed, its error-wave-amplitude appears to have been less than that of any of Hipparchos’ three successive solar orbits (ibid §G10, K10, & K9). See, e.g., Rawlins 1982G, Rawlins 1985G, Rawlins 1987 (p.238 & fn 38). Also Rawlins 1991W §K4, N17, eqs.26&24.

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

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G Postscript A: Un-Re-Evaluating

G1 A Hist.sci scholar of the highest credentials & international esteem wrote Thurston (1994/12/29, with copy to DR):

I am very pleased to see that your article will appear in JHA [26:164; 1995/5], as it deserves . . . . I am glad both that the meanderings of Jones’ argumentation can be set aside, and that Rawlins will have a little bit of recognition for the discovery of UH [Hipparchos’ long­lost solar orbit, used by him for his solar­obs trip C]. . . . I have checked [Rawlins’] calculations and found nothing to quibble about. I hope your article will trigger some important re­evaluations.

G2 In the many months since, nothing has been re­evaluated.\hfill 19

H Postscript B: Biggie’s Smallness Confuses Even Him

H1 On 1995/6/2, Thurston sent the following to Hoskin, asking that it be printed as a correction to Thurston’s JHA 26:164 note:

The phrase “As Jones shows” which starts the second paragraph is not part of the original note and was added without my knowledge. It was Rawlins, not Jones, who showed that a simple eccentric motion fits the data. What Jones did was to try but fail (as his addendum openly and honestly acknowledges) to show that no continuous motion fits the data. . . . Hugh Thurston

H2 On 1995/6/20, Hoskin responded by: [i] transmitting an incredibly complicated attempted explanation for JHA’s inexcusable behavior, and [ii] refusing to publish Thurston’s very brief §H1 notice, instead publishing Hoskin’s own ultra-brief correcting note (pleading carelessness, contra §§C3&C5 and §§D7&D9), thus, directly refusing to publish an unambiguous notice that DR has contributed significantly to scientific history. (JHA can hardly claim that it has made DR’s contributions clear, when its own Editor pleads so much confusion about the matter, that JHA has had to correct itself!) The latest JHA note could’ve simply said that “As Rawlins shows” was meant. But Hoskin sees black at the middle word: after years of Hoskin attempts to embarrass & damn DR into nonexistence (for which JHA will never express regret), the JHA just can’t bring itself to frankly admit an important DR achievement. i.e., the most vital shortcoming here is not of math but of character.

H3 Hoskin’s §H2 gyrations add to those described earlier here — and have the same bottom lines: embarrassment-minimization, & the JHA’s squandering yet another opportunity to partake of the bracing & cleansing experience of honest, open, uninhibited generosity. (In extenuation: [i] Even though posing as the type of deity known as “editor”, Hoskin is human; no one enjoys self­embarrassment. [ii] He has, for years, had untrustworthy advice from archons whose eminence superficially implied trustworthiness, and whose hefty censorial bigotry constrained editorial options.) Plain facts: [a] Hoskin tampered with Thurston’s text in a way that tended to cover JHA shame. [b] JHA then prevented correction of this inexcusable alteration by failing to send proofs to Thurston. [c] Now, it must prevent publication of the further §H1 embarrassment — failing yet again to print (as DR repeatedly urged: fn 7 & DIO 4.2 §B4; see also §§D5&D6) clear JHA acknowledge­ment of DIO’s undeniable priority in proving that (contra JHA’s lead paper) Greek-style
E6 I remain (as always)\textsuperscript{17} prepared to sit down with Muffio (not in terrified-Muffie \citep{DIO 4.3 115 §§G13-G14} secrecy this time) — as I tried to do at the 1994/5/6-8 Dibner conference — in order to work towards: [i] mutual understanding, [ii] Hist.sci tolerance of heterodox research approaches, & [iii] establishment of safeguards and of equitable procedures for lowering the chances that future academic dissidents will have to endure the sort of dictatorial arrogance that has cursed the Ptolemy controversy. By [i], I do not mean that we will cease disagreeing, and criticizing & improving each others’ knowledge & positions. But I would prefer to see this done unrelentlessly and without vituperative belligerence. I.e., why can’t both sides confer, face to face, to enjoy the high heritage we share in common and to see our differences as sources of nourishment, not as heretical sins? (Further: have Hist.sci archons considered the time-factor here? Like any hatred or other narcotic, Muffia loathing of DR is just getting progressively more unrenounceable as the years pass.) One side continues, as ever (for over a decade: fn 17), to be ready for this. Just as one side has for years repeatedly acknowledged the value of some of the other’s output (despite receiving largely noncitation and hit&run sniping in return). And too much of the Hist.sci community continues, as ever, to despise-exile the accessible, appreciative, & non-party-line side.

E7 Indeed, during the 2 decades of the ancient astronomy Controversy, not one Hist.sci scholar has ever once expressed a word of appreciation for DR’s consistent policy of praising & encouraging (& pointing out his intellectual debts to the valid work of snobster-enemies, even those attempting to murder his career \citep{DIO 4.1 σ 5 fn 1}. If one didn’t know better, one might get the idea that maintaining principled, impartial fairness in the evaluation of academic output, does not concern or so much as interest Hist.sci archons.

F The Positives

But I’ll end on the upbeat aspects of the Muffia-Orbitality incident. While the JHA appears to have done little more than the bare minimum [this consistent strategy becomes crystal clear at JHA] (so that, knowing AAAS-Science to be watching \citep{[A]3-A4}, JHA can appear honest), nonetheless: some slight improvement is visible.

F1 First, Hoskin’s promise to publish the Thurston & Jones note was kept. (A similar Hoskin promise to DR has not been kept: \emph{DIO} 1.1 §5 fn 25 & Rawlins 1991W §O8. DR continues to await its consummation — and Hoskin’s attainment of sufficient maturity to communicate with DR.) Further: the following \emph{F2/F3} compliments reflect some credit also upon Hoskin, since the JHA printed the material.

F2 Second, Alex Jones (Its Board) deserves commendation for going somewhere beyond the minimum: it was evidently his decision to cite the \emph{DIO} 1.2-3 paper correcting his errors — despite the fact that this paper was none too gentle on him & his Muffia colleagues. (I.e., it was written rather in the fashion of the Muffia scorn heaped for decades upon Robert Newton & co.) Which factor only adds to the praise Jones merits — and to the respect which \emph{DIO} henceforth owes him.

