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Paul Forman (History of Physics, Smithsonian Institution): “DIO is delightful!”
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B. L. van der Waerden (world-renowned University of Zürich mathematician), on DIO’s demonstration that Babylonian tablet BM 55555 (100 BC) used Greek data: “marvellous.” (Explicitly due to this theory, BM 55555 has gone on permanent British Museum display.)
Rob’t Headland (Scott Polar Research Institute, Cambridge University): Byrd’s 1926 latitude-exaggeration has long been suspected, but DIO’s 1996 find “has clinched it.”
Hugh Thurston (MA, PhD mathematics, Cambridge University; author of highly acclaimed Early Astronomy, Springer-Verlag 1994): “DIO is fascinating. With . . . mathematical competence, . . . judicious historical perspective, [&] inductive ingenuity, . . . [DIO] has solved . . . problems in early astronomy that have resisted attack for centuries . . . .”
Annals of Science (1996 July), reviewing DIO vol.3 (Tycho star catalog): “a thorough work . . . extensive [least-squares] error analysis . . . demonstrates [Tycho star-position] accuracy . . . much better than is generally assumed . . . . excellent investigation”.
British Society for the History of Mathematics (Newsletter 1993 Spring): “fearless . . . [on] the operation of structures of [academic] power & influence . . . much recommended to [readers] bored with . . . the more prominent public journals, or open to the possibility of scholars being motivated by other considerations than the pursuit of objective truth.”
Note added 2013. In 1996, the article commencing on the opposite page conspicuously launched a test of Muffia-enforced omertà, 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/pri.htm#tq2s) 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 fn 6, should instead be to Enry Iggins (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 §02 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 correct edition.

1 At the time of her receipt (1995/5/6-7) 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 tyrannosaurian gentility, as happy proof that a hated general theory is entirely false and abhorrent.” Except for an obvious vulture analogy (DIO 1.2 †5 fn 14), 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 tyrannosaurian gentility, as happy proof that a hated general theory is entirely false and abhorrent.” Except for an obvious vulture analogy (DIO 1.2 †5 fn 14), this dumb error was snuffed partway during printing (1994/12/30-31); and the DIOs going to libraries at this time all received the correct edition. (Typically, B knew this without even checking.)

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 †. Though the several corrections entailed are 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.]
B Simpson as Near-Saviour: a Tragically-Wronged Hero

B1 So the truth is that, on the night of 1994/6/12, Kato — moonstruck 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 thighs of the flairer Nicole & Ron — while powerful OS got his left hand cut while trying heroically but vainly to defend them. (OS at least 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 Kato and the person Kato together? (As J.Cochran would say: think about it.) Is it a coincidence that ferally-smiling OS-houseguest Kato (whose hairdo is a hairdon’t) is THE shaggiest of all the characters in this hugely-casted drama? (Think about it.) And were-dogs are almost as shaggy as this story.

B3 Like were-wolves, were-dogs kill by fangning 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 wooofwoof 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 discerned 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.)

[c] Up to now, werewolves have had a better showbiz agent. (Until Kato’s hit the toob.)

B7 Final piece of the puzzle: at the time of the murders, the Moon was setting on the western horizon — just the celestial trigger for a were-dog strike. As my mentor Johnny C would say: this is all completely consistent. The sky don’t lie — you can’t deny.

6 Since were-dogs don’t right from wrong, punishing them would be senseless vengeance (DIO 4.2 §9 [F2]). OS matches the great SatNightLive civil rights pioneer, Emily Litella, in standing 4square against woof-abuse. SatNightLive

7 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.

8 Astrologers will note that the murders occurred not at lunisolar opposition (Full Moon) but at octile, which makes the were-dog all the more dangerous, because it seems as friendly as — well, as friendly as OSimpson.

9 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 Avirett 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-wind tunnel 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.)

10 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.

§1 Testing Princetitute-Muffia Omertà

by givin’ em a wee fit:

Equation 3.1

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! MuffieMythMirthBalls

by Rawlins

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

The conclusion4 of the Muffia’s late don-of-dons, Otto Neugebauer of the Princeton Institute for Advanced Study,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 ( & exploring the remarkable implications of) a burst of serious new findings regarding the empirical & math methods of ancient Greek astronomers. Nonetheless, those DIO-J.HA followers, looking for the dependably entertaining math-antics of their favorite modern imps, will not be disappointed by the Muffia-circle jollies presented below at DIO-J.HA 4.2 §7 B1, §7 B4, fn 14 [both Neugebauer 1975 §643.) Compare to fn 136.

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

3 The incomparable Ivy League “Muffia”, comprising some of academe’s most glamorous supermuddles, was introduced to DIO readers at DIO 1.1 §1 §§C5, C7, & C12. See fn 5. Also “Black Affidavit” at DIO 1.1 §10. And “Casting Pearls Before Pigaretts” at DIO 4.1 §4.


5 Due to its long association with O.Neugebauer, the Princeton Institute continues, uncomplainingly & unqualifiedly, to confer prestige and funding upon the skewed and-or outright-censorial output of the nest of Neugebauer-clonies whom DIO has reverently dubbed: the Muffia. See, e.g., fn 139, §M5, and Rawlins 1991W 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, typically-suppressive 1993 pre-publication advice from the unfalsifiable Muffia, warning against a Cambridge University-trained mathematician’s repeated citation (in a 1994 Springer book) of DR’s finding of the first evidence of Greek influence upon Babylonian astronomy (discovery now widely-accepted, despite the Muffia’s worst efforts):


**Gnawing Holes in the Dike**

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

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

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

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

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

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7 Throughout the present paper, the existence of high-level ancient Greek empirical science is repeatedly found to be consistent with our available evidence, including two ancient equations not previously solved: eqs. 10 & 31.

8 See below, fn 137.

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

10 For other examples, see, e.g., DIO’s series, Competence Held Hostage (DIO 4.1 onward).

11 See, e.g., DIO 4.3 §15 §§B18; also DIO 1.1 §1 §C7 & fn 20, and Rawlins 1991W §H2.

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**§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 ¶8, millions of OSimpson-trial-junkies take an ingenuous 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 unheralded in the standard media.

A2 Conventional analysts have needlessly made the OS case much too complicated.

A3 And, you want to talk LAPD cover-ups? According to a well-fueled Hungarian housekeeper (suspiciously vanished since), a cop on the scene said — and I quote verbatim — he was certain that this was "the unconscious, 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."

A4 The case will now solve itself, once we add-in some background: [a] OS’s lawyer first entered bigtime celebblood (this was even before he became a feminist publisher) when he got murderer Sam Sheppard off by inducing Sam to suddenly recall that his initial story (which the unanimously-guilty-voting jury hadn’t bought, in his 1st trial) was incomplete because — aw-shucks — he was altruistically holding back relevant information in order to protect another party. [b] OS, too, is a very selfless guy.

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1 One is tempted to suppose that the werewolf is a Hollywood creature, since there has only rarely been a female film-werewolf. (Though, see J.G.Melton Vampire Book [speaking of doppelfangers] Detroit 1994 p.33.)

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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 of 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 Playboyo-clown punishingly called Gallery.

4 Popular history (controlled largely by FLeaBee himself) has exonerated Sheppard, who was formerly the prominent police surgeon of Bay Village (Cleveland OH suburb). His case has similarities to OS’ (even beyond FLB’s involvement): rich client, “whopping” legal fee (FLBailey 1971 pt.2 ch.2), nasty marriage (Sheppard-divorce 1st discussed only 3 weeks before murder: pt.2 ch.1), suspect’s infidelity, no time-alibi when wife killed, didn’t kill her husband, & wet with the victims’ fresh blood. (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 unheralded in the standard media.

5 Despite the Muffia’s worst efforts:

6 Despite the Muffia’s worst efforts:

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7 One is tempted to suppose that the werewolf is a Hollywood creature, since there has only rarely been a female film-werewolf. (Though, see J.G.Melton Vampire Book [speaking of doppelfangers] Detroit 1994 p.33.) 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.)

8 But this apparent gender-bias may instead be merely an unintended side-effect of the US’ lucrative diabolical industry’s fashst veto-power over female imagery in films: after all, how salaciously-charismatic would the public find a film werewolfless (Mad Magazine, are you ready for this?) — entirely covered with hair, excepting legs and armpits?

9 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 of 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."

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10 See, e.g., DIO 4.3 §15 §§B18; also DIO 1.1 §1 §C7 & fn 20, and Rawlins 1991W §H2.
C7 Defense attorney Rob't Blasier (1995/10/3), alibing his huge foe: “People who don’t have alof money aren’t going to get the same kind of justice. [But it’s] not a matter of [Simpson] buying justice, it’s a matter of everybody else should be entitled to this as well.” Great. The case’s sum legal cost (both sides) was roughly $15,000,000. So, with over 20,000 US murders/year, the annual legal fees (for trying all of these cases with the care Blasier insists upon) will total about 1/3 of a trillion dollars. Adding in the cost of trying rape & assault cases, we’re talking about spending the nation’s entire annual budget (ordmag $1,000,000,000,000) just for paying lawyers to entertain us with the latest state-of-the-artful tricks in jury-chiseling and evidence-juggling. Ad infinitum. And, better yet: no one will ever again be convicted of anything. Unless he’s unattractive. (See DIO 4.2 §9 [F2] Look on it as a revolutionary national beautification project.

C8 The above-cited (§B5) ABC-TV ‘snews poll reported that, of its 19 sample of blacks, 18% thought OS guilty.20 But only 64% thought that the police planted evidence to set OS up for crucifixion. Thus, since 18% + 64% = 82%, we find that the remaining 18% (nearly 1/5) of blacks believe that OS was innocent but not framed. (For whites, the equation is 70% + 26% = 94%, so 6% agree.) Understand, for these splatter-defense-zapped minds (18% black, 6% white), blood with OS’ DNA was all over the place, but it wandered there without being planted by the cops.

C9 Clinton near-appointee Lani Guinier21 (torn between her black and feminist lobby contacts): “I think what we saw in [the Simpson verdict] is that there are many truths. And we have to be in a position to try to hold in our minds all of those truths, and to work with all of those truths so that we can understand that the jury was dealing with the facts as they saw it [sic] from their experience and that their experience is a legitimate experience.22 It is part of the American experience. And we have to as a society move beyond the racial divide and the racialized way in which blacks are experiencing the criminal justice system.” Though such a bizarre defense of mass-insanity will justly enrage rational feminists, it may well be that Guinier’s dementia has a debt to the nut-wing of (not only those who pander to black paranoia but also) the gender feminists.23 Anyone who has experience with cults (certain religions, the “paradigm”-obsessed Hist.sci clique, or the shrinks) is familiar with Guinier’s ploy: no one can be judged, because [a] there is no objective truth,24 and-or [b] one must be a member of a cult to criticize it.25 This is horrid philosophy of science. But, as bloc-political-pandering, it is so ethereally pure — that one can see right through it.

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Summary

We find that ancient Greek astronomers, by using eclipse cycles about 7 to 8 centuries long (eqs. 8, 19, & 31), established-confirmed the sidereal, synodic, & anomalistic months to an accuracy of about 1 timesecond or better. The Babylonian part of the empirical base of this Greek math is shown (eq. 11 & §B5) ABC-TV ‘snews’ reported that, of its 19 sample of blacks, 18% thought OS guilty.20 But only 64% thought that the police planted evidence to set OS up for crucifixion. Thus, since 18% + 64% = 82%, we find that the remaining 18% (nearly 1/5) of blacks believe that OS was innocent but not framed. (For whites, the equation is 70% + 26% = 94%, so 6% agree.) Understand, for these splatter-defense-zapped minds (18% black, 6% white), blood with OS’ DNA was all over the place, but it wandered there without being planted by the cops.

A Hipparchos’ Most Reliable Eclipse-Interval

A1 It is well-known that, for analyzing the synodic & anomalistic motions of the Moon, Hipparchos’ basic empirical relation was the neat 345 yr cycle (Almajest 21 (torn between her black and feminist lobby contacts): “I think what we saw in [the Simpson verdict] is that there are many truths. And many of those truths, and to work with all of those truths so that we can understand that the jury was dealing with the facts as they saw it [sic] from their experience and that their experience is a legitimate experience.22 It is part of the American experience. And we have to as a society move beyond the racial divide and the racialized way in which blacks are experiencing the criminal justice system.” Though such a bizarre defense of mass-insanity will justly enrage rational feminists, it may well be that Guinier’s dementia has a debt to the nut-wing of (not only those who pander to black paranoia but also) the gender feminists.23 Anyone who has experience with cults (certain religions, the “paradigm”-obsessed Hist.sci clique, or the shrinks) is familiar with Guinier’s ploy: no one can be judged, because [a] there is no objective truth,24 and-or [b] one must be a member of a cult to criticize it.25 This is horrid philosophy of science. But, as bloc-political-pandering, it is so ethereally pure — that one can see right through it.

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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 ‘snews’ 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.


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


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24 In this paper, I use the word “saros” to signify the interval of eq. 14; and “saros series” is abbreviated “ss”.