F3 Third, Hugh Thurston was the sole participating scholar who possessed both the specialized math knowledge and the sheer nerve that were required to compose his correcting note and then to send it to the \emph{JHA}. For these deeds, he merits the gratitude and admiration of every ancient-astronomy scholar — and, as well, of all academics who value open thinking and free speech in the scholarly community.

\begin{thebibliography}
\item [\textsuperscript{17}]{DIO 1.1 §3 fn 7, DIO 1.3 fn 269.}
\item [\textsuperscript{18}]{With the critical difference that \emph{DIO} explicitly recognized the worth & high scholarship of some of Jones’ work \citep{DIO 1.2 §4} and suggested that he had elsewhere been let down by poor referencing (\emph{ibid} §F4).}
\end{thebibliography}
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 Hipparchus’ use of a 7160° cycle.\(^{52}\) Perhaps Hipparchus was using the cited — 719° & -140° pair for confirmation, not discovery. But Ptolemy’s result is slightly worse than Hipparchus’ (or whoever’s: §A6) eq. 19.\(^{53}\)

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) Hipparchus would have used the pair of perigee eclipses highlighted above (-830° & -140°).

D5 And, knowing that 9146 anomalistic returns had occurred during 5345 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:

\[
V_H = \frac{58543M_A}{9146} = 251M_A/259 = 92123683^d 6972480 = 27°555469(8)
\]

which was correct to about 1 times! To be precise: the mean error (of eq. 8) during the centuries\(^{53}\) discussed in this paper = -1°3.2°±0.1°. (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)\(^{55}\) daily motion given at Almajest 4.3&4 (based on eqs. 2&3):

\[
V_H = 360^d/V_H = 360°/(M_A/259) = 13°05'53''56''29''38'''38''''
\]

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''''

\(^{50}\) Hipparchus’ & Ptolemy’s approaches are better than that Aristarchus may’ve used. Yet, see fn 79.\(^{51}\)

\(^{52}\) Though in the guise of an 8 1/2-century-span (note §H15 item (d)) trio-proof, Ptolemy’s Almajest 4.6 development attains laughingly ornate agreement with Hipparchus’ anomalistic motion (fn 55). (As also for the planets, Ptolemy’s amaturish 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 unanted non-ignorable perturbations would degrade any other type of empirical foundation. See fn 18 & fn 35.) I note that between two of the 4.6 eclipses (-719/3/9 & 136/3/5), there were 45 Metonic cycles = 10575 = 10470\(^{19}\) = 19°3122860. In a Metonic series of sycygy, eclipses recur every 24 Metonic cycles and thereafter, 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).

\(^{53}\) Almajest 6.9 (again using the -719/3/9 eclipse) takes advantage of the near-commensurability: 716° = 77°, where 10 such cycles give an approximate (fn 49) demi-return in anomaly. (True, thirteen 716°-cycles would have to produce a single lunar month, but to use this, Ptolemy would’ve needed data from c900 BC.) [See below at §F3].

\(^{54}\) For the real ancient lunar and solar periods and motions in this paper, I use modern-calculated values for Phil (1 -321/12/11 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 fn 15).\(^{46}\) For the modern calculations I have attempted here for the solar day’s proper motion, I refer to Ptolemy’s Almajest 4.2&9 correction, of Hipparchos’ draconitic motion, degraded its accuracy by a similarly trivial amount. However, it is hard to fault Ptolemy’s Almajest 4.3&4 correction, of Hipparchus’ draconitic motion, degraded its accuracy by a similarly trivial amount. However, it is hard to fault Ptolemy’s

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E How To Throw Away a Chance for Progress

In addition to Hoskin’s continuing silence towards DR:

E1 At the 1994/5/6-8 Dibner Inst conference (M.L.T.), DR spoke amiably to a number of Muffia scholars. But no communication has come from any since. (To the contrary, no unsigned Hist.sci scholar dares submit papers to DIO, for fear of cult ostracism.)

E2 Dr. J. H. A., the Editor of the eminent (& very high quality) Danish series Acta Historica Scand Med (which has published Neugebauer & Pedersen), is the Editor of the eminent (& very high quality) Danish series Acta Historica Scand Med (which has published Neugebauer & Pedersen), offering to [a] publish the DIO Tycho catalog, [b] distribute thousands of advertisements, & [c] make DR famous. [Curious. DR never sought such spectacular interaction. Index Librorum Prohibitorum Vat City 1948 p.ax: It is the faithful’s duty to report dangerous (fn 13) literature.] Contracting for this Imprimatur would risk: [i] removing (credit from DIO & control) (see DIO 2.1 & 3 fn 8) of the work’s date (or even act) of publication into said hands. (DR just went with imminent distribution of the uncensored DIO 3 rendition of the Tycho catalog.) DR had already been through this process before (again: instant DR-assent requested), with Moesgaard & JVA, as regards the latter’s written 1981/9/17 acceptance of a paper now known as Rawlins 1999: see DIO 1.1 fn 25. (On the former, see: JVA 2.1 fn 56&170 & DIO 4.3 fn 41.) Over 10° later, the paper remains unpublished. (General rule: don’t rush into publication-offers connected to cliques who’ve spent years ignoring and—slandering everything you’ve already written when you’re on the verge of having something impressive to say. But when they’re most stingy they’re also most imagitious when they’re on the verge of having something impressive to say. DR 1.1 & 25 fn 99, DIO 4.2 & 9 & 9, DIO 4.3 & 7 fn 42; i.e., partial-essence citations come first, & then later we get to publication-cooperation.)

E3 E.g., the upcitsing if critical revelations at DIO 3 fn 54, fn 141, §§L8-L11, §§M4-M5 (D675-80, 971, 1001-4).

E4 DIO 1.2 fn 92. [A Muffia complaint of insufficient notice to its literature is funny and—or nutty: DIO 1.2 fn 144.]

E5 See DIO 1.2 fn 25! Near-exceptions (all late 1970s): a return-phon emoji from Brittan, a too-busy-to—check librarian; & a return-phon emoji from Sweden’s Mr. Muffiosi, and a return-phon emoji from the Dibner conference. Alan Bowen creditably attempted (evening of 1994/5/7) to have a leisurely conversational exchange of ideas privately with DR but was swiftly warned by Mr. Goldstein to cease.

E6 A point of absolutely null weight for scholars [i] primarily interested in present funding, & [ii] having complete [ mechanisms to withstand a corrupt system’s flaccid capacity for shuffling—repelling considerations of reason & equity. Infinitely (I)}.
achievement, which used the same Greek-trig-orbit idea to solve all three of Hipparchos’ eclipse trios.