25 Several equations in this paper relate successive quantities (e.g., synodic months, anomalistic months, draconitic months, anomalistic years, & days), using serial equals-signs. It should be stated explicitly that, in each such serial equation, all quantities (past the first) are computed directly from the number of synodic months. E.g., in eq. 11, 290315d07h is found by multiplying U = 29d530959 (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 — 323, in solar days of that epoch) by 9831 — not, e.g., by multiplying the length of the anomalistic year by (795 — 323, in solar days of that epoch) by 9831 — not, e.g., by multiplying the length of the anomalistic year by (795 — 323, in solar days of that epoch) by 9831 — not, e.g., by multiplying the length of the anomalistic year by (795 — 323, in solar days of that epoch) by 9831 — not, e.g., by multiplying the length of the anomalistic year by (795 — 323, in solar days of that epoch) by 9831 — not, e.g., by multiplying the length of the anomalistic year by (795 — 323, in solar days of that epoch) by 9831 — not, e.g., by multiplying the length of the anomalistic year by (795 — 323, in solar days of that epoch) by 9831 — not, e.g., by multiplying the length of the anomalistic year by (795 — 323, in solar days of that epoch) by


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27 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 §5 traced to Hipparchos Babylon’s “System B” yearlength on one of the major cuneiform tablets containing eq. 2, and Rawlins 1985S showed how inclusion of ancient rounding (during the math descent) indeed could have permitted eq. 2 to have been derived from eq. 1, just as Ptolemy said.

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

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

\[ 251^\circ = 269^\circ \]

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.15

B2 At length, Yale University’s A.Aaboe 1955 (pp.123-124) made the clever16 discovery that the −7°1/2 remainder in eq. 1 could almost be explained by assuming use of the

B3 15 See fn 137. It is an indication of how highly Hipparchos was regarded in antiquity that his −13° S.Sostice had become internationally accepted on a level with Meton’s revered −431 observation.

B4 16 Even the Muffia’s 1st pope accepts this yearlength part of System B (Neugebauer 1955 p.200), which thus independently supports DR’s suggestion that Babylonian astronomers’ System B was derived from Hellenic science: fn 15 & Rawlins 1985S.

B5 The −620/4/22 eclipse is one of only four pre-600 BC eclipses preserved by Ptolemy (Almajest 5.14). The middle of the −275/4/18 total eclipse was below the horizon at Alexandria, but the eclipse started about 18° above the horizon at 1 AM local Mean Time there. Just adding a rough total eclipse semi-duration of 110° or 120° would then provide the correct time of mid-eclipse to within about 10°. (And reports from sites further west could have improved the firmness of the mid-eclipse time.)

B6 Hipparchos almost certainly used the attested 345yr-period: −490/4/25-26 (Almajest 4.9) & −1459/4/21-22 (Almajest 3.1). Though eq. 3 may already have been known, the stability of eq. 1’s 345 yr time-interval would show its superiority vs. other multiples (fn 19 of eq. 3). This consideration reminds us that no one eclipse-pair could establish eq. 1; either trios were used at both ends (to establish anomalies), or (far more likely in my opinion) examination of several 345 yr-pairs revealed the striking fact that eq. 1’s interval (126007/318h) was virtually identical for all such pairs, observably varying less than an hour (fn 56): an ordmag 1-in-10 million constancy that is very probably (though see fn 30 the direct, convenient basis of the even more accurate eq. 2. Curiously, it has been little noted (probably due to Ptolemy’s unparalleled learning for non-period-return “proofs”: fn 51) that the astonishing constancy (fn 56) of eq. 1’s 345 yr interval was, historically, the critical, uneventually plausible (not theoretical) empirical evidence for the secular stability of lunar mean motions. The fact that lunar periods were remarkably (§A6) constant (despite the seeming untamability of confusing short-term lunar wanderings) is one of the most important of all ancient (or modern) astronomical discoveries.

B7 Eq. 3 (sometimes credited to Kidenas = Kidinum: see van der Waerden 1974 p.240 & Neugebauer 1975 p.611 n.36) was probably suspected long before eq. 1. (However, see fn 18.) Note that eclipses can occur for much shorter multiples of eq. 3 than 17, namely for 4 & 9 repeats. Moreover, there is much better draconitic commensurability (than for eq. 1) at 13 repeats (3263), though solar anomalistic commensurability is ordmag 10 times worse, since 3263 equals about 263° – 60°. Finally, I note that 22 repeats is 5522° = 5018° = 5990° 1/2 – 17° 446° 20° – 20°. (So Aristarchos might have been able to use the −719/9/1-2 and −272/15-16 eclipses as a rough foundation for eq. 3 — if he could correct for solar anomaly differentially.) But this is not as helpful as a relation like eq. 1.

B8 At least since Manitius 1912-3 1:196 note b. Taking Phil 1 as epoch: for 4267 real synodic months, eq. 1’s solar remainder would have been −6°1/3 for the real sidereal year, −1° 6 for the real tropical year, and −3° for the Hipparchos-Ptolemy year.

B9 DR has long admired Aaboe’s intellectual gifts. See, e.g., DIO 4.3 §15 fn 8 & Rawlins 1987 n.35. On 1994/5/8, Aaboe perceptively noted (at the Dibner Inst conference at MIT) that a nice way of finding Hipparchos’ Autumn & Winter seasonlengths (841/8 & 919/12, resp.) is by subtracting Spring & Summer seasonlengths 941/12 & 921/12, resp) from (3651/4/2). This is quicker than Ptolemy’s Almajest 3.4 math, & the results are identical. (See also fn 44.)

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 1994/9 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 on top). “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 it took him a month to mount a campaign to hunt for the killers of his children’s mother — but that even so . . . if he was on the jury, 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 (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 spectacle would rightly be called racist. Question: why does racism submerge feminism?) A few days later (10/31), she said she was sure of his innocence: “No doubt in my mind.” So I asked how OS got cuts on his hands. Entire reply: “Who cares?” [A colleague’s version, 1995/11/21: “So what?”] Over 80% of blacks agree. Such is the state of black epistemology — even after decades of persistent US gov’t uplift, integration, & preference programs.

15 E.g., I believe that there are Muffiaos who are quietly embarrassed by their own cult’s misbehavior (11 fn 3&5), but I don’t expect public confirmation of that DR optimism.

16 Double-usage potential jurors’ handwriting (Neuwirth 1995/10/30 p.84) when selecting likely 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-filters systematically dumbf'y US jurors?)

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 wakeup-shock to even the densest traditional crusty-righteous Lib. It may assume the same rôlle which N.Chamberlain’s 1938 peace-in-our-time naiveté served, for 1938-1940 observers (who might have blamed England for war, had hostilities started in 1938): when give ‘em-enough-ropes gentility goes far beyond rationality, it can ultimately help enlighten the very slowest learners.
in the 3 digit range? (Thanks to an earlier generation of Baileys & Cochran, one couldn’t today convict even JFK’s killer, L.Oswald.)

[b] Most US inner city blacks have relatives who’ve been arrested, not imprisoned police.

[On the other side: many have relatives who’ve been killed.]

[c] If we discount the influence of OS-team money corrupting the press, then: just for entertainment (profit) purposes, the media fanned more public paranoia10 than any force since the Third Reich. Those selling media ads wanted (as for the previous sports-brutality soap-saga: Tonya-Nancy) to spur ratings with sports-biz-style gotta-watch cliffhanger-ness. (I.e., generate “controversy” regardless of truth.) But only paranoid theories could create an “other side” in the OS case. So, the media regularly paraded before the public a succession of strange and ghastly explanations, the most ingeniously deceitful springing from writings of years of evolution. All these chaps were introduced as experts, without mentioning that their top expertise is deception, which is what they do for a living. (But: do we expect moral reversion from the very TV-ad-exec who demanded an OS clifflanger?)

[d] Given the kinds of politicians US whites elect, they are in a weak position to condemn blacks who fail for plausible con-men like OS’ chief-lawyer Johnny Cochran, who clearly deserves rapid elevation to: Congressman JC. If one believes a 1995/103 ABC-TV ‘new poll, only 70% (+4%) of whites think OS guilty. (This in a case in which the evidence is what Manson-prosecutor V.Bugliosi rightly called a prosecutor’s dream. Again, one wonders: is the remaining 30% any relation to the [data-mass-discussing & nit-obssessing] paranoid bunch who believe that WW2 Germany didn’t systematically kill millions of Jews?)

Taking this number to the 122 power, we find that the odds of picking a unanimous guilty jury, from whites, would be barely 1/1036.42 billion. The chance of such a dozen in the black community is found by taking 17% or 18% (§5)12 to the 122 power: 1-in-a-billion. Yet despite this, I hoped12 that my long discouragement at black culture’s mean state13 would be spectacularly disconfirmed by a perceptive verdict. (I delight in stereotype-breakers: DIO 4.3 §13 [B4]. And I empathize with the Goldman family, who — while so many were treating wifebeater & sneak-slasher OS as a hero — were mourning

10 Cult-rejection of reasonable police testimony has a long history. E.g., the Jerusalem police reported that Jesus’ body was not resurrected but was stolen by his disciples. (Christians, applying their usual delicacy in evidential discrimination, prefer to believe instead that: god suspended the laws of nature and whiffed life into a stiff.) One of Jesus’ rich backers (Matt 27:57-60) had already persuaded the body out of the grasp of Pilate (who earlier had tried to ploy-intervene in Jesus’ behalf: Matt 27:17f). The tomb was hermetically sealed (à la J.Carson’s Karnak) with a huge stone (ibid 66). And, well, yes, somebody later moved the stone & sat on it — but that’s OK because he said he was an angel: Matt 28.2. [If an Academy-Award-night envelope is ever, in mid-ceremony, discovered to be unsealed, I suspect the emcee just sit on the envelope & announce that no one should worry about tampering because he’s an angel!] Many of the very US establishmentarians, who privately realize blacks are fantasizing alibis for OS, will swear that the ancient Roman police faked their rational explanation of Christians’ Easter “miracle”. (Matt 28:11-15 says Jewish priests bribed the military police. If the Bible is the word-of-god, then god says those cops were Jewish-conspiracy-paid liars. . . just another lesson in the reliability of legend, sacred writ, & popular wisdom.)

See DIO 2.7 §1 [A4].

12 I did better in the S.Mith case, where (before suspicions were made public) I argued with high skepticism about her convenient story that a black kidnapper had stolen her children.

13 Mass black paranoia won’t die just because race-haters are cleared out of the L.A.P.D. [The bizarre fixation of the media & the film industry] upon the ordinary-people-eating-plant — rather than the pathology of inner city neighborhoods — is a deeply, life-threatening error. (Note: if the US legal system’s revolving-door-courtroom turns police work into perpetual ploughing-in-the-sea frustration (like the Korean War, which was actually called a “police action” at the time) and if lawer-protected mobster-druglords corrupt the police, then: why be surprised if many idealist cops depart — and the L.A.P.D.’s alloys are remained with time-servers & sadists? Moreover, since the lawyers are helping to cause this trend, it ill becomes them to attack the police as immoral. (Indeed, for US defense lawyers to criticize anyone for immorality is the height of comedy.) Further, given spectacular nationwide black crime rates: attempting to exumate statistical race-postjudice in cops is akin to asking monks to reject what their eyes (& a 1st-grade counting ability) tell them. (Though, this is no excuse for damning whole groups or treating individuals unfairly.) The press is applying the until-you’ve-walked-in-my-shoes-to-logic to all groups except cops. (Since TV ‘news has already clarified our thought by identifying the police as the chief curse of inner cities, it follows that the terrible “random” shootings occurring there must be by invisible cops — who are taking recreational time off from yawner-routine evidence-plantings, in order to enjoy the tension-relieving spice-enhancement of race-hate-target-practice.)

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fundamental22 Babylonian ratio of yearlength23 to monthlength:

$$1^y = 122^w = 08^o$$ (4)

Almost.

B3 The charmingly plausible Aaboe, an enormous mathematical talent, has since become the venerable loving-papa, guru-mentor, & shepherd of current lamb-brained Muffiadum: an above-it-all, Princely architect of its public-nonconnt-cum-private-slander strategy24 towards heretics. For openmindedness on central sacred tenets, Aaboe can match the real pope

B4 Through the very same forced-math approach previously perfected25 by his own mentor, Princeton Institute’s Otto Neugebauer, Aaboe 1955 calculated sexagesimally (using eqs. 1&4) as follows:

$$2676/(122208^w/1^y) = 345^y 58^z 42^u \ldots = 345^y - 0^v 01^t 17^s \ldots = 345^y - 7^o 42^r$$ (5)

and he then rounded the remainder to the nearest 1/2 to find −7/12. 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 (Ô cover for) Aaboe’s delusion. (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 18”, not 17”. Simple subtraction. (See Rawlins 1991W §9 comments on: [a] Aaboe-prot´eg´e N.Swerdlow’s sneers at E.Rosen’s errors of arithmetic; and [b] the glass-house irony of stoner-age Muffles. See also fn 28 on Muffia outrage at noncultural’s alleged academic dishonesty!) Why do Muffiosi have such a magic-touch penant26 for this sort of thing? (Note that MacArthur Fellow [see DIO 4.3 §15 fn 24] Swerdlow’s most polished forced-math gem, cited at DIO 1.1 §5 fn 7, debuted in a thesis heavily assisted by Aaboe. The world loves a quick learner.) Answer: some moderns (§5B, §2K, & fn 128) are as determined to find Babylonian influence27 in Greek astronomy as their hero C.Ploymy 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 astroligators who used the equation. Indeed, we don’t even know if they knew [or cared] what kind of year the equation involved.

24 All right, so it doesn’t take a Klausswitz to anticipate that academic’s vaunted archons will do absolutely nothing to curtail such medieval, reason-evading behavior. (When arrogant academic cults suppress evidence-based discussion, they leave no rational avenue for idea-evolution. So, unsurprisingly, the banned parties occasionally react against emus. E.g., DIO 4.12 §1 fn 28)

25 See the three forced-math entries under Neugebauer in “Casting Pearls Before Pyglets”, DIO 4.1 J (“Muffia Catalog: the Incompetence-Chargers’ Competence”). But note also his distancing himself (see §B7 & end of fn 29) from Aaboe’s egregious fn 5 sleight.

For catalogs of other entertaining instances of Muffia (& Princeton Inst) fudge, see “Black Affair!” at DIO 1.3 §10, and “Casting Pearls Before Pyglets” at DIO 4.1 J.4. (Note that the Princeton Inst is named for the town and has no more official relation to Princeton University than the Stanford Research Institute has to Stanford University.) Mean- time, Princettute-supported scholar-scholarship continues (e.g., Britten 1992 p.xvii to bluff-suggest that dissenters’ work is insidiously-riddled — but, when challenged (Rawlins 1991W fn 252) to reveal the purportedly enormous List-Here-in-My-Hand of actual heretics’ errors, Muffiosi have for years stayed as secretive as Joe McCarthy.

27 Question: has a single case of unattributed Greek use of Babylonian astronomy ever been established? (Why would Greeks even bother to hide their use of such elementary work? — which was hardly worth stealing.) We know of use of Babylonian material in the Almagest (see, e.g., Dicks 1994 §D1); and of Babylonian math in the lesser work of Geminius 18.9. See also Neugebauer 1975 §601. But all of this is openly cited. So, why do Muffiosi assume that large unattributed borrowings went on? For the hyperwissyp, sometimes miscontrued nature of the few alleged Muffia proofs of said borrowings, see, e.g., §5B and Rawlins 1991W §§D2-G9 & fn 73, and below here at §J. (Note contrast to DR’s in 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. (Principitied-fund Corrected Britton 1992 repeatedly loyalizes the Muffia’s: [a] attack on RN’s honesty [p.xvi] & [b] the cult-spectulation-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 (1976/399) called R.Newton’s research: “incompetent work in my realm.” (See DIO 4.3 §15 G9.) For the Muffia’s own dazzling competence here (which accounts for this paper’s J.Hyster.Astron co-header), see fn 1.

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

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 $\pm 7^\circ 46'$ to $\pm 7^\circ 1/2$ is nothing much, alleging that Ptolemy reported (Almajest 4.2) that Hipparchos “rounded this deficit to” $\pm 7^\circ 1/2$. In fact, Ptolemy nowhere states that Hipparchos rounded anything here.

C Old Question: Why Is Eq.1’s $\pm 7^\circ 1/2$ Remainder Incorrect? New Answer: It Isn’t.

On 1995/4/23 (even while editing this paper), I independently came upon an explanation of the $\pm 7^\circ 1/2$ 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 geocentrics, 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 fnm 19 &57.)

It seems that the solar apogee A was placed near 60° (fn 44) by Kalippos (330 BC, when the correct A was 63°), though (ibid fn 190) the mature Hipparchos put it at 65° and later at 67° (146 BC & 128 BC, resp, when the real A = 66°1/2: Rawlins 1991H §C8). In the recent excellent11 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 (1st century BC). (The author28 & DR agree that 66° could well be a just convenient rounding of 65° or Ptolemy’s 65°1/2. But it is also possible that this is one of a series of successively increasing values ancient astronomers used for A over the centuries.)

28 E.g., Rawlins 1991W fn 252 & DIO 1.1 §3 [D3].
29 Neugebauer 1975 p.312 seeks solace by citing an attribution of $-8^\circ$ to Hipparchos in an unreliable (ibid p.310 n.6) late Greek source. But $-8^\circ$ is a rounding of $-7^\circ 1/2$, not vice-versa. And this does not explain the Hipparchos version reported at Almajest 4.2. However, considering his own obsession with tying Greek work to Babylonian (Rawlins 1991W fn 73), ON deserves credit for showing (Neugebauer 1975 p.298) that the explanation for eq. 1’s $\pm 7^\circ 1/2$ remainder could be Greek.
30 Best ordmage 1000° synodic-anomalistic cycle: $16092^\circ = 17246^\circ = 17463^\circ - 9^\circ = 1301^\circ + 3^\circ = 475260d08h$ (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 [1]II&He 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. B) averaging several 345° pair-intervals would (fn 18) produce comparable accuracy.
31 I particularly admire van Dalen’s exemplary perfectionism, which his Table 4 (p.131) typifies — and which is directly responsible for ultimately producing a perfect reconstruction of a complex ancient procedure. However, the paper’s credits at p.149 n.1 exhibit several problems, which I have informed him of, without reply. (See also DIO 4.3 §14 fn 4.)

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converges on him, but: the very instant he puts a gun to his head, the genius L.A. cops (DIO 1.2 §H2) cringe and snap into reverse with the trampoline-elasticity of cross-zapped vampires. (If you see Othello as a tragedy, then think on this: Cleavon Little did not live to see 1994/6/17’s impromptu Blazing Saddles remake.)

A5 By contrast, my wife took one glance at the televised Bronco&retinue Simpson-cavalcade and left the room, declaring (in justifiable disgust at the decline of the theatre in our age): heeeee’s not gonna kill himself. . . .

B This Year’s Trial-of-the-Century Ends

B1 Going easy on criminal celebrities is nothing new and not restricted to any race, as we know from Roman history (Horatius, Scipio, Barabbas) or white US history (Nixon). But OS (who sprinkled his blood & bloody clothes around his victims, car, & house almost as freely as S.Smith sprinkled tears around S.Carolina) may have attained a rare superlativeness, namely: the clumsiest torture-murderer ever to be voted innocent. (Thanks to the master-truth-shaders of the US lawyer-queue — and its transformation of justice-seeking from a noble profession into a dark art.) I.e., OS’ once-lovably-inert “Nordberg” character (in the Naked Gun film series) came to sinister, amoral life.

B2 In response, one hears mostly talk of continuing on, fumbling&tampering with the traditional jury&court system. My own contribution in this direction — a semi-facetious hang-by-the-jury-untilyou-are-dead scheme (DIO 4.2 §9 III) — in truth was as shortsighted ( & as subject to arbitrary abuse) as anything it might seem to correct.

B3 If OSimpson’s DreamUp-Team’s defense cost him λ millions, but (now-ended) alimony to his ex-rated wife Nicole was costing him α millions/year, then simple division suggests that wife-murder may pay: he’ll break even in λ/α years, and will be fiscally ahead of the game from then on.

B4 Some justly suggest that the OS case was lost at the jury-selection stage. One may ask: why (fear of offending political blocs?) did the Los Angeles DA (G.Pontius Garcetti) lay the basis for eventual acquittal by [a] oversiftly forswearing the biggest stick (seeking death penalty) for inducing a confession-deal, [b] moving the venue from OS’ neighborhood (to an area with lots of OS support), and [c] not using jury-profiling? And why has Garcetti (1995/103) gratuitously provided a post-verdict federal civil rights indictment of OS?

B5 Given mass black joy at the verdict ( & the jury’s 75% black composition), the race angle has correctly been given alot of play in this case. But it can be interpreted in several ways. (And there are other, non-race-related points to consider.)

[a] Some jurors may have been less bigotted than simply (for whatever reason) mentally unqualified. A juror — as humorless as mirrorless — put it very well (while abusively & unselfconsciously attacking the ... “garbage in, garbage out.” Does anyone seriously believe that mean US juror IQ (white or black or whatever) is
§4 OJ Darts & Nordberg Walks

A Is-That-a-Promise? The Bacon-Brooks Hypothesis

Whites-Ain’t-So-Smart-Either (Part 4. Continuing a series begun at DIO 4.2 9 [R].)
[And mostly-white — part American Indian1 — DR also ain’t so smart: §C5.]

A1 Why (other than to help enrich lawyers instead of undertakers) do US officials put suicide-watches on murderers, such as Susan Smith2 (the South’s answer to drought) and OSimpson’s H. Corp. boardperson & NBC-TV commenter?

A2 In Othello 5.2: blonde, innocent Desdemona has been murdered minutes earlier by the obsessively jealous Moor Othello, who saves the Venetian state the bother of an execution by stabbing himself to death. Othello’s “arithmetician” Florentine lieutenant, Cassio, is illogically shocked by this, albining his own failure to prevent the suicide by protesting in anguish, “This did I fear, but thought he had no weapon.” (Emph added) C3 Likewise, one recalls the outlandish M.Brooks fantasy scene in his 1973 film, Blazing Saddles, as a 19th century redneck town’s unexpectedly-black newly-arrived sheriff, Cleavon Little, moths the brown wilderness preaching by suddenly grabbing, “This is it — put it to his head & counter—threatening to: do exactly what the crowd had wanted him do, namely, kill Cleavon Little. The crowd shrinks back: Careful, men — he means it. . . . (Cleavon after escape: boy, are these whites DUUUMBB.) Question: if Shakespeare was really F.Bacon, then: was F.Bacon really Mel Brooks?

A4 When audiences first encountered the Mel Brooks DUUUMBB scene, all thought it hilarious precisely because it was so wildly nutty — so outrageously, utterly impossible in the real world. Fast-forward to 1994/6/17, as womanbeater Othello J. Simpson darts for freedom after murdering his Dezzzy-blonde exwife (6/12); but, hark, he’s spotted — and is swiftly surrounded by most of the population of L.A. (Largely cheering the murderer.3 Gosh, I can’t imagine why L.A. is suffering a middle-class exodus.) A fleet of police-cars

[1] DR does not use the term “Native American” (since we Indians are not the only persons born in the Americas). For similarly misguided racism, we turn to the notorious G.Elias case, where a heavily “African-American” Baltimore court committed a judicial outrage against Dr. Elias, a decent, brilliant U.Md Hospital oncologist who was evidently perceived as part of the “white” satanic Enemy. Actually, the only true “African-American” in the entire court was Elias himself. (He is a Cairo born Egyptian.) Elias is a protégé of one of the finest men I’ve ever known, Geo Yeger, who was a close friend of my late father (Low Rawlins) from 1931. George (who reached age 90 on 1995/10/19, still as wise and cheery as ever) has been tops in his field as: surgeon, teacher, researcher, editor, architect, & administrator — a combination of high skills which will never again occur in the history of medicine. (George successively headed the UMD & Union Memorial hospitals.) He is probably the only living 3rd generation professional descendant of the original Johns Hopkins Hospital’s now-immortal Four: Halsted, Kelly, Osler, Welch. George’s mentor, Harvey Stone (a pupil of Halsted), was also the father of his dear wife, the gifted artist Dorothy Stone Yeger.

[2] Smith is the me-first-social-climbing S.Carolina yuppy who drowned her innocent, terrified, strapped-in children in her car — and then tried to drown the rest of the neighborhood in the bathtub. My wife Barbara suggested right after the S, Smith confession that the US has become so lawyered with alibi-artistry that Smith might try excusing her deed on the Grouchouesque ground that she didn’t want her kids to grow up in a world containing her. As with the OSimpson LAPD replay of M.Brooks’ Blazing Saddles nut-fantasy (§A3), it is no longer possible to satirize the dramas of modern crime: just a couple of weeks after B’s ironic comment, we read (Washington Times 1994/11/26 p.A7) of a Smith-copycat — a debt-saddled, alimony-terrified Japanese physician — who (10/29) murdered his divorcing wife and then killed their two innocent children as well, (as recounted by police): “Their future would be without no mother and a father who was a criminal.”

[3] Dr. Tom Linden, psychiatrist, commented live (1994/6/17), on national TV’s news, regarding OSimpson’s suicide threat [emph in orig]: “He has to be taken very VERY seriously.” (Those addicted to shorelineanalysis pay fortunes to receive the wisdom of such experts.)

[4] Another prosecution expended ordmag $10,000,000 & tens of thousands of pages of record, reconstructing a massively detailed trail of OS sanguine droppings, and spending thousands of hours of finely-tuned reasoning & inductive logic, what had changed? OS’ fans were cheering as lustily as ever. And the jury’s response (like fn 14) was a cursory show-it. (After encountering similar nat-logic from Ivy League Mulfossi, in reaction to similarly motived DR’s assertion above, now more aware than most that cultist prejudice’s damage to clear thought is a problem not restricted to ethnic ghettos.)

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 Kalippus and of Hipparchos supports the general thesis that there was. And their contemporaries may have been more accurate yet: §C3. Note: since geocentrists’ large (conveniently Sun-shrinking: Rawlins 9191W fn 280 & §R14) parallax-guessestimates would degrade the accuracy of apogee-determination, it is reasonable34 to ascribe to ancient heliocentrists the here-proposed discovery of correct solar apogee-precession.

C4 The fact that no (surviving) ancient astronomical text mentions this is not critical, as the case of the Earth’s obliquity shows: [a] The obliquity (23°/3°) used by genuine ancient astronomers is nowhere directly attested in extant works. (See Table 1 of Competence Held Hostage 82 at DIO 4.2 p.56.) [b] The fact that the obliquity was gradually decreasing is also not found in any surviving ancient astronomy text. Yet the accuracy of values used by Hipparchos (& perhaps earlier by Eudoxos)34 suggests that ancient scientists could have recognized the obliquity’s decline. And, at Plutarch Moralia 411A, we find an explicit statement (though in a strange context) that the obliquity was decreasing.