D6 Regarding ref-comment §D2, on historical support of Greek-trig solar solutions: [i] All three orbits (fitting trios A, B, & C) are shown to be founded upon seasonlength data which are historically connected to Hipparchos. See ibid §M5 (trio A), §§K4-K9 (trio B), & DIO 1.1.6 §§C6f (trio C). [ii] Moreover, the long-mysterious amplitude of the error curve of the zodiacal stars of the (late Hipparchos) Ancient Star Catalog is perfectly matched by the amplitude of the error curve of the (late Hipparchos) trio C orbit. (See ibid §§F3-F5.) [iii] Finally, the Almajest 5.5 mean longitude of the unrecomputed 2\textsuperscript{nd} position of trio C agrees on the nose with the DIO solution for trio C. (See the astonishing match at ibid §H5.) Again, JHA readers are not informed of any of this — despite the urging of Hoskin’s chosen referee that such material be brought in.

D7 Evidently fearing that further interaction might produce requests for adding such — which would reveal just how powerfully & consistently the historical & mathematical evidence favors the DR solutions — Hoskin instead: [a] continued the JHA’s immature refusal to communicate with DR (whose corrections were, after all, the cause of the entire Thurston article & Jones retraction!), and [b] failed (uncharacteristically) to send Thurston the article’s proofs.

D8 The results of Hoskin’s behavior (which placed accuracy not quite atop the JHA’s list of priorities [§I]): [a] A potentially confusing slip\textsuperscript{7} never got corrected. [b] An astronomical immortal’s name was mis-spelled. [c] The 2\textsuperscript{nd} observation’s time of day was 5 2\textfrac{1}{2} hours, but the JHA printed it as 5 hours. (Such errors will create problems for any JHA reader who tries to check the math of the situation. But, then, as Thurston has often pointed out, Hist.sci readers seem to be an amazingly trusting lot: whereas he instinctively checks out numbers in papers [including DR’s] while reading them, this appears to be a rare trait — which Alex Jones & DR are one in admiring Thurston for.) [d] An erroneous attribution was inserted (§D9).

D9 In Thurston’s ms as submitted, the 2\textsuperscript{nd} paragraph begins: “There is in fact a simple eccentric solar motion . . . that accounts for [the trio C observations].” However, in an attempt to save Muffia face, Hoskin made an astounding, uncomprehending, and invertedly\textsuperscript{10} false insertion — without even asking the author’s (or Jones’) permission! — and altered the above passage to read (insertion italicized): “As Jones shows, there is in fact a simple eccentric solar motion . . . that accounts for [the trio C observations].”

D10 The foregoing details are provided partly as a warning to those scholars who are trusting enough to send material to the JHA. Lesson: you never know how it’s going to come out. . . . (It’s an Art Levine satire-fantasy,\textsuperscript{11} come to life.)

\textsuperscript{7} Thurston trustingly took the year-numbers of the three observations from p.415 of O.Pedersen’s valuable (as Thurston rightly notes) but error-ridden (DIO 1.1.5 fn 6) 1974 Survey of the Alm; however, all these numbers are low by unity. Thurston intended to make such corrections when the proofs arrived. Which they never did. Fortunately, Pedersen’s flub has no effect upon deduction of eccentricity & apogee (which are the only elements Thurston treats).

\textsuperscript{10} The whole point of the 1991J SJA paper (which the Thurston note undoes) is the claim that Greek eccentric motion will NOT account for the data.

\textsuperscript{11} A-Levine (ContribEd, WM) “Have You Got What It Takes to Write for the Washington Monthly?”, WM 21:1-54. (Editor Chas. Peters not only had the self and critical-soul to run this in 1979 — he then re-run it in the 1989/92 issue. “Writers for The Washington Monthly sometimes complain that we’re a little too, shall we say, unbusinessed, about urging our point of view upon them. — and their manuscripts. . . . [Are you sitting at home wondering:] How can I become a Washington Monthly writer? . . . Could I get published in your magazine? Welcome aboard! Our top-notch editors will be glad to add [to your ms] . . . loads of thought-provoking opinions without troubling you with the dreary task of doing it yourself. Many contented writers have said that there’s no surprise quite like seeing a manuscript of theirs end up as an article in The Washington Monthly. Often they find themselves espousing ideas they’ve never even heard of, much less agree with.”

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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 perige: fn 65.) If so, then the ancients probably had access to a compact eclipse trio\textsuperscript{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 –830/24 Eclipse

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

\[
3277^d = 3512^v
\]

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

\[
9834^d = 10536^v = 10668^v 1/2 + 22^v = 795^v - 65^v = 290315^v 07^h
\]

(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\textsuperscript{st} relation (eq. 3 or eq. 10), whose error is estimated at §D5.

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

\[
7042^d = 7547^v = 7642^v - 18^v = 569^v 1/3 = 207954^v 11^b
\]

\textsuperscript{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: –833/26/27, –839/20/21, –831/9/10 (see fn 103 & §H4), & –830/24/5. The mean amplitude of the eq. 1 time interval’s variation was \( \pm 38.35 \); this amplitude-smallness is, of course, mostly due to eq. 1’s slight sidereal-year \(- 7/2 \)\textsuperscript{nd} remainder. (Multiply twice sin 3\textsuperscript{rd}/4 times the 2\textsuperscript{nd} solar eq.cr amplitude, and divide by the 1\textsuperscript{st} hourly synodic motion, to find 0h.5. Checking other cycles [In 19] based upon eq. 3 would verify that this amplitude was consistent with solar-anomaly-causation; see fn 79.)

\textsuperscript{57} 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.

\textsuperscript{58} Other useful syndric-anomalistic cycles: \( 1520^d = 1629^v = 1649^v 1/2 - 1^v = 133^v - 40^v = 448861^v 1/2; 5787^v = 6020^v = 6280^v 11^v = 468^v - 47^v = 170893^v 13^v; 8046^v = 8623^v = 8731^v 1/2 - 5^v = 659^v 1/2 + 2^v = 237603^v 04^v.

Unstated lunar-anomaly remainders: \( +1^v, +0^v \), & \(-0^v\), resp. (For eqs. 1, 10, & 20: \(-1^v, +1^v \), & \(-145^v\), resp.)

\textsuperscript{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 −7199/9/12 (Almajer 4.6), except that the latter one occurred below his horizon. 

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

\[ V_Y = 9831M_A/10536 = 3277M_A/3512 = 27d.5546000508 \]  

(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 matching the accuracy of eq. 2; see Rawlins 1991H fn 1.) So the majority of either eq. 3 & eq. 10 was just right, and it is a credit to Ptolemy's judgement 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 1/10. 