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

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

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

C8 We do not know.36 But, fortunately, we do not need to know, because: the stars have nothing to do with eclipse periods. (The stars could all be tripping the trepidation 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, $453 \approx 345° − 7°/2$ — be unacceptably hybrid & inconsistent if it used anomalistic returns for the Moon, but not for the Sun?

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

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

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

[36] Neugebauer 1975 pp.293&298 suggests 2 different possible values for Hipparchos’ sid yr: 365d1/4 + 1/144 & 365d1/4 + 1/100, adding that Hipparchos may’ve believed the sid yr inconstant. [ON who they others, “Hipparchos” commits 2 sins at ibid p.1083: [a] Giving the modern (not ancient: fn 38) anoy yr. [b] Rounding this AENA 1900 value, 365d.2584134 (≈ 365d1/4 + 1/104), decimally to 365d.2586 & then expressing it sexagesimally as 365d15′34″′33′′′′ + 1′00000 times more precise than accurate. Same muffs (idem) for both tropoksid years: p.1084 exhibits similar (less severe) oddities for lunar periods, plus misprinting 16°8′ as 16°18′ in 2nd anm expression.]
Thus, let us test quantitatively the hypothesis that the ancients knew the Sun’s anomalistic motion; we start by proposing an accurate value $G_s$ for the solar anomalistic year, rounded in typical ancient fashion:

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

(6)

and divide it into eq. 1:

$$4267 M/A / G_s = 345^\circ - 7^\circ 33^\prime -$$

(7)

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

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

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

Thus, two elementary considerations recommend our speculations 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 $365^{\circ} + 1/102 (23)$: Rawlins 2018U K[5].]

And this adds more credibly to the position that the famous geocentrist astronomers, Hipparchos & Ptolemy, drew much of their astronomy from often-unnamed but able astronomers (probably heliocentrist)— not politically well-connected — whose

Ptolemy did not recognize the precession of the solar apse. (But he also did not know either the correct obliquity or its temporal variation — or even the fact that it varied.) Stronger marks (than these) against our hypothesis: [i] Many ancients had trouble finding the tropical year (in $\text{fn } 43$); fixing the anomalistic year (actual value, for Phil 1-epoch, by Newcomb’s solar theory: 365d14+1/102) would be tougher yet. [ii] The ancients knew the Moon’s anomalistic motion only to a precision of c.17/4 per 100$^\circ$ (even though the Moon’s orbital eccentricity was more than thrice Earth’s), but this error is approximately the size of the difference between the precessions of the solar apse & the stars. [iii] Rawlins 1991W §N5 estimates an error of nearly 5$^\circ$ 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 (in $\text{fn } 13$) the tropical year to eq. 30, but such factors were irrelevant to either the anomalistic year or the sidereal year, which were of no popular interest whatever. This contrast — and the known high accuracy of ancients’ adopted sidereal length — encourages the theory that the ancients also had an accurate value for the (equally non-civil) anomalistic yearlength. (Aristarchos’ sidereal yearlength was ordmag 100 times better than his tropical yearlength: $\text{fn } 43$.) [c] A hypothetical ancient scientist who determined solar anomalistic motion need not have known anything about stellar precession in order to obtain the correct remainder in eq. 1. [d] If Neugebauer 1975 p.298 can speculate, from eq. 1, that the ancients may have had an accurate figure for the precession of the equinoxes (though both the implied sidereal & tropical years he proposes are highly inaccurate — even while ancient values for the former are known to have been very accurate: item [b] above & Neugebauer 1975 p.601), then the at-least-asc-credible speculations here ought to be permissible.

If the “or’f” 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: Hipparchus 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 Ephemers version of the Brown-Newcomb luni-solar theory. (Adjusting for Earth-spin acceleration is obviously needless in this case.)

Rawlins 1991H fn 1.

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 Aabobe: fn 21), yielding $A = c.60^\circ$, consistent $(ident)$ with Sample A’ of the Ancient Star Catalog. (Though, van der Waerden 1988 pp.89f makes an intelligent case for $A = 75^\circ$.) This reconstructed orbit was pretty accurate ($\text{§C2}$, Neugebauer 1975 p.627, & DIO 1.1 §5 fn 13); indeed, its error-wave-amplitude appears to have been less than that of any of Hipparchos’ three successive solar orbitis (ibid §G10, K10, & K9).


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

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I Postscript C: Priorities & Sentences

I1 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.

I2 I urge that Hoskin issue a public (not typically behind-the-back, thus uncheckable) explanation of the mysterious priorities underlying that revealing finale.
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 so 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’ long-loss solar orbit, used by him for his solar-obs trio C] . . . . I have checked [Rawlins’] calculations and found nothing to quibble about. I hope your article will trigger some important re-evaluations.

G2 In the many months since, nothing has been re-evaluated.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 inexplicable 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 would’ve simply said that “As Rawlins shows” was meant. But Hoskin sees black at the middle word: after years of Hoskin attempts to banish & 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 vitalshortcoming 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 self-embarrassment. [ii] He has, for years, had untrustworthy advice from archons whose eminence superficially implied trustworthiness, and whose hefty cenotaphs 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 inexplicable 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

D High-tech level research texts were much less widely circulated and thus now lost (Rawlins 1984A p.984).

C14 So, in sum, we have the edifying spectacle of top modern ancient astronomy experts spending almost a century puzzling over — and conjuring up the most ingenious (even forced: eq. 5) theories to explain — the large error of eq. 1’s — 3°71/2 remainder. And now it turns out that: there never was an error that needed explaining . . . . (Note the parallel to the “mystery” of the disparate metrologies supposedly underlying Eratosthenes & Andronicus’ differing Earth-size values, a discrepancy-problem which dozens of analysts persistently worried for over a century [ignoring intelligent warnings by D.Dick & O.Neugebauer], a mystery which eventually turned out to have a simple, purely physical [not metrological] cause.)

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

D Doubling the 345° Interval

D1 Returning to the question of the empirical base for eq. 1: whether Hipparchos was discovering or checking it, he might (fn 18) have preferred having as long an interval as possible, since that is astronomers’ secret for determining accurate heavenly motions. The catch with the eq. 1 cycle is that (as Ptolemy correctly points out at 4.2) the number of draconic months corresponding to 12600°71/2 is not quite semi-integral: as one sees from eq. 1, it misses precisely 4630°1/2 by over 1%. Since (even for lunar perigee) an eclipse can only occur within c.12° of a lunar node, there will be few eclipses having a 345°-cycle match-two cycles ago: 690° into the past. (Odds against: ordmag 1-in-100.)

D2 Yet, of the (merely three) known eclipse observations by Hipparchos, one of them, that of 14/10/27 (partial, magnitude m = 3.1 digits at the S.limb: Almajest 6.5&69), is in fact two cycles of 345° (eq. 1) later than an earlier eclipse (visible at Babylon), namely, that of −830/2/4 (partial, m = 0.5 digits at the N.limb). ²⁰

D3 Moreover, since both of these two eclipses occurred very near lunar perigee, we can point to Hipparchos’ line preceding: at Almajest 6.9 & 4.2, Ptolemy reports Hipparchos’ use of a pair of near 48° apsidal-line eclipses (one of them the −140 eclipse, again), to determine the

¹⁹ Indeed, despite the Muffia Orlimann disasters (and DR’s perfect-fit Hipparchos-based solutions to all the 3 data trios involved: §§H3), Muffiosi continue lockstep-sweari that the solar data of eclipse-trios A&B are Babylonian: DIO 4.1 § 15 fn 26. See DR-vs-Muffia comparisons at Rawlins 1991W fn 209. Similarly, see ibid §§P1-P2.

²⁰ JHA 26:274 (1995/8): “In the Note by Hugh Thurston that appeared on p.146 of our May issue, the opening words of the second paragraph (‘As Jones shows’) were an incorrect editorial gloss and should be deleted. The Editor pleads inexcusa.”
Moon’s draconitic motion (eq. 19). Note: Ptolemy’s idem criticism of this method is valid — his own Almajest 4.9 method is superior50 to Hipparchos’ use of a 7160u cycle.52 (Perhaps Hipparchos was using the cited —719 & −140 pair for confirmation, not discovery.) But Ptolemy’s result is slightly worse than Hipparchos’ (or whoever’s: §A6) eq. 19.

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

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

\[ V_H = 8534M_A/\pi146 = 251M_A/269 = 19212638^3/6972480 = 27\times54569^9 \]

which went to correct to 1 timesec! To be precise: the mean error (of eq. 8) during the centuries53 discussed in this paper was = 1±3±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)54 daily motion given at Almajest 4.3&4 [based on eqs. 2&3):

\[ v_H = 360^2/V_H = (360^2/M_A)/269/251 = 13\times0353^3/5645^9/3845^9/3845^9 \]

And Ptolemy’s Almajest 4.3&7 value (the basis of his Almajest 4.4 anomalistic motion tables) differs from eq. 9 by merely55 12′′.

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

51 Though in the guise of an 8 1/2-century-span (note [H15 item [d)]) trio-trio proof, Ptolemy’s Almajest 4.6 development attains laughably overneat agreement with Hipparchos’ anomalistic motion (fn 55). (As also for the planets, Ptolemy’s amatorious inexperience with the empirical unrelatability, of the anomalistic mechanisms of his orbit-models, led him to prefer a nonintegral-retro “proof” of the mean motion, instead of the integral-return observations on which the mean motion tables were actually founded — by scholars who knew from frustrating experience that untamed non-ignorable perturbations would degrade any other type of empirical foundation. See fn 18 & fn 35.) I note that between two of the Almajest 4.6 eclipses (−719/3/9 & 136/3/6), there were 45 Metonic cycles = 10576u = 10476w = 31228601h. In a Metonic series of syzygies, 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).

52 Almajest 6.9 (again using the −719/3/9 eclipse) takes advantage of the near-commensurability: 716u = 777w, where 10 such cycles give an approximate (fn 49) demi-return in anomaly. (True, thirteen 716u-cycles would have put the centering about the 48 Metonic-cycle interval). (True, thirteen 716u-cycles would have put the centering about the 48 Metonic-cycle interval). (True, thirteen 716u-cycles would have put the centering about the 48 Metonic-cycle interval). (True, thirteen 716u-cycles would have put the centering about the 48 Metonic-cycle interval).

53 For the real ancient lunar and solar periods and motions in this paper, I use modern-calculated values for Phil 1 (−323/11/12 Alexandria apparent noon). [For lunar acceleration, see Dickey et al. Science 265:482 (1994/7/22).] This includes (where apt, e.g., fn 12) the effect of Earth-spin-acceleration on the length of the solar day (DIO 1.1 fn 51). Our modern calculations (modern calculations! — from modern reports) will involve some circularity (Rawlins 1991H fn 5) for judging experiments depending on the Moon’s mean synodic period in solar days (see fn 12 for real value, eq. 2 for ancient standard value); so I estimate its accuracy (§A6) by assuming that our knowledge of \( \Delta T \) for that era is good to roughly a quarter hour.

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

55 More precisely, −11′′46′′32′′/59′′′′ (explained at Almajest 4.7). Note the differing comments of R.Newton 1977 p.179 on the transparency of Ptolemy’s pretense to empiricism & empiricism’s erasure at Almajest 4.7. (But see here at fn 61.) R.N.’s charge is bolstered by our realization that the Ptolemy \( \eta_H \) actual result (ord2 pm in a million) is ordmag 10 times larger than the above-cited tiny (−12′′), supposedly-empirical difference between Ptolemy’s \( \eta_H \) (Almajest 4.3&4) and Hipparchos’ \( \eta_H \) (eq. 9)! (Ptolemy’s shift from \( \eta_H \) to \( \eta_H = 13\times0353^3/5645^9/3845^9/3845^9 \) made a very slight improvement in accuracy, though his analogous Almajest 4.2&9 correction, of Hipparchos’ draconitic motion, degraded its accuracy by a similarly trivial amount. However, it is hard to fault Ptolemy in the latter (1995). [c] Direct communication (1997). [d] Acknowledgement of some heresies’ truth (20?). [e] Acknowledgement of their value to the field (???)

E6 I remain (as always)17 prepared to sit down with Mufffosi (not in terrified-Mufff [DIO 4.3 & §15 §§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 unreflexively and without cultish 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 unenunciable 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 noncitica 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 (DIO 4.1 §4 fn 1). If one didn’t know better, one might get the idea that maintaining principled, impersonal 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-Ortubinary incident. While the JHA appears to have done little more than the bare minimum [this consistent strategy becomes crystal clear at [H] (so that, knowing AAAS-Science to be watching [[A3-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: DIO 1.1 fn 25 & Rawlins 1991W fn 08. DR continues to await its consummation — and Hoskin’s attainment of sufficient maturity to communicate with DR.) Further: the following §§F2&F3 compliments reflect some credit also upon Hoskin, since the JHA printed the note.

F2 Second, Alex Jones (Isis Board) deserves commendation for going somewhat beyond the minimum: it was evidently his decision to cite the 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 fashion18 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 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 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.