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\(^{a}\) which is just short of the outer limit (25\(^{a}\) 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 §F1.) 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 yr cycle after −381/12/12 eclipse (which is attested at Almajer 4.11), \(^{d}\) 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\(^{b}\) (5\(^{b}\) shorter than 9831\(^{a}\) in eq. 11). Division by 10536\(^{e}\) (eq. 11) produces \( V = 27d.554583, \) ordmag

\(^{59}\) 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 must have been 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 from Greeks by the 6th century onward, identifications of 569 yr-cycle eclipses should have made. [c] Possibly the Greeks did use either this cycle or a similar one (e.g., §D3’s 7160\(^{a}\) = 7770\(^{a}\) \( \approx 7673/1/2 \approx 579^2, \) at Almajer 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\(^{b}\) (or eq. 18’s 2729\(^{b}\) or its triple [662 yrs], which has a better lunar anomalistic return but a remainder of 40\(^{b}\), nonetheless) but by continued-fractions analysis. (Eq. 19’s derivation is 345 cycle pair 5458\(^{a}\) apart.) 

\(^{60}\) 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. 

\(^{61}\) 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 207954\(^{b}\)). 

\(^{62}\) See also Ptolemy’s draconitic reversion: fn 55. 

\(^{63}\) For our similar but far greater debt to Ptolemy, see Rawlins 1991W fn.94. 

\(^{64}\) This pair ended a series of several 795 yr-epochs connecting two cycles, a series which had started with the pair −10479/7/27 & −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 & −234/6/10.) Note, however, that this series of 795 yr-cycle pairs was not the only one that ended in Hipparchos’ time. Pairs which ended other such series were: −9351/8/6 & −140/1/72 (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 \( x = −113^\circ \) & −112\(^\circ\), 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\(^{b}\) pairs are from saros-series whose means-Mucke numbers differ by 53\(^{b}\)] 

\(^{65}\) If, despite its large solar-anomaly remainder (−65\(^{b}\)), eq. 11 (795 yr base) was found via the −36/12/7 eclipse, then the discoverer preferred it to eq. 1 (345 or 90 yr base) simply because its interval was more than twice as long. The −36/12/7 mid-eclipse was at 22:51 Mean Time (22:56 Alex App Time), at \( A = 74^\circ.7\) & \( \beta = 0^\circ.9\) (topocentric); its magnitude \( m = 6.9\) (N.Ierb).
From DR’s 1995/3/14 letter to a Hoskin-circle Hist.sci figure (footnotes in orig):

Why is it that I want refereeing of the ancient astronomy controversy — while, by contrast, the Muffa has flouted it for decades? Why has Hist.sci conformed this? . . . JHA’s Hoskin will not [even] look at DIO (see DIO 4.2 §7 §B6) . . . Perhaps some think that this ploy will help excuse a deliberate policy of nonciting DIO. Moesgaard has told DR directly (1994/5/6) that he swore over 10 years ago to have nothing to do with DR, so his noncitation-policy [see bizarre instance remarked parenthetically at DIO 1.2 fn 56] is deliberate by his own account. Is this honest scholarship? Is it considered ethical in Hist.sci [a] to fake the nonexistence of existing discoveries by Unapproved scholars, and [b] to fake the nonexistence of existing ironclad refutations of Approved scholars’ attacks on heresy — attacks which are then cited [e.g., Centaurus 37:97, p.149 n.1] . . . without informing the reader of DIO’s demonstration of these attacks’ fatal blunders.

From DR’s 1995/3/14 letter to a Hoskin-circle Hist.sci figure (footnotes in orig):

V.

B4 Why would a cult insist on going right on evading honest parties in such a haughty fashion, thereby inviting the continuation of a running-sore disgrace to academe? Well, if you want to know, you’ll have to ask the cultists themselves.

C Thurston’s Proof

C1 In the summer of 1994, Hugh Thurston (Prof. Emeritus, Univ Brit. Columbia Math Dep’t) found, by geometric (ancient) means, the orbit which fits the Almagest 3.5&6’s threesome of Hipparcian solar positions: “tio C”. Used to Keplarian orbits (where direct solution is impossible), DR had unthinkingly presumed that the solution would be iterative. Thurston quickly solved the problem — noniteratively. He sent his result to the JHA, adding a note which pointed out that it confirmed the (iteratively-derived) DR results published at DIO 1.1 §6 [eqs.17-18.

B From DR’s 1995/3/14 letter to a Hoskin-circle Hist.sci figure (footnotes in orig):


0.1 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 −380/2/4 & −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 −380/2/4 eclipse. That is, all 3 of the eclipses we’ve concentrated on here (§§D&E) were perigee-events:68 −380, −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 1811 period between ss eclipses is governed by the relation:

$$223^2 = 239^2 - 3^2 = 242^2 - 0^2 = 2685^2/13 = 18^4 10^2/3 = 18^5 10^2/2 = 18^6 10^2/3$$

(14) for which we remember (§A1) that superscript K = Kallippic years of length Y K, where:

$$Y_K = 365^4 1/4$$

(15) in contrast to the Hipparchos-Ptolemy “tropical”67 yearlength adopted at Almagest 3.1-2&9.4:

$$Y_d = 365^4 1/4 - 1/300$$

(16)

F2 For comparison: the actual tropical yearlength at the epoch of Phil 1 was about 365.254/4 – 1/133, thus eq. 16’s Y d was too high66 by 6 — i.e., 10 centuries!

F3 The mean ss-length of 8 centuries is governed67 by eq. 14’s −0282 draconitic remainder,68 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 more than 7 centuries and more than 10 centuries, the average ss lasts69 between 8&9 centuries72

65 The three cited eclipses’ lunar mean anomalies were: +1° (−380/2/4), −1° (−140/1/27), & +1° (−36/1/27).

66 See Almagest 4.2 or Geminos 18.6. The 10d23/maider is rendered as 10d23 at Neugebauer 1975 p.502, from the Babylonian yearlength/monthlength ratio 22.12,08 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 Muffa 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?

67 See fn 13.

68 Causes examined in Rawlins 1985.

69 I.e., multiplying the famous saros-period (eq. 14: 18102/3) times (222 − 0282/saros), we have a figure close to fn 72’s 8 1/3 centuries.

70 The −0282 draconitic remainder was the real value. (In all the cycle-equations of this paper, the remainders displayed as actual [DIO-computed] ones, for the epoch Phil 1 = −333/11/12. 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 71 ss-lengths (minus 1), and multiplying by eq. 14, yields 837 yrs for the average (umbral) ss-length for the data of Liu & Fida 1992 Table 3.1.
as a near-perfect synodic-draconic return. (See also Neugebauer 1975 p.310.) And how well did the ancients do, when choosing eq. 19 (= 1611738 = 441.5° ± 97°) as the basis for their draconic 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.

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’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 (1), 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-ss is 38 saros periods long.)