17 With the critical difference that DIO explicitly recognized the worth & high scholarship of some of Jones’ work (DIO 1.2 §4) and suggested that he had elsewhere been let down by poor refereeing (ibid §F4).
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.I.T.), DR spoke amiably to a number of Muffia scholars. But no communication has come from any since. (To the contrary, no untented Hist.sci scholar dares submit papers to DIO, for fear of cult ostracism.)
E2  We understand that K.Moesgaard (Univ Aarhus, Denmark) is reluctant to review our Tycho star catalog (DIO 3.1993) — which he privately deems valuable — so long as it contains anything displeasing to the Muffia. (Some Hist.sci scholars — even Danes — regard political game-playing as more important than doing justice to Denmark’s Tycho and to accurate history.) However, Annals of Science and Isis have recently requested & received review copies of the DIO Tycho catalog. (Predictably, the J.Hist. Astronomy has not been heard from.) Good to see; however, in an ongoing context of archonial aloofness & conference-exclusion (and years of Hist.sci’s total-blackout-noncitication of the DR discovery [DIO 2.1 §4 Tables 1&2] that Tycho faked 10 stars), DIO will, until the reviews appear, maintain a skeptical wait&see attitude: reasonable in light of the boilerplate relation (and thus eq. 10) was about as accurate ([E4] as the much more famous 251u in eq. 3, whose error is estimated at §D5.

By this time, Muffiosi know just as well as DR that: [a] their horrid behavior will be embarrassingly repulsive to future historians & [b] Muffia-proscribed RN-DR work will be regarded as of value (as will some Muffia work). Question: knowing that, sooner or later, we’re going to end up at [b] (probably sooner — unless recent Hist.sci gestures are just cynical pretenses), what sort of scholars need to be tediously dragged, kicking & snarling, millimeter by millimeter, to our presumed eventual state of fairness & tolerance? Why not just go straight there without more decades of bloodletting?

E5  Otherwise, it’s going to be a gradual incremental series of Muffia Acknowledgements, each step usually separated by (strife-filled) years: [a] DR exists (1992). [b] DIO exists as I have made known to several scholars recently, the Muffia&clo seem to have no wish (fun 7&22, §1 §2) to make any essential change in their habits. (All right, all right, so We made a few technical slips, but nothing here upsets orthodoxy or hegemony.) A unilateral refusal to communicate is an obvious sign of this. (E.g., §1, §1 fn 31.) As also is the very recent Isis-Muffia attempt (E2) to harm a DIO sympathizer. (DR has written and/or phoned several Neugebauer-clan-Muffiosi over the years. [See, e.g., DIO 1.3 in 280.] Nothing has come back. After two decades of such, I’m catching a trend here.)

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

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

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

$$\gamma = 3512^h$$

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

$$9831^d = 10536^d = 10668^d \times 1/2 + 22^d = 795^d - 65^d = 290315^d07^h$$

We again recall that superscript g = anomalistic solar years: ÆA1.) Remarkably, this relation (and thus eq. 10) was about as accurate (E4) as the much more famous 251u in eq. 3, whose error is estimated at §D5.

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

$$7042^d = 7547^d = 7642^d - 18^d = 569^d1/3 = 207954^d11^h$$

case, since eq. 19 was already so near-perfect [§F8 & fn 78] that any attempt to improve it was almost certain to effect an opposite outcome. Which may be why Ptolemy eventually reverted, at PlanHyp 1.1.6, to the unaltered original: our eq. 19, Heiberg 1907 pp.78-79.) Moreover, the difference between v and Ptolemy’s other (later) value for the anomalistic motion (eq. 13) is about twice as big as v’s error — and 16 times larger than Ptolemy’s superfluous — 12″ shift.

Consultation of Oppolzer 1887 (p.330) indicates that a quad (foursome) of eclipses was available, of which any 3 could have served as a trio for ancient geometric purposes: —832/26/27, —832/20/21, —831/9/10 (see fn 103 & §H4), & —830/2/4. The mean amplitude of the eq. 1 time interval’s variation was ±0.85; this amplitude-smelliness is, of course, mostly due to eq. 1’s slight sidereal-year — 7° 1/2 remainder. (Multiply twice sin 3.7/44 times the 2° solar eq.cr amplitude, and divide by the 1°/2 hourly synodic motion, to find 0h.5. Checking other cycles [fn 19] based upon eq. 3 would verify that this amplitude was consistent with solar-anomaly-causation; see fn 39.) Which would suggest (to an analyst who wasn’t correcting for solar anomaly) that averaging an around-the-zodiac set of 345 yr intervals’ offsets 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.

Other useful synodic-anomalistic cycles:

$$1520^d = 1629^d = 1649^d \times 1/2 - 1^d = 123^d - 40^d = 44886^d1/2, 5787^d = 6202^d = 6280^d + 11^d = 468^d + 47^d = 170893^d13^d, 8046^d = 8623^d = 8711^d + 5^d = 6501^d2 + 2^d = 237603^d04^h$$

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

The solar anomaly remainder of eq. 12 was about double eq. 11’s. But, given the size of both remainders, neither relation would yield highly accurate day-length intervals without (fn 79) averaging or solar-anomaly corrections.
Had the eclipse of −149/1/7 been visible to Hipparchos, he could have paired it with that of −719/9/1-2 (Almajer 4.6), except that the later one occurred below his horizon.60

E3 Returning to the attested 3277° relation: if (analogously to §D5) we combine eq. 10 with eq. 2, then we have

\[ YV = 9831.5M_A/10536 = 3277M_A/3512 = 27°.5/54600508 \] (13)

E4 The error in eq. 13 was +1.4±0.1° — 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 both of the asymmetry of eq. 2; see Rawlins 1991H fn 1.) So the mean of eq. 3 & eq. 10 was just about right, and it is a credit61 to Ptolemy’s judgement62 that he recommended both values (and no others). Another way of putting it: the average of Ptolemy’s two estimates (eq. 8 & eq. 13) of the lunar anomalous month was almost exactly accurate: error ordmag 1/10.

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

E6 Further, said pairs occur not randomly but rather in bunches. (See §F11.) Astonishingly, the last pair that happened before Ptolemy (who imparted eq. 10) started with the −830 eclipse — an event which occurred a thousand years before! That pair is: −830/2/4 & −36/12/6. The latter eclipse (−36) is just one 36 yr cycle later than −381/12/12 eclipse (which is attested at Almajer 4.11),64 and the former (−830) is the very eclipse we already suggested (at §D2) Hipparchos might have used for the 690 yr cycle.

E7 Note: the actual interval between the 2 eclipses of §E6 was 290315°/26° (56 shorter than 9831° in eq. 11). Division by 10536° (eq. 11) produces \( V = 27°.554583 \), ordmag

Naive estimates65 are not rare, then the Greeks’ access to 8th century BC Babylonian eclipse material was much less full than is suggested by Ptolemy (fn 84). (Rawlins 1985S has implied that the data available to Greek astronomers (from this time indeed may have been fragmentary; however, see §C1’s alternate explanation for ancients’ evident non-use of eq. 12 & such.) [c] Since fuller data are cited by Greeks from the 6th century onward, identifications of 690 yr-cycle pairs should have been made. (e) Possibly the Greeks did use either this cycle or a similar one (e.g., \( JD3’s 7160° \approx 7770° \) \( \approx 7675° \) \( \approx 579° \), 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° (or eq. 18’s 2729° or its triple [566 yrs], which has a better lunar anomalous return but a remainder of 4°, nonetheless) but by continued-fractions analysis. (Eq. 19 is mentioned at Almajer 4.2, but not in connection with an empirical eclipse-pair 5458° apart.) See fn 79.

Note that if eq. 19 was derived by continued-fractions (and its prominence by Hipparchos’ era is likely related to mathematically-refined investigations), then we will probably not be able to trace its ultimate empirical foundation (see Neugebauer 1975 p.106-107, partially cited here at the outset: fn 4) — especially if it is not built upon a specific period relation, as eqs. 11 & 31 each were.

Arystyllos may have had the opportunity of discovering the 569 yr cycle from the eclipse pair: −831/9/9-10 & −26/1/15-16 (interval 207954116).66

E8 For our similar but far greater debt to Ptolemy, see Rawlins 1991W fn 94.

This pair ended a series of such 795 yr-pairs (connecting two series), a series which had started with the pair −1047/9/27 & −252/7/30. (Neither of these two eclipses was visible in Europe or Babylon. Of this series, the first pair visible in Babylon was −1029/10/8 & −234/6/10-11.) Note, however, that this series of 795 yr-cycle pairs was not the only one that included eclipses in Hipparchos’ time. Pairs which ended other such series: −935/3/126° & −140/1/127 (fn 86), and −924/2/24 & −130/1/227. (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 are \( \approx 113° \) & \( \approx 112° \), and resp magnitudes \( \approx 0.4 \) & \( \approx 0.5 \); so this is virtually the outer edge of possibility for 795 yr-cycle eclipse pairs, remarkably far from perigee.) [All 795° pairs are from saros-series whose Meus-Mucke numbers differ by 53.] 67

E9 If, despite its large solar-anomaly remainder (−65°), eq. 11 (795 yr base) was found via the −36/12/7 eclipse, then the discoverer preferred it to eq. 1 (345 or 690 yr base) simply because its interval was more than twice as long. The −36/12/7 mid-eclipse was at 22:51 Alex Mean Time (22:56 Alex App Time), at A = 74°.7 & d = 0°-9 (topocentric); its magnitude \( m \approx 6.9 \) (N.limb)
While DR was glad that the JHA replied at all to Thurston, it must be said that JHA’s reception of the Thurston note was atypical from the start: Hoskin did not acknowledge receipt until after refereeing. At that point, Hoskin said that the JHA would publish a version that was agreeable to Jones. (Note that, had Jones been a completely dishonest scholar, this policy could have given him the power to squelch the note. It’s happened.) No thought of checking with JHA-shunned DR, who had caused the entire proceedings — not to mention having indisputably first arrived at (and published: DIO 1.1 §6) the orbit which fit trio C.

Now, when a journal has published an erroneous paper by scholar J, whose errors are corrected by scholar x, it might seem to you and me that the party whose advice should be sought is the latter. But that is not the way the JHA operates, since this journal’s prime measure of authors is personal rather than substantial. Despite the strong hint at DIO 4.2 (p.54 News Note C), neither Hoskin nor the Muffia communicated with DR during the refereeing, composition, & publishing of the JHA correction.

I quote further from the 1995/3/14 DR letter excerpted at §B2:

What kind of journal publishes a correcting note based on scholar x’s revelation of . . . errors in a lead paper in the journal, illustrating the journal’s refereeing derelictions, but refuses to seek refereeing by or even [communication] with scholar x? (Hoskin’s condition for publishing the correction was that the note must be approved by the [erroneous author], not the corrector. Do you seriously regard DIO as over-reacting, when we treat the Hist.sci community as a zany cult?) It seems that DR is the only party who [DIO 2.3 §6 §F3] doesn’t want the Muffia-DIO war to continue. (I prefer concentrating on historical discoveries, not modern flubdubs.) However, the Muffia insists on keeping its lordly nose in the air (see Black Affidavit, DIO 1.3 §10, end of §4 [also at DIO 4.3 §15 §J2]), and its critics are delighted to watch DIO keep bloodying that nose.

If a cult keeps cheating scholars, some naïve souls may object. A few extreme-nonrealists (DIO 4.3 §15 §C11) may even do so out in public.

Though the main initial referee report on Thurston’s paper was something of a credit to the JHA, Hoskin’s close-to-vest approach then undermined the opportunity for progress — and even (§D8) needlessly degraded accuracy a bit.

The JHA’s referee report on Thurston’s note commented:

Are the parameters for the eccentricity and apogee historically supported? . . . some verification of any historical use of these parameters apart from this derivation would be necessary in order to reach any conclusions.

Can this theory [the DR&Thurston Greek-trig solution of trio C] explain the discrepancies between Hipparchus and Ptolemy in the eclipse trio observations? [I.e., trios A&B of Jones 1991 & DIO 1.2-3.] If the author can explain these as well, then the case for Hipparchus using a “Ptolemaic” solar motion will be much stronger, since it is the eclipse trios that led Jones to the Babylonian model [as an explanation for Hipparchus’ solar positions].

DIO merely possessed the emotional calmness to seek input from an exiled §G & DIO 1.2 §B3 party, Hoskin would’ve had the answers to these comments.

As to ref-comment §D3: DIO 1.3 §§M4&K9 contained (fn 21) the very Greek-trig solutions, for both trio A & trio B, which the JHA’s own referee recommended be brought to the issue. (Not only solar orbits but lunar orbits as well: ibid eqs.6, 8, 9, 19, & 20.) However, JHA readers are not informed of that — i.e., of the cohesive breadth of the DIO 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 $V$.

Also, both the $-30/2/4$ & $-36/12/7$ eclipses were very near perigee. Likewise for the $-140/1/27$ eclipse proposed at §D2 as a possible 690 yr-cycle-match with the $-830/2/4$ eclipse. That is, all 3 of the eclipses we’ve concentrated on here ($§D&E$) were perigee-events.$^{65}$ $-830, -140, -36.$

The average saros series (abbrev “ss”) lasts about 8 centuries (see fn 69, fn 73, & §F3 for details), as its successive eclipses slowly grow, crest, & then fade in magnitude. The famous 18$^{1/15}$ period between ss eclipses is governed by the relation:$^{66}$

$223^c = 239^c - 3^c = 242^c - 6^c = 65855/1^c = 188^c10/2^c/3 = 188^c10/2^c/3$ (14)

For which we remember (§A1) that superscript K = Kallippic years of length $Y_K$, where:

$Y_K = 365^c1/4$ (15)

in contrast to the Hipparchos-Ptolemy “tropical”$^{67}$ yearlength adopted at Almajest 3.1-9.4:

$Y_{J} = 365^c1/4 - 1/300$ (16)

For comparison: the actual tropical yearlength at the epoch of Phil 1 was about $365^{c1}/4 - 1/133$, thus eq. 16’s $Y_J$ was too high$^{68}$ by $6^c$ — i.e., 10$^c$ century!$^{69}$

The mean ss-length of 8 centuries is governed$^{70}$ by eq. 14’s $-6^c - 28^c$ draconic remainder,$^{71}$ which requires roughly a few dozen saros to cover the $22^c$ — range surrounding a lunar node, in which mean-condition eclipses can occur. Though ss-lengths can be less than 7 centuries and more than 10 centuries, the average ss lasts$^{72}$ between 8&9 centuries$^{73}$. 

$^{65}$ The three cited eclipses’ lunar mean anomalies were: $+1^c$ (−830/2/4), $-1^c$ (−140/1/27), & $+1^c$ (−36/12/7).

$^{66}$ See Almajest 4.2 or Geminos 18.6. The 10$^c/23$ remainder is rendered as 10d2/3 at Neugebauer 1975 p.502, from dividing the Babylonian yearlength/monthlength ratio 12:22:08 into 223. (The discrepancy once temporarily misled a highly eminent Hist.sci referee.) Speculations on the original Babylonian figure, 12:22:08 (eq. 4, regarded by the Muffia as “perhaps the most fundamental parameter in Babylonian astronomy”, Aaboe 1955 p.123; see DIO 29 & its concluding emphasis): [a] Was it caused simply by an ancient’s division of the mistaken version into 223? [The remainder is misrendered as 10d3 at p.v.21 Menzel & Gingerich 1962, where we also find an example of the most elementary of ancient astronomy errors: $-412^c$ confused with 412 BC.] Or [b] Did an eclipse-investigator find it by dividing the anomalistic year by the synodic month $—$ a ratio which, if then divided into 223, would have given 18$^{10/23}$ (or eq. 14) 18$^{10/23}$. Regardless, note that the Greek version (using 10$^c/23$; eq. 14) is peculiarly hybrid: Kallippic & Aristarchan. It was based upon dividing the Kallippic yearlength (eq. 18) into $223/3$, where $M_A$ (eq. 2) is non-Kallippic. See Heath 1913 pp.314-315, Neugebauer 1975 p.603, & Rawlins 1985S. 

$^{67}$ i.e., multiplying the famous saros-period (eq. 14: $18K10^c39/3$) times (22$^c$) (0$^c$/saros), we have a figure $837^c$ for the Kallippic yearlength — 1st reconstructed by the adventurous genius of PThamney.

$^{68}$ See fn 13.

$^{69}$ Causes examined in Rawlins 1985S.

$^{70}$ I.e., multiplying the famous saros-period (eq. 14: $18^c10^c2/3$) times (22$^c$) (0$^c$/saros), we have a figure close to fn 72’s 8&9 centuries.

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

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 12/2 and 15/2 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.)

Taken the weighted average of the fn 73 ss-lengths (minus 1), and multiplying by eq. 14, yields 837 yrs for the average (umbral) ss-length for the data of Liu & Fiala 1992 Table 3.1.
— but in fact very few (about 1/12th) are in this “mean” range, most ss being nearer the extremes: about 70% are either between 7&8 centuries or between 9&10 centuries.73 The median ss lasts 44 eclipses (also the mode) or 775 years. (See data of fn 73.)

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

F5 The happy circumstance, that several centuries passed (between 37 BC and the death of Ptolemy)74 without a 795 yr pair occurring, is the fortunate accident which enables us to prove from eqs. 10-11 that the Greeks were using eclipse data from no later than ~380/24 — i.e., more than a century earlier than had been established by now-surviving explicitly dated records (the earliest of these being the ~720/3/19-20 eclipse reported at Almajest 4.6).

F6 Two neat eclipse period-relations (eqs. 17&18) establish what I will call the “PBT”: the Precessing ss-Bound anomalistic-Triangle, governing ss-starts&ends; and this triangle’s slow-motion precession in turn explains75 the long gap (in the occurrence of 795 yr-cycle eclipse-pair-ends) following the ~36 eclipse.

F7 Two relations underlying the PBT are: the wellknown76 29 yr cycle,

\[358^8 = 389^2/3 + 2^2 = 388^1/2 = 29^2 = 20^2 = 10572^1\]

and the 221 yr cycle

\[2729^2 = 2924^2/3 + 13^2 = 2961^1/2 = 22^4 = 131^2 = 8058^4\]

as a near-perfect synodic-draconitic return. (See also Neugebauer 1975 p.310.) And how well did the ancients do, when choosing eq. 19 (= 16117^8 = 441^2 = 97^2) as the basis for their draconitic tables? With components this large, the best choice should be accurate to better than 1 part in 10 million. And the accuracy of eq. 19 was indeed about that fine.

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

\[545^8 = 5923^8\]

(19)

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

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

74 See §H4 & §F11.

75 Partial history at van den Bergh 1955 pp.22-23.
Crawling Towards Integrity

A Historical

A1 The 1995 May issue of the Journal for the History of Astronomy contains (at p.164) a historic item: the JHA has there actually (for the first time ever) cited DIO. Moreover, the 1991 May author, Muffia-protégé Alex Jones, has explicitly stated that there were three errors in his 1991 May JHA lead paper and that the correct math was first published in DIO 1.5-7’s “Muffia Orputary”.1

A2 Whatever the shortcomings of this JHA notice, it is nonetheless an event which (we oblige & glad to acknowledge) DIO predicted would not happen at all.

A3 However, what JHA Ed. Michael Hoskin has done so far is depressingly minimal2 — and even this only occurred after Eliot Marshall of Science (Amer Assoc Adv Sci) 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 Muffisi (notice at DIO 4.2 ‡ §B38).

A4 So, it has taken [a] years, [b] independent-method Cambridge-math-verification, & [c] two AAAS phonecalls even to get the JHA to own up to errors of elementary arithmetic. And there has not been the slightest indication of Muffia-JHA interest in going beyond this slim concession. (To the contrary: §D6, §E3, §G, §H, & DIO 4.3 §15 §F4.) The consistent, regrettable suggestion is: for JHA & Muffia finally to acknowledge their manifold errors of post-highschool math will presumably require even greater stimulation than that cited in §A3. Well, 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 Muffia along, as it snails down the long road towards integrity.

B Three Muffs Down, Three Dozen Retractions to Go

B1 But, besides the Muffia Orputary affair, there have been several other JHA disasters. Like forty-odd. (See, e.g., fn 1, reminder at DIO 2.1 §2 §M, & compact itemization of Muffia muffs at §A of “Casting Pearls Before Pyglets”, DIO 4.1 §14.) There has been no Hist.sci notice of any of these miscretes, though officers of all the erring journals are now aware of them. Evidently, simply not reading (or acknowledging or citing) the corrections is considered acceptable behavior in Hist.sci (§B2 & fn 3): a curiously dishonest way of protecting 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 “Muffia”. (He has printed [& never apologized for] highly insulting Muffia charges against R.Newton’s competence [DIO 1.1 §11 §C7], which was of the highest order; but JHA cannot print DR’s use of the word “Muffia” for the Neugebauer clique, which actually has published dozens of muffs: forty-five cataloged at DIO 4.1 §14 §A; more here at §1 fn 1. Enjoy also the Muffia’s inadvertent gross slander of its own godparent: DIO 1.1 §15 fn 15.) This censored comedy recommends the approach announced in DIO’s inside-back-cover publisher’s statement: DIO authors can use any style they like. Their choice, not DIO’s.

2 Are Muffisi hoping onlookers will conclude that DR is as Impossible as pre-labelled (Rawlins 1991W §B1), from his failure instantly to assume the proper footkissing-gratitude prostrate-position (§C5), at the JHA’s amazingly small &muddy concession? (Compare fn 7 [1995/3/14 letter] vs. §D6, G, & H.) Note: the Muffia’s utter inability to understand DR will continue so long as Muffia insist (§C5 §E, fn 15, §H) upon noncommunication.

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(Err. 19 is less accurate today, merely 2 parts in 10 million.)7 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 suggests7 that it (§& eq. 29) may be from the time of Aristarchos.

[The origin of eq.19 was finally solved in 2002 at DIO 11.1 §3 eq.3: Hipparchos used the technique described at Almajest 6.9 but paired his own — 140 eclipse with a much older one (~1244) than that cited by Ptolemy (~719).]

F10 Returning to examine PBT behavior: the little 2° term, by which the anomalistic part of eq. 17 differed from precisely 1/3 of an integer, ensured that (on average)3 the upper or lower temporal bound of each ss would, in the short run (a few decades), occur at one of 3 evenly spaced points — a nearly equilateral tri-angle that was pretty stable in time (i.e., mean drift = merely c.2°/299 yrs). And eq. 18’s similar anomalistic 13° term ensured that, in the long term, each triangle faded into another which was5 (on average) 13°/221 yrs ahead of the previous one.

77 van der Bergh 1955 p.24 mentions eq. 19 but (due to basing his p.18 continued-fractions analysis upon modern motions) prefers cycyles at pp.18-19 which were not (in antiquity) as accurate as eq. 19. The clever reasoning at van der Bergh 1953 p.24 finds a 1769 yr cycle, but secular variations over such a period will slightly degrade its use. (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 eclipse magnitudes is not ideal as a basis for eq. 19: §A The eclipse-magnitudes are different. §B The number of non-independent uncertainties: see fn 48.) Note also that, over a 441 yr interval, the effect of a 4 1/2 hr discrepancy was ordnag 1 part in a million (though, in fact, eq. 19 is alot more accurate than this: §F8), thus, a hypothetical draconitic yearlength based upon the ~719 & ~278 pair (proposed in Rawlins 1985S) would probably be more accurate than any value known to exist previous to Ptolemy, even without any compensations: simply dividing 5923 yrs into 17585 cycles yields the equivalent of about 274.222 (which was low by about 2 times).)

79 The modern eq. 18 remainder is more than 14° — was there actually (for the first time ever) cited JHA & Muffia finally to acknowledge their manifold errors of post-highschool math will presumably require even greater stimulation than that cited in §A3. Well, we at DIO will do what we can, to provide what is needed.

80 The modern eq. 18 remainder is more than 14° — was there actually (for the first time ever) cited JHA & Muffia finally to acknowledge their manifold errors of post-highschool math will presumably require even greater stimulation than that cited in §A3. Well, we at DIO will do what we can, to provide what is needed.

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

82 The modern eq. 18 remainder is more than 14° — was there actually (for the first time ever) cited JHA & Muffia finally to acknowledge their manifold errors of post-highschool math will presumably require even greater stimulation than that cited in §A3. Well, we at DIO will do what we can, to provide what is needed.
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.

References

Hugh Thurston Mayan Eclipse Table 1996 January DIO 6 §2 35

[Note added 1995.] E.g., Lost Civilizations (Time-Life) nationally broadcast on 1995/7/9 the explicit claim that the Mayas could predict solar eclipses.
C5 The total number of days covered by our table (the last accumulated total in row B) is 11598. The total number of months (sixty-6-month periods plus nine 5-month periods) is 405. Both these numbers are divisible by 3. And the numbers 3986 and 7972, which are, respectively, one-third and two-thirds of 11598, both occur among the cumulative total in row B. So the table breaks cleanly into three parts, in each of which

135 months = 3986 days

My suggestion is that the table was based on this figure, which is reasonably similar to the figures at Palenque and Copan. (We do not know for certain where the Dresden codex came from, but it was not from either of these cities.)

C6 The question that faced the table’s constructor is: how can we build an interval of 135 months out of 5-month and 6-month intervals, with substantially fewer 5-month intervals than 6-month intervals? There is only one way: twenty 6-month intervals plus three 5-month intervals. Then there should be three 5-month intervals in each third of the table. There are.

C7 If each 6-month interval is 177 days and each 5-month interval is 148 days (as they will be if 135 months = 3986 days: §C5), there will be a total of 3984 days. Two days short. So twice in each third of the table the cumulative table should increase by 178 instead of 177. And this is just what happens. It looks as though the person who discovered this slipped the extra days in without telling the scribe who painted the table, and who innocently filled in all the 177s along the bottom without checking the addition.

C8 The distribution of the 177-day, 178-day and 148-day intervals is shown below. The top row shows the first third of the codex, the middle row the middle third, and the bottom row the last third. (There are twenty-three intervals in each row, thus sixty-nine intervals in all.)

C9 There is another interesting point about the numbers in row C. If we list them, we find that they are far from evenly distributed: they clump into three sectors of the 260-day cycle. There are none from day 12 to day 52; twenty-three between days 53 and 91 (inclusive); none between days 92 and 149; twenty-three between days 150 and 184; none between 185 and 236; twenty-three between 237 and 11 (equivalent to 271 in the 260-day round cycle). This is what should happen if the dates are in fact dates of eclipses. The reason is that an eclipse cannot occur unless the Sun is near a lunar node. The average time for the Sun to travel from one node to the next is 173.31 days.6 If the Sun is at a node on day 72, then it will be at the other node on day 245 (or perhaps 246), back at the first node on day 159 (or 158) and back again at the second node on day 72. Since eclipses occur only when the Sun is near a node, they will cluster around these dates. I chose day 72 as the middle of the first occupied sector of the 260-day cycle. Days 245 and 159 are in the other occupied sectors.