The 1st sure post-Ptolemy 795 yr-cycle eclipse pair was −540/19/10 & +254/11/12, visible in Babylon & Europe, resp. Pogo 1938 (recommended without checking by Menzel & Gingrich 1962 p.xi) contradicts Oppolzer 1887 in claiming that the syzygies of 236/10/11 & 245/10/12 were eclipses. But Liu & Fiala 1992 & Meeus & Mucke 1992 agree with Oppolzer that no eclipses occurred. (My calculations find: magnitudes rs = +1.01° & −0.1°.) Between 37 BC & Ptolemy’s death, the nearest thing to a break in the 795 yr-drought was the pair starting with the syzygy of −812/2/15. (An eclipse then could have paired with the eclipse of −812/18/19, the start of which was visible in Rome & probably Alexandria.) However, all sources agree that there was no −812/2/15 eclipse: Oppolzer 1887, Liu & Fiala 1992, & Meeus & Mucke 1992. (Even Pogo 1938.) I calculate rs = −0.2.

3 G.1, which was of the highest order; but JHA cannot print DR’s use of the word “Mufa” without a 795 yr pair occurring, is the fortunate accident which enables us to protect one’s reputation for honesty.

1 I gather that JHA requested & received the article’s title, for the Jones note’s bibliography — but then broke the journal’s own hitherto-sacredly-rigid style-rule by omitting this title, since Hoskin personally disapproved of its mention of “Mufa”. (He has printed & never apologized for) highly insulting Mufia charges against R.Newton’s competence [DIO 1.17 [11] §C7], which was of the highest order; but JHA cannot print DR’s use of the word “Mufia” without a 795 yr pair occurring, is the fortunate accident which enables us to protect one’s reputation for honesty.

2 Are Mufia’s errors in his 1991 May JHA lead paper and that the correct math was first published in DIO 1.2 & 3’s “Mufia Orbits”? A2 Whatever the shortcomings of this JHA notice, it is nonetheless an event which (we are obliged & glad to acknowledge) DIO predicted would not happen at all. A3 However, what JHA Ed. Michael Hoskin has done so far is depressingly minimal — and even this only occurred after Eliot Marshall of Science (Amer Assoc Adv Adv) placed a phonecall to Jones’ home and another to the Cambridge Univ-trained mathematician Hugh Thurston (who had directly informed the JHA of its blunders) — a phonecall also known to Mufioi (notice at DIO 4.2 [7] §B38). A4 So, it has taken [a] years, [b] independent-method Cambridge-math-verification, & [c] two AAS phonecalls even to get the JHA to own up to errors of elementary arithmetic. And there has not been the slightest indication of Mufia-JHA interest in going beyond this. (To the contrary: §D6, §E3, §G, §H, & DIO 4.3 [11] §F4.) The consistent, regrettable suggestion is: for JHA & Mufia finally to acknowledge their manifold errors of post-highschool math will presumably require even greater stimulation than that cited in §A3. We, at DIO will do what we can, to provide what is needed. A5 But one must crawl before walking. So this JHA correcting-note is encouraging; and we must cheer&chide the Mufia along, as it snails down the long road towards integrity.

\[ \text{as a near-perfect synodic-draconic return. (See also Neugebauer 1975 p.310.) And how well did the ancients do, when choosing eq. 19 (= 1611738 = 441.5° ± 97°) as the basis for their draconic tables? With components this large, the best choice should be accurate to better than 1 part in 10 million. And the accuracy of eq. 19 was indeed about that fine.} \]
Hugh Thurston  Mayan Eclipse Table  1996 January  DIO 6  §2

[7] for further reading. Of all the theories that I have come across about the use of the table, this is the only one that has any degree of plausibility. The statement, made all too often, that the Mayas could predict eclipses is definitely false.

References

Rawlins  Princettitute-Muffia Ousertà  1996 January  DIO-J.HA 6  §1  17

(Eq. 19 is less accurate today, merely 2 parts in 10 million.) 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 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 that it (eq. 2)98 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 2u3 term, by which the anomalistic part of eq. 17 differed from precisely 1/3 of an integer, ensured that (on average) 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 tri­angle that was pretty stable in time (i.e., mean drift = merely c.2u29 yrs). And eq. 18’s similar anomalistic 13u2 term ensured that, in the long term, each triangle faded into another which was22 (on average) 13u221 yrs ahead of the previous one.

77  van den Bergh 1955 p.24 mentions eq. 19 but (due to basing his p.18 continued­fractions 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 synodic­draconic cycle (eq. 19) was so wonderfully correct.

99  It should be pointed out that the eclipse­pair (— 719/3/8­9 to — 278/6/19­20, the latter event half­visible) proposed by Rawlins 1985S is not ideal as a basis for eq. 19: [a] The eclipse­magnitudes are different. [b] The number of anomalous 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 compensated­for (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 more­sophisticated Almajest 4.6 alleged determination of the anomalistic lunar motion can be faulted for using an interval not anomalistically­integral: 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 yr­cycle pairs. (See fn 110 for a hypothetical example of such averaging, applied to 781 yr­cycle data.) Such pairs are common. Indeed, the — 720/3/19­20 & — 279/6/29­30 pair was also available — though the interval was almost a full day less than eq. 19’s 5458° = 161178 days. This is the price one pays for non­integral anomalies: here, the lunar eq­ct­difference minus the solar eq­ct­diff equals the negative mean lunar elongation diff = 9°34, an amount which lunar synodic motion will require most of a day to compensate for. By contrast: for the — 719&278 pair, the lunar eq­centers are nearly equal (within a half­degree), and the mean elongation discrepancy — predominantly due to solar­anomaly­inequality — 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 non­independent uncertainties: see fn 48.)

50  For both — 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 period­commensurability relation of them all, eq. 30, has a huge anomalistic remainder. Using the real [not Metonic] tropical year, we have: 235° = 19°+02h = 19°—08h = 252°—53° = 255° + 8° = 6939d17h.) Note that, for both situations, further data may have been available.

43  Fn 43.

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

82  The modern eq. 18 remainder is more than 14°. But the past­tense references, in our PBT discussions here, apply with virtually equal force today.
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 alot of anomalous 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

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.

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 the latter (both were part of the same ss, ending at −36/12/7), namely, −957/11/20-21 & −1629/9/22-23 and −921/12/12-13 & −126/10/14-15. On the other hand: [a] No attested Hipparchos eclipse observation is part of a visible 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 material (§E6) can explain eq. 10 just as well. Nonetheless, see the speculation of §§H6-H7.

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.

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 (2nd century BC) & Ptolemy (2nd century AD), a time whose high science has hitherto been a virtual blank 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.

Rawlins 1985K traces the Almajest 9.3-4 Venus & Mars tables to the reign of Kleopatra (52-30 BC), 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

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[Note by DE: 173½/3 is precisely one-third of two rounds, thus it has been reasonably (if controversially) speculated that the Mayas may have chosen their 260-day round-interval out of interest in eclipses.]
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C Reconstruction

C1 We can deduce some details of how the Mayas might have constructed their table if we look at the table itself in a bit more detail.

C2 The numbers in the top half of Dresden codex page 54 (reproduced above at page 32) are:

<table>
<thead>
<tr>
<th></th>
<th>B: 1211</th>
<th>1388</th>
<th>1565</th>
<th>1742</th>
<th>1919</th>
<th>2096</th>
<th>2244</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>177</td>
<td>177</td>
<td>177</td>
<td>177</td>
<td>177</td>
<td>177</td>
<td>148</td>
</tr>
</tbody>
</table>

The numbers in row B are a cumulative total of the numbers in row D: to get a number in row B, add the number in row D to the previous number in row B. But six times in the full table the number added is 178, not 177. What has happened? Should the number in row D be 178, or is there a mistake in addition? This is where row C helps.

C3 The glyphs in row C are the days of the Mayas’ “sacred round”. These days, like ours of the week, are repeated in fixed cycle independently of the date, but unlike our days of the week they form a long cycle of 260, not a short cycle of 7. [See §C9 & fn 3.] The three days in the first column of the top of Dc page 54 are the 78th, 79th and 80th in the cycle. In fact, throughout the table, each column of row C shows three successive days. The days in the middle of row C of Dc page 54 (read across) are obtained by adding successive numbers in row D (%C2) to the one before (and, if the total is more than 260, subtracting 260):

<table>
<thead>
<tr>
<th></th>
<th>C: 79</th>
<th>256</th>
<th>173</th>
<th>90</th>
<th>7</th>
<th>184</th>
<th>72</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>177</td>
<td>177</td>
<td>177</td>
<td>177</td>
<td>177</td>
<td>148</td>
<td></td>
</tr>
</tbody>
</table>

Where the cumulative total (row B) increases by 178, the number of the day in the sacred round (row C) also increases by 178 (or by 178 minus 260), so the addition is correct and the compiler of the table has, for some reason, not entered the 178 in row D.

C4 Most early astronomers had a figure for the average number of days in a month. The Chinese Sun Tong calendar of 7 B.C., for example, used the relation 81 months = 2392 days. Ptolemy quoted “the ancients” as knowing that 669 months = 19756 days. [See here at §83.] The Mayas had similar figures. From inscriptions that gave the age of the Moon at various dates, which must be calculated not observed, because some of the dates are mythical, John E. Teeple [6] deduced that the Mayas were using 81 months = 2392 days at Palenque and 149 months = 4400 days at Copan.

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

H1 Anonymous is certainly now the name of the 9th 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 69 ml 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 9th century BC. Since such awareness doesn’t develop overnight, the hint [70] is that there: ss were being studied in Babylon in the 8th 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. Thererafter, instead of declining, the magnitude stabilized at about 1.2 digits and 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 falling to m = 0.9 on − 884/1/3-4. By −866/11/13-4, the magnitude had fallen to m = 1.3, and the next eclipse of this ss visible 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 tentatively speculate that Babylonians of this early epoch additionally were aware (whether from direct speed-estimation or from theory — or perhaps just from the #H2 linger-factor) [92] that the − 830/2/4 eclipse was a perigee event. (See also §D6.)

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” [reprinted uncritically from Almagest 6.6] of the superficially-plausible-but-undoubtedly-false Ptolemy-Princetitute proposition [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 & Moucke 1992, m = 0.4; Liu & Fiala 1992, m = 0.5; DIO, m = 0.5. 89

90 And see fn 30 on the 1309 cycle’s accuracy-compatibility with eq. 2. For counter-hints, see fn 88, 99, & 103. 91

90 This remarkable ss lasted from −921/12/12-13. Since such awareness doesn’t develop overnight, the hint [70] is that there: ss were being studied in Babylon in the 8th millennium BC.

92 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 Almagest 9.3-4 mean motions: DIO 4.3 71.) 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. 94

Note: there are several scribal errors in our illustration of Dc page 54. (E.g., 1742 is miswritten as 1748.)

An explanation of how to deduce these numbers from the glyphs is given in, e.g., Hugh Thurston Early Astronomy Springer 1994 pages 196 & 201.
times he cites], he would have found an interval of 147.3, or less than 29.5/month, which is shorter than the average 29.45306 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 4.3 115 [§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 −381/9/9-10 & −380/2/4 eclipses are five months apart— and both were visible77 in Babylon. So, this very rare short observable interval might have been a cause of Babylonian interest in the −380/2/4 eclipse: Which hints at a further possibility (one that does little violence to what we already know of early Babylonian astronomy): that the −381-380 grazing-eclipse-pair marked the first gleanings of organized eclipse-prediction in Babylonia. (Note: −381/9-9/10 = ss-start, and −380/2/4 = 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 −380 eclipse due to a 5 month passing affair (the most conservative interpretation at §H4) or 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 enthral modern historians), did early Babylonian lunar priests keep (now-lost)73 records of the eclipses of entire ss?

{H7} If the Babylonians specially preserved the −380/2/4 ss-conclusion eclipse, then are we witnessing the starting 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 19th century BC, the partial (4 digit) eclipse of −1804/6/29. This is near the misty era of Hammurabi and Ammi-zaduga — 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 conclusion100 of Neugebauer 1957.)

106 DR’s spotlighting of the −380/2/4 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).

107 The very occurrence of the −381/9/9-10 dawn eclipse (r = 0.6 digits by Oppolzer 1887, & 0.1 digits by Mees & Mucke 1992) is said by Poge 1938 to be questionable & by Liu & Fiala 1992 to have not occurred. I find: eclipse began near start of nautical twilight, magnitude r = 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 §G2 §G3 & 115 item [d]. Also, the evident lazeness of Babylonian regular adoption of the Metonic calendar; though, tracking the Metonic cycle is not the same as tracking the Saros. (Long-term repeat Metonic eclipse-nests occur only after twenty-four 19 cycles = 456)

109 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 trust here side with conservatism.

110 Despite Neugebauer’s intolerances, he had a becoming self-deprecatry side. (See Neugebauer 1975 pp.vii & 1-2. See also his final top protégé Swerdlow’s too-modest remark at DIO 4.3 113 [§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.” (See his relations with the Muffia & with Miss G.) So, for example, the ‘Muffia’ (the 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 seem familiar, but what we have attempted here. We have artfully ejected from small bits of evil the fence within 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 Thus Having’s Making the Multitudes Dance NYC 1993 p.350 attempts a hilariously anachronistic projection of the facing 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

11 I have found that the smallest 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:

\[ 9895^w = 10738^w + 5^\circ = 292205^{11}/4 \] (20)

12 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 eclipse cycle had been discovered by ancient scholars — 2000 years ago! To be specific: they certainly knew of it by the late 2nd century AD, and most probably already had it by 62 AD. (See also fn 110.) [One-fifth of eq. 20 is cited at Geminios 8:40-41.]