C10 The Mayas undoubtedly knew of this. They placed vastly more importance on their sacred round than we do on the days of the week. (The best we can do is a rhyme like “Monday’s child is fair of face . . .” or a general belief that Friday is an unlucky day for a wedding.) Victoria and Harvey Bricker have an ingenious and complicated theory showing how the Mayas could have used sacred-round entries to turn the table into an efficient table for warning of the possibility that an eclipse might be imminent: the table spans 405 months (§C5), and only on the 69 months listed could an eclipse occur. I recommend their paper

89 All modern calculations agree closely on the −830/2/4 eclipse’s magnitude: Oppolzer 1887, m = 0.4; Mees & Mucke 1992, m = 0.3; Liu & Tiala 1992, m = 0.5; DIO, m = 0.5.
90 Mees & Mucke 1992, m = 0.4; Liu & Tiala 1992, m = 0.5; DIO, m = 0.5.
91 This remarkable ss lasted from −1840/6/8 (invisible in Babylon) to −830/2/4 (visible there). These two bounding eclipses’ magnitudes were, respectively, m = 0.2 digits & 0.5 digits. (Mees & Mucke 1992 has m = 0.01 for the former.)
92 The −848/12/25-26 eclipse occurred well below the horizon at Babylon. Its m = 1.0 digits.
93 See Geminos 18.5 (Anaj ed., p.94), cited by Pedersen 1974 p.163 n.3.
94 My impression of Babylonian astronomical sophistication is inconsistent with either non-direct explanation.
95 Perhaps ultimately due to Hipparchos: Pliny 2.57. (One would suppose that the Princetitute could improve upon 2 millennia-stale math. But, then, see Rawlins 1987 n.30 on the Almagest 9.3-4 mean motions: Dio 4.3 [15] 11.) 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: §III.
times he cites), I would have found an interval of 147.3, or less than 29.5/month, which is shorter than the average 29.5306 month. Yet another triumph for the Muffia’s supreme ability to know the answer to a problem, without having to bother about mere evidence: DIO 4.3 [15 §13.] Neugebauer 1975 p.130 n.2 did not know of any such interval where both eclipses were visible to active astronomers. (See also ibid p.504 n.12 & pp.525f.) So, the very evening when I first received the Thurston paper, I naturally turned to the 9th century BC eclipse trio cited above (fn 56) and thereby instantly found what Neugebauer had vainly scoured Oppolzer 1887 for: the –831/9/9-10 & –830/2/4 eclipses are five months apart[6] — and both were visible[7] in Babylon. So, this very rare short observable interval might have been a cause of Babylonian interest in the –830/2/4 eclipse. Which hints at a further possibility (one that does little violence to what we already know of early Babylonian astronomy): that the –831-830 grazing-eclipse-pair marked the first gleanings of organized eclipse-prediction in Babylonia. (Note: –831/9/9-10 = ss-start, and –830/2/4 = ss-end — typical for a 5 month-separated eclipse-pair.)

H5 I will next (§H6) examine yet another possibility — and thereby leave us on one of the horn&horn extremes (of our range of choices): was Babylonian interest in the –830 eclipse due to a 5 month passing affair (the most conservative interpretation at §H4) or 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)[9] records of the eclipses of entire ss?

H7 If the Babylonians specially preserved the –830/2/4 ss-conclusion eclipse, then are we wrong in covering it holding one end of a thread of traditional Babylonian observations which extend 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-zadaga — the time of the very beginnings of Babylonian scholarship.

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

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

97 The very occurrence of the –831/9/9-10 dawn eclipse (mt = 0.6 digits by Oppolzer 1887, & 0.1 digits by Meers & 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 mt = 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 [§B1]), to reward their patience.

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

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

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

101 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.

Hugh Thurston Mayan Eclipse Table 1996 January DIO 6 §2

| (no fives) | 6 | 12 | 18 |
| (one five) | 11 | 17 | 23 | 29 | 35 | 41 | 47 | 53 | 59 | 65 |
| (two fives) | 64 | 70 | 76 | 82 | 88 | 94 | 100 | 106 | 112 | 118 |
| (three fives) | 111 | 117 | 123 | 129 | 135 | 141 | 147 | 153 |
| (four fives) | 158 | 164 | 170 | 176 | 182 | 188 | 194 | 200 |

etc. The longer the interval, the more fives are needed. This could easily give rise to the idea that intervals between eclipses should be based on 6-month intervals with occasional 5-month intervals.

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[2] above at page 31) are:

B: 1211 1388 1565 1742 1919 2096 2244

D: 177 177 177 177 177 177 148

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 our days 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):

C: 79 256 173 90 7 184 72

D: 177 177 177 177 177 177 148

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 fn 83.] The Mayas had similar figures. From inscriptions that gave the age of the Moon at various dates, which must be...
Each half-page of the table consists of: two rows of text, which I have labelled A; a row of numbers, B; a triple row, C; and a row, D, of numbers. On average, 177 days amount to 6 months; 148 days amount to 5 months. This inevitably reminds us of Babylonian tables of eclipses, which consist of a list of dates, given in years and months. Each entry is either 5 months or 6 months after the previous one, and the intervals appear in a strikingly regular (saros-length) pattern, namely

repeated over and over again every 223 months. (The numbers of successive 6-entries are seven, six, seven, seven, six.) You can see a transcription of one of these tables in [2]; another table, ACT 60, two hundred years later, carries on the same pattern. (See [3] page 525; and [4] volume 1 pages 106-109 & volume 3 page 38.)

We know how these tables were constructed. The Babylonians had ephemerides which tracked the latitude of the Moon and which display an eclipse magnitude whenever they regarded the latitude at Full Moon or New Moon as close enough to the ecliptic. The dates when this happened were excerpted from an ephemeris for Full Moons (or for New Moons) to form an eclipse table. The underlying mathematical theory of the motions of the Sun and Moon make the entries occur in the regular pattern.

As far as we know, the Mayas did not track the Moon’s latitude, or indeed any latitudes or longitudes. The only data in codices or inscriptions are times, not latitudes.

Without a theory of latitudes, how would anyone ever light on 5-month and 6-month intervals? It is easy enough to say gleefully that these are good eclipse intervals (I have been guilty of doing this) or, as Otto Neugebauer has said more specifically, “it is a well-known rule of thumb in antiquity that eclipses can occur at 6-months distance or occasionally at 5-months intervals” ([3], page 504). But lunar eclipses at a 5-month interval are possible only under conditions which Neugebauer himself admitted are “very special” ([3], page 130). So the 5-month intervals would be particularly hard to find from mere observation. [See here at §1 §4.]

Let us look at an example. It is no use taking eclipses from Oppolzer, Mecus-Mucke, or Lii-Fiala because these modern compilations list eclipses visible anywhere on Earth, and we need eclipses visible to the person who is compiling the table. No table of eclipses visible from a Mayan site is readily available, but [5] lists eclipses of the Moon visible at Babylon, and any other site will show a broadly similar pattern.

For eclipses in one sixty-year period (I happened to choose page 146 in [5]) the intervals between successive eclipses are 6 months (which occurs twenty-seven times), 11 months (once), 12 months (ten times), 17 months (three times), 18 months (twice) and 23 months (six times). No 5-month intervals. However, we might reason as follows. The commonest interval is 6 months. It can be doubled, but (for this period) once out of about eleven times one of the 6-month intervals is replaced by a 5-month interval. If it is trebled, it incorporates a 5-month interval more often than not. If quadrupled, it incorporates a 5-month interval every time.

The Mayas even have thought that there was something occurring fairly regularly without which there could not be an eclipse but with which there could be. This is no more fanciful than the celestial dragons rahu and ketu which Vedic astronomers thought caused eclipses and which later Indian astronomers identified with the nodes. A dragon at the node could swallow the Sun

This reasoning is reinforced if we compute the intervals between every pair of eclipses, not just successive eclipses. The intervals, in months, which occur more than once between the eclipses listed on page 146 of [5] are:
used the Heron 62/3/13-14 Alexandria midnight eclipse (Neugebauer 1975 p.846) with the 
−719/3/8-9 Babylon midnight eclipse (both at the star 49 Vir, whose latitude \( \beta = -3 \)°, in order to found the equation:

\[
\begin{align*}
9660^a = 781^y & = 10483^w - 2^2 = 285265^13^h \\
(21)
\end{align*}
\]

Of course, it is possible that the relation was known even earlier, since 781 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: Both eclipses are attested (\( \text{§}34 \)) connected to a specific star (49 Vir). The −719/3/8-9 pair is unusually neat; both mid-eclipses occurred at local midnight, thus the parallaxes were small (merely +8° in longitude & \( \text{ordmag 1'} \) in ra., and the differential parallax was trifling (fn 110). The solar arc between mid-eclipses fell only 0.3° short of precisely 281160° or 781 sidereal revolutions.

Eq. 21 produces a value for the sidereal year \( Y_\text{\textbar} \), implicit in PlanHyp, of very nearly:

\[
Y_\text{\textbar} = 9660 M_A/781 = 365^1/4 + 1/148 \\
(22)
\]

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

\[
Y_\text{\textbar} = 36000 Y_\text{\textbar}/35999 = 365^1/4 + 1/147 \\
(23)
\]

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

See fn 107. One must make explicit the implication here (on which a direct estimate of the sidereal year depends: fn 110) that some of the early Babylonian observers preserved not only the time & magnitude of an eclipse but also the identity of the star at which it occurred. (If not, then eq. 21 was rather in the nature of a lunar definition of the sidereal year: akin to eq. 30’s better-known definition of the Metonic “tropical” year.) For further evidence consistent with this unsurprising hypothesis, see DIO 2.3 §18 fn 20, which reveals that Ptolemy accepted that the −719/3/8-9 mid-eclipse occurred at 49 Vir. Ptolemy (\( \text{Almajest} \) 4.6) put the eclipse at 163°34′. And he put 49 Vir there also: merely adding 8°1/2 (the probable Ptolemy rounding of 8°34′) — see, e.g., Toomer 1984 p.452 n.69) for §100 precession gave 172°14′, this star’s longitude at Almajest 7.5° — with a quarter-degree ending that is found (DIO 4.1 §13 fn 11) in only 5 of the Ptolemy star catalog’s longitudes (less than 1/2 of one percent of the 1025 stars), all 5 of them associated with lunar or Venusian conjunctions: again, see DIO 2.3 §18 fn 20. (Ptolemy took the reported −719/3/8-9 conjunction as ecliptical, though the 1st century AD discoverer of eq. 21 evidently realized it was equatorial.)

Eq. 21 can be re-rendered: 781° = 285265°13/1 = 9660°−3°, & eq. 20: 800° = 292205°15° = 9895°−6°.

Both equations are based upon the real mean periods for epoch Phil 1 (~323/11/12).

And eclipses 800° apart are also common. Indeed, the 800° cycle could have been discovered by the same astronomer who found the 795 yr cycle, since the start of the −302/10 eclipse was easily visible in Alexandria (not Babylon), just 800 sidereal days after the −830/24 eclipse (\( \text{§}32 \)) presumably used for finding the 795 yr relation (eq. 8). Both the −830 & −30 eclipses occurred around 777 Leo. Note that no Nest relation is very close to integral in anomalistic returns. (In this respect, the infrequent 743 yr cycle is the best of the lot: 911° = 984°+14°.) Eq. 21 (781 yrs) is poor in this regard (9660° = 10353°−92°), but it was used anyway — and to good effect, since the interval is so long that even a few hours of anomalistic-nonreturn-caused error had little effect on a direct-division result: merely 1 part in ordmag a million. Note that, for gauging the sidereal (star) yearlength from the empirical data that went into eq. 21, one needed merely each mid-eclipse’s: [a] time, & [b] position vis-à-vis 49 Vir. (The parallaxes virtually cancelled for this lovely pair: in fact, the parallaxes’ difference was ordmag 1′. See \[b\] if the data for [b] existed (fn 108), then the lunar anomaly — though useful for gauging synodic monthlength — would be unnecessary for estimating the sidereal year. Moreover, there were other (adjacent) 781 yr-pairs. Of the −720-719 eclipse-tightquad (see fn 103), all four mid-eclipses were visible in Babylon. And most had accessible matching eclipses in 61-62 AD — all were visible at mid-eclipse in Alexandria except the last, the end of which was visible around Persepolis & east thereof. The four pairs [intervals in brackets]: −720/3/19-20 & 61/3/24-25 [285265d12b], 719/3/11-12 & 610/17-18 [285265d02b], −719/3/8-9 & 623/13-14 [285265d01b], −719/3/2-7 & 629/9-7 [285265d20b]. Dividing just the 3rd interval by 781 would have given a sidereal year of 365d14/163; but averaging the four intervals or the two extreme cases would have produced a mean interval of 285265d12b. (Very near to 9660° = 285265°13. See eq. 21.) Dividing this by 781 produces a sidereal year of 365d14 + 1/149, which agrees closely both with the truth (365d14 + 1/154) and with eq. 22. (See fn 54.)
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 31 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

- uinal = 20 days
- tun = 360 days
- katun = 20 tuns

and higher units that we don’t need here. So

2 katuns 0 tuns 3 uinals 5 days

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

2 3 5

(written in a column).