13 Unlike the 690 yr & 795 yr cycles discussed here previously (eqs. 8 & 10-11, respectively), the 800 yr cycle does not comprise an integral number of anomalistic returns; thus, its appearance is diffused on either side of the 800-mark. Since the discrepancies (vs. the exact 800 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 times:

\[ 743\text{ yr}, 762\text{ yr}, 781\text{ yr}, 800\text{ yr}, 819\text{ yr}, 838\text{ yr}, 857\text{ yr}, 876\text{ yr}, 895\text{ yr} \]

14 The earliest explicitly dated trio of eclipses whose records have come down to us is the Sargon-era Babylon threesome of –720-719, whose times were preserved for us by Ptolemy. The –719/3/8-9 midnight lunar eclipse observation was Ptolemy’s favorite 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.

15 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 (§11). This scholar (maybe Heron or, more likely, a contemporary) was preserved for us by Ptolemy. The –719/3/8-9 midnight lunar eclipse observation was Ptolemy’s favorite 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.

16 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. [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 §11) 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 (§144) was near dawn.

17 The –719/3/8-9 eclipse was certainly (Almajest 6.9) used by Hipparchos. Rawlins 1985S proposes that Aristarchos also used it.

18 Note the wisdom of van der Waerden 1963 p.277 on G.Cantor’s (familiarly Mufflo) historical naiveté, 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 in the star 49 Vir, whose latitude \( \beta = -3^\circ \), in order to found the equation: \( 9660^w = 781^y = 10483^w - 2^2 = 285265^1 1^b \) 

I6 Of course, it is possible that the relation was known even earlier, since 781\(^w\) pairs are common; 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 (\( \frac{3}{3} \) [4kIS]). \[b\] 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\(^1\) in longitude & ordmag 1\(^1\) in rt.asc), and the differential parallax was triling (fn 110). [d] The solar arc between mid-eclipses fell only 0.3\(^1\)3 short of precisely 281160\(^b\) or 781 sidereal revolutions.

I7 Eq. 21 produces a value for the sidereal year \( Y_\alpha \), implicit in PlanHyp, of very nearly:

\( Y_\alpha' = 9660M_A / 781 = 365^1 1/4 + 1/148 \) 

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

\( Y_\alpha' = 36000Y_\alpha' / 359990 = 365^1 1/4 + 1/147 \) 

A Mayan Table of Eclipses

by Hugh Thurston

A The Dresden Codex

A1 The Dresden codex (Dc) contains eight pages which have long been recognized as some kind of table of eclipses.

A2 This codex, which dates from fairly late in Maya history, probably not far from 1000 A.D., is one of the very few Maya documents to survive the ravages of time and the depredations of the Spaniards and the Catholic Church. It is painted, mostly in black and red, on tree-bark beaten as thin as paper.

A3 Each of the eight pages, which have been numbered 51 to 58 by modern historians, is divided into a top half and a bottom half. The tops of the first two pages contain an introduction; the table itself starts with the top of Dc page 53, which is followed by the tops in order and then the bottom halves. On page 32 of this paper is displayed Dc page 54 (taken, by high-contrast photography, from [1]); its top half is the second half-page of the table, its bottom half the tenth. You can see the whole table in [1].

B Interpretation

B1 It is the numbers that are important, not the text, which is concerned with mythology rather than astronomy, and does not explain the numbers. We have to do some detective work to interpret them.

B2 The Mayas used a dot to stand for 1, a bar for 5, and a shell for 0. So the combination of one bar plus two dots stands for 7. The units of time are:

\[
\begin{align*}
\text{uin}l &= 20 \text{ days} \\
\text{tu}n &= 360 \text{ days} \\
\text{katus} &= 20 \text{ turns} \\
\text{higher units} &= \text{we don’t need here}. \quad \text{So} \\
2 \text{katus} &= 0 \text{ turns} \\
3 \text{ uin}l &= 5 \text{ uin}l \\
5 \text{ days} &= 14 \text{ days} \\
\end{align*}
\]

amounts to 14465 days. In the codex the units are not written, so this time interval would appear as

\[
2 \quad 3 \quad 5
\]

(44 days, in a column).

\[ ^1 \text{Hugh Thurston has made his mark as mathematician, cryptographer, & skeptic. Bios of him are found at, e.g., DIO 4.2 and J.Hist.Astron 26.2.} \]
Eq. 21 provided ancients the number of sidereal months in 9660\(^a\); thus, combining this information with eq. 2 gives us the length of the PlanHyp implicit sidereal month:

\[
S_Y = \frac{9660M_A}{(9660 + 781)} = \frac{123234713}{4510512} = 27^d.32166836048
\]

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:

\[
S_I = \frac{1}{[1/M_A + 35999/(36000Y_1)]} = 27^d.32166858515
\]

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):

\[
35999^y = 36000^y
\]

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

\[
781^y + 7^o.4/5 = 9660^a
\]

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

\[
1^y = \frac{5796000^a}{468613}
\]

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:

\[
1^y = \frac{12^a + \frac{1}{3 - \frac{4}{1 + \frac{2}{1 + \frac{1}{448 - \frac{1}{7 - \frac{1}{8}}}}}}}{\phantom{448}}
\]

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 \(1^d/\)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 \(][10\) \([b\) (which converted eq. 21 into eq. 27) affected our math by barely 1 part in 30 million \((\text{see fn 113})\). (Mufosi 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\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):

\[ 19^9 = 235^a \]  
\[ (30) \]

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

\[ 852^3 = 105416^a \]  
\[ (31) \]

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 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 provides the ratio of the tropical yearlength to the synodic monthlength. (Another comment in passing: Mufiosis will reflexively attempt to ignore or\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 Mufiosis to ask: when is discovery so powerful and central that it forces re-evaluation of one’s perception of context? See, e.g., fn 137 & Rawlins 1991H.

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 (§16 item [a]). [c] And one of the two eclipses, on which we are proposing 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 the first Babylonian records (§11) 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 Almajest 4.9, a report of this very ss’ next-last eclipse, +125/4/5, \( m = 1.8 \) digits.)

\[ \text{References} \]


\textit{Almajest}. Compiled Ptolemy c.160 AD. Eds: Manitius 1912-3; Toomer 1984.


David Dicks 1994. DIO 4.1 11.


Hugh Thurston 1994E. DIO-J.HA 6 \$1 29.
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 sees us to a Babylonian-vs-Greek contrast that needs to be made explicit.

M2 This is a cult of modern scholars (Mufa&Gingerich) who impute serious sophistication to late Babylonian astronomy, and who have thus for decades incessantly hoped 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 Mufa catatonic horror, the only Babylonian parameter ever precisely connected to anybody’s empirical data is based entirely upon Greek observations! Mufa never has anything 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 Mufa viewpoints: e.g., §§5 & §2.)

M4 Two comments: [a] It is obvious that the Mufa’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 Mufa viewpoints: e.g., §§5 & §2.)

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

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135 See Rawlins 1991W §3E, 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 & —335 solstices of Meton & Hipparchos, resp. See Rawlins 1991H §A6 & eq.6) — and (despite attempts at accommodation &/or muddling, see fn 7) its acceptance by, e.g., van der Waerden (Rawlins 1991H fn 4), Moesgaard (DIO 2.1 §2 [D2]), Thurston 1994RE pp.123&128, & Dicks 1994 fn 37. See also fn m15&h16 here.

138 Needless to say, §H here is entirely speculative. But it’s hard granite compared to some of the Mufa’s gas about, e.g., Hipparchos’ nonexistent Babylonian confab. (See skeptical critiques by: Dicks 1994 §C2 & DIO 4.2 §9 [B2].)

139 Which explains the seemingly wild §134 DR prediction that the Mufa 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 and out of the academic establishment) accounts for this paper’s title — as does the Princetitute’s continuing effective-endorsement of Mufa arrogance, suppression, cult-fundamentalism, mismeath, & false slander (e.g., §B5, fn 8, & DIO 4.3 §15 [B3]). Other obviously-valid DIO findings Mufaists still noncite: fn 137, DIO 3 §5 [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, this would include putting Mufa at risk — which perhaps better testing the optimistic hypothesis that the rigid behavior of some young Mufaists is simply due to fear (DIO 4.3 §15 [G14]) of losing funding now controlled by petrified-bread archons.

141 As is clear from Rawlins 1991W fn 266, I am not trying to starve enemies. (That’s the Mufa’s tactic: ibid fn 16 & DIO 4.2 §7 [B3]). I hope that Mufa research will continue (fn 126) — but without the usual centralist (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, §3E, & Rawlins 1991W fn 170.

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J1 But, for reasons given previously, the Princetitute-Mufa-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 Mufaists continue to recognize the merit only of misarithmetic that confirms its own cult’s totally-flawless 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 Mufa-reaction-to-DR is a rigid proscription-prescription (fn 7).

J4 Reason: if a single R.Newton-DR discovery is admitted by Mufaists 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 (in 124) Mufa condemnation of RN-DR. Better to try pretending (despite multi-layer contextual consistency: §§15) 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 Mufa slander against important contributions to the very field where Mufaists pretend to ownership and exclusive expertise?) Note well the cult’s inflexible rule (fn 7): the Mufa 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 its 105416&8523 occurrence in the 1st place — but not just probably but certainly a fluke. (How else justify total nonacknowledgement of RN-DR contribution?)

J5 OK, it’s not that I have much condence that any PRIMO scholar possesses the intelligence (primarily a feel for probability) & 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 archondom’s antibody-rich ability to remain uniedly immune to evidence, logic, & equity — even for decades on end. (Mufa-vedt, DIO-noncitcating 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 soir´ees) with the most archons, is the one who’s right. No exceptions to this rule can be admitted without implicitly defiling archond majesty, most dangerously by creating impious inrmity (even skepticism) about whether academe REALLY NEEDS ARCHCONS. Remember On the Waterfront’s labor-gangster-boss, Johnny Friendly, reacting to the horror of just one person’s deance 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.”
K How Topo 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-Mufa scholarship can get. For a lesson from The Experts (who regularly declare that those who disagree with them are incompetents: §B5 & DIO 2.3 §E2), we turn to the paper of Mufa genii B.Goldstein (Mufa capo) & A.Bowen (Inst Res Class Philos & Sci, Princeton—publisher of Princetitude-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 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 & Hipparchos’ 600-Year Cycle”. Since Mufosi regard Hipparchos as “virtually a closet Babylonian in Greek drag” (Rawlins 1991W §E1), the 600’ period must (fn 27) have a Babylonian128 origin. (Origin has never added needing-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:

$$\sum = 12^{22}06^{1}3$$

G&B then round this to the unattested expression

$$\sum = 12^{22}06^{3}600$$

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”:

$$\sum = 7421^{10}$$

K3 The trifling inconvenience that 74210 is not an eclipse cycle is handled in the most artfully Muffioso fashion: it isn’t mentioned. (This, even though the Pliny 2.43-57 context is: eclipses.) Nor is 74210 an anomalous cycle. In fact, 74210 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,

$$\sum = 9895^{5}0$$; by pathetic contrast, 800’ = 74210 + 90’ and 600’ = 74210 - 13’. As for draconitic commensurabilities: 98950 = 107385 + 5’, but 74210 = 80533 + 86’.

128 Bowen 1995 takes it for granted that Geminus & Pliny (embph 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 Ibis readers will know that this presumptive (R.Newton 1991 §D14) evaluation is merely a [bedrock] fantasy (Rawlins 1991W §E4) of the Muffa cult — lacking the very “independent confirmation deriving from the times in question” which the same review (Bowen 1995) requires of non-Mufosis. 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). 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” attribution which lesser scholars lack — even though the 600 yr interval has long been rightly (DIO 1.3 fn 211) recognized by the Muffa’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 JHA) could the riseless 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 of Muffa’s work has given us some important reconstructions, e.g., DIO 1.3 fn 2778/280). 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., our 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.

K5 However, now that the Muffia has opted for its 600’ cycle, not for DR’s 800’ cycle, the former will be uniformly regarded as superior and will be the only one cited. Just modern academy’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 (already) reconstruction. (Similar attack by Bowen 1995 against Thurston: DIO 4.3 §15 [E3]. So, I leave it to Muffa lawyers to explain why G&B’s 600’ “cycle” isn’t thus also to be classed as Fiction — by their own on-again-off-again anti-reconstruction criterion.

L The Long View

Summing up the ancient-astronomy revelations of [A-S]:

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 19855 — about the Greeks’ discovery of lunar period-relations, is that the same anti-reconstruction criterion.

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] 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 during 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 assumed133 that Babylonian data were plentifully available. In Greece: we have the Thales 6th century BC legend; and astronomers Meton134 & Euktemon were already at work before 400 BC. Yet, Ptolemy

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 single common multiple occurs more often than the 13 triple.

133 The suggestion here (at [b]) is that he was not the only ancient (or modern) guilty of this oversight.

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

135 Another potential implication: little valid new lunar period-relation research occurred during 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 that Babylonian data were plentifully available. In Greece: we have the Thales 6th century BC legend; and astronomers Meton & Euktemon were already at work before 400 BC.