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

\[ 1 = \frac{1}{2 + \frac{1}{4 + \frac{1}{448 - \frac{1}{7 - \frac{1}{8}}}}} \]

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

1

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.
Truncation after the 3rd fractional term (the 2) will produce the famous 19\(^{th}\) Metonic cycle (a valuable artificial identity still used to compute the date of Easter):

\[19^\circ = 235^\circ\]

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

\[8523^\circ = 105416^\circ\]

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\(^{nd}\) 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 \(\rightarrow\) 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 sidereal monthlength. (Another comment in passing: Muffiosi will reflexively attempt to ignore eq. 210 damn eq. 31 by claiming that the historical context — read: their idea of same — does not support any connexion with eq. 21. It will not occur to Muffiosi to ask: when is a discovery so powerful & central that it forces re-evaluation of one’s perception of context? See, e.g., fn 137 & Rawlins 1991H §§A7&B12.)

Note also a few other related coincidences: [a] The 781\(^{st}\) interval is the only one of the six members of the 800\(^{th}\) Metonic nest (§3) that yields eq. 31. [b] It is also the only member of this sextet which we know was observed (§6 item [a]). [c] And one of the two eclipses, on which we are proposing that this star-year relation (eq. 21) was founded, has been precisely related to a (very unusually-ruined)\(^{122}\) conjunctive star in the Ptolemy catalog. [d] Finally, do not miss the provocative fact that the main two ancient cycles recovered in this paper, 781 yrs (eq. 21) & 795 yrs (eq. 11) — interval-lengths agreeing to within 2% — are both (as might be expected, if we are on the right track) a little less than the c.900 yr time-interval (fn 51) from the start of regular Babylonian records (§G1) down to Ptolemy, whose corpus contains both cycles in the same paragraph of the same work. (Note: if lasting Babylonian records actually began with the \(-832-830\) trio, then the first ss ever tracked from start to finish could have been the 974\(^{st}\) 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 its output. In fact, Ptolemy relays, at Almajest 4.9, a report of this very ss’ next-last eclipse, \(+125/4/5, m = 1.8\) digits.)

\(^{115}\) Similar ancient continued-fraction truncation (explaining Eratosthenes’ famous obliquity) at Rawlins 1982G p.262. (Theory initially proposed by Neugebauer in 1943: see DIO 2.1 §3 in 26.)

\(^{116}\) See fn 79 & van der Waerden 1974 pp.103-105, 246-247.

\(^{117}\) The 19\(^{th}\) cycle-calendar’s inventor, Meton, was portrayed as a fake by his conservative contemporary, Aristophanes: The Birds kicks Meton off the stage, and The Clouds accuses new-fashioned-calendar reformers (like Meton) of starving the gods by confusing them about the dates of their eats-festivals.

\(^{118}\) See fn 79 & Rawlins 1991H fn 1.

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

\(^{120}\) Wide range of Muffia slights explored at Rawlins 1991W §H2 & DIO 2.3 §C.

\(^{121}\) At §6 item [b].

\(^{122}\) See fn 108 & DIO 2.3 §B in 20.

\(^{119}\) Neither of these two cycles (781 yrs & 795 yrs) is recognized in van den Bergh 1955 (nor is that of fn 30). Indeed, both cycles have apparently lain unknown for the nearly two millennia to antiquity from the present paper.

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J Testing History-of-Silence’s Archons

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

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

J3 No matter how sensible this request may seem (to the uninitiated), it cannot be acceded to — because Muffia-reaction-to-DR is a rigid proscription-prescription (fn 7). Reason: if a single R.Newton-DR discovery is admitted by Muffiosi to be of potential worth — an epochal event yet breathlessly awaited by DIO readers — then it becomes impossibly difficult to justify a continuation of two decades of religious (fn 124) Muffia condemnation of RN-DR. Better to try pretending (despite multi-layer contextual consistency: §15) that eq. 31’s exact match to Ptolemy is just a fluke — and not just excessively-competent? Muffia slander against important contributions to the very field where outside the Princeton Institute-Muffia can such god-like intelligence be found?

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

J5 OK, it’s not that I have much confidence that any PRIMO scholar possesses the intelligence (primarily a feel for probability) — and the integrity that would be required to pass this test. However, [a] There’s no harm in trying. (Yet again.) [b] DIO wishes to leave behind as clear a record as possible of proud academic archondom’s antibody-rich ability to remain unfiendly immune to evidence, logic, & equity — even for decades on end. (Muffia-vetted, DIO-nonciting B.Goldstein 2003 p.70 tests (fn 119) Hipparchos vs eq.31, but won’t ask: how (eqs.26-31) did 105416&8523 occur in the 1st place? Pass the cotton!) J6

The operative principle here at §J3 is a solid piece of academic-climber-reasoning: in any controversy, the scholar who does business (and soirees) with the most archons, is the one who’s right. No exceptions to this rule can be admitted without implicitly defiling archonal majesty, most dangerously by creating impious infirmity (even skepticism) about whether academe REALLY NEEDS ARCHONS. Remember — in any controversy, the scholar who does business (and soirees) with the most archons, is the one who’s right. No exceptions to this rule can be admitted without implicitly defiling archonal majesty, most dangerously by creating impious infirmity (even skepticism) about whether academe REALLY NEEDS ARCHONS. Remember On the Waterfront’s labor-gangster-boss, Johnny Friendly, reacting to the horror of just one person’s defiance of his gangster-boss, Johnny Friendly, reacting to the horror of just one person’s defiance of his

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

125 See §J3 and/or DIO 4.1 §4 [B4].

126 E.g., fn 141. Also: Rawlins 1991W §J3 & DIO 3 §[L8].

127 Rawlins 1991W §H3 & DIO 2.1 §3 fn 23.
K How Topche Scholars Discover a Lunisolar Cycle

K1 DIO's silly recourse, to deriving Greek lunisolar cycles from Greek methods and actual lunisolar observations, merely shows how amateurish non-Muffia scholarship can get. For a lesson from The Experts (who regularly declare that those who disagree with them are incompetents: [§5 & DIO 2.3 §6[2]], we turn to the paper of Muffia genii B.Goldstein (Muffia capo) & A.Bowen (Inst Res Class Philos & Sci, Princeton - publisher of Princetitute-supported Britton 1992) in the 1995 May Journal for the History of Astronomy. (The paper is a jollygood joke. One trusts the authors knew this.)

K2 Pliny 2.53-54 includes a famous passage on Hipparchos' 600' cycle calculations. G&B propose that everybody else has misunderstood this to mean an interval, when it really (fn 129) means a lunisolar cycle. Thus, their paper's title is: "Pliny & Hipparchos' 600-Year Cycle". Since Muffiosi regard Hipparchos as "virtually a closet Babylonian in Greek drag" (Rawlins 1991W §E1), the 600' period must (fn 27) have a Babylonian[2] origin. ( origin. (Even has adding needless-to-add been so: needless.) Thus, borrowing a notion of N.Swerdlow, G&B start with the well-known Babylonian version of our eq. 30, namely:

\[ V = 120^{22}06'1/3 \]

G&B then round this to the unattested expression

\[ V = 120^{22}06'600 \]

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

\[ 600' = 724^{1} \]

K3 The trivial inconvenience that 7241 is not an eclipse cycle is handled in the most artfully Muffiose fashion: it isn't mentioned. (This, even though the Pliny 2.43-57 context is: eclipses.) Nor is 7241 an anomalous cycle. In fact, 7241 doesn't equal anything recognizable, other than roughly 600' — and even that equality isn't exact enough (as we'll see in §K4) to be worth the slightest notice.

K4 Compare to our genuine & remarkably precise 800' cycle, eq. 20. (See fn 109.) That is, 800' = 9895° - 0°.4; by pathetic contrast, 600' = 7241° + 90° and 600' = 7241° - 13°. As for draconic commensurabilities: 9895° = 10738° + 5°, but 7241° = 8053° + 86°. 129

Bowen 1995 takes it for granted that Geminus & Pliny (emb 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 I use of I use of these tricks will know that this presumptive (R.Newton 1991 §D14) evaluation is merely a [bedrock] fantasy (Rawlins 1991W §E4) of the Muffia cult — lacking the very "independent confirmation deriving from the times in question" which one review (Bowen 1995) requires of non-Muffish. 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 gonna-have-ancient-attestation play in Bowen 1995, cited at §K6 & DIO 4.3 §15[5]. (Bowen 1995 says Thurston ignores real Hist.sci scholars' "rigorous demand for independent confirmation deriving from the times in question" and instead lets "reconstructions — supplement, or be confused with, the data reconstructed."). Evidently, G&B consider the explicit 600 yr figure the sort of "rigorous" attestation which lesser scholars lack! — even though the 600 yr interval has long been rightly (DIO 1.3 fn 211) recognized by the Muffia's saner Neugebauer & Toomer as (not G&B's sexagesimal-expression by-product but) simply the time-span from the famous epoch Nabh 1 down to Hipparchos' epoch. In any case, we thank the JHA for publishing yet another precious, yet highly efficient demonstration (better than our own JHA could) the possible inductivity-stereotyping of G&B's much-touted historical method. Incidentally, assuming that the catfight I witnessed at the end of the 1994/5/8 Dibner Inst conference was real, it would seem that this avenue is a dead end — except for the publications of scholars in the era of high ancient science, whose research-tradition was no longer carried on. (I.e., the original Babylonian data may not have survived except in later works' spurious secondary citations of them.)

L The Long View

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

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

L2 The antiquity of the data used by these scientists has here been shown to be at least a century older than the earliest data used by Ptolemy: the 721-720 BC trio. L3 The Greeks' resulting awareness of far longer eclipse cycles than previously suspected has been demonstrated beyond reasonable doubt.

L4 However, there is an implication in the foregoing that may escape notice if not highlighted here. A striking aspect of what we have been learning — both above & in Rawlins 1985S — about the Greeks' discovery of lunar period-relations, is that the same few oldtime eclipses keep turning up (e.g., fn 52): ~830, ~720-719 (trio), & ~620. This suggests that either [a] the Babylonian records available, to Greek scientists of the 3rd-1st centuries BC, were a lot less complete than Ptolemy indicates ([§G1]; or, [b] the records were then pretty full (fn 79 iii]), but the eclipses not selected for analysis & publication at this time were lost131 to later investigators (fn 133).

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

L6 Again ([§L4], the most obvious explanation is that there were very few 6th-7th century BC data available to (classical-era) Greeks. L7 Another potential implication: little valid new lunar period-relation research occurred any 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

128 Bowen 1995 takes it for granted that Geminus & Pliny (emb added: "undertook to assimilate Babylonian celestial science in a cognitive structure that adhered to Greco-Latin requirements of what counted as proper science.

129 See, e.g., the gonna-have-ancient-attestation play in Bowen 1995, cited at §K6 & DIO 4.3 §15[5]. (Bowen 1995 says Thurston ignores real Hist.sci scholars' "rigorous demand for independent confirmation deriving from the times in question" and instead lets "reconstructions — supplement, or be confused with, the data reconstructed."). Evidently, G&B consider the explicit 600 yr figure the sort of "rigorous" attestation which lesser scholars lack! — even though the 600 yr interval has long been rightly (DIO 1.3 fn 211) recognized by the Muffia's saner Neugebauer & Toomer as (not G&B's sexagesimal-expression by-product but) simply the time-span from the famous epoch Nabh 1 down to Hipparchos' epoch. In any case, we thank the JHA for publishing yet another precious, yet highly efficient demonstration (better than our own JHA could) the possible inductivity-stereotyping of G&B's much-touted historical method. Incidentally, assuming that the catfight I witnessed at the end of the 1994/5/8 Dibner Inst conference was real, it would seem that this avenue is a dead end — except for the publications of scholars in the era of high ancient science, whose research-tradition was no longer carried on. (I.e., the original Babylonian data may not have survived except in later works' spurious secondary citations of them.)

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

131 Since the Enlightenment, a primary criticism of Ptolemy has been similar: he should have published more data than theories. The suggestion here (at [b]) is that he was not the only ancient (or modern) guilty of this oversight. 132 The reason that 569 yr-cycle eclipse-pairs occur more often than 795 yr-cycle pairs is that the 18th draconic remainder in eq. 12 is critical less than the 22° remainder in eq. 11. (Because the latter remainder is so near the 25° outer limit of pair-possibility, 795 yr-cycle-eclipse-pairs tend to occur near perigee and are impossible near apogee.) By comparison, since a remainder less than 19° will permit eclipse-pairs for any anomaly, this generous condition applies to 569 yr-cycle pairs (given eq. 12's 18° remainder). Thus, there will be no centuries-long periods when such pairs are virtually nonexistent, as we found was true for 795 yr-cycle pairs: [§F1L].

133 However, it is possible that whatever old Babylonian data still survived were (by Ptolemy's time) mere hand-me-downs (14A), effectively selected from ancient centuries considered too extreme even by the publications of scholars in the era of high ancient science, whose research-tradition was no longer carried on. (I.e., the original Babylonian data may not have survived except in later works' spurious secondary citations of them.)

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