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• Publisher & journal cited (1996 May 9) in *New York Times* p.1 analysis of his discovery of data exploding Richard Byrd's 1926 North Pole fraud. [*DIO* vol.4.] Full report co-published by University of Cambridge (2000) and *DIO* [vol.10], triggering *History Channel* 2000&2001 recognition of Amundsen's double pole-priority. New photographic proof ending Mt.McKinley fake [*DIO* vol.7]: cited basis of 1998/11/26 *New York Times* p.1 announcement. *Nature* 2000/11/16 cover article pyramid-orientation theory: *DIO*-corrected-recomputed, *Nature* 2001/8/16. Vindicating DR longtime Neptune-affair charges of planet-theft and file-theft: *Scientific American* 2004 December credits *DIO* [vols.2-9]. *DIO*-opposites mentality explored: *NYTimes* Science 2009/9/8 [nytimes.com/tierneylab].

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• New findings on ancient heliocentrists, pre-Hipparchos precession, Mayan eclipse math, Columbus' landfall, Comet Halley apparitions, Peary's fictional Crocker Land.

• Entire *DIO* vol.3 devoted to 1st critical edition of Tycho's legendary 1004-star catalog.

• Investigations of science hoaxes of the -1^{st}, $+2^{nd}$, 16^{th}, 19^{th}, and 20^{th} centuries.

Paul Forman (History of Physics, Smithsonian Institution): "*DIO* is delightful!"

E. Myles Standish (prime creator of the solar, lunar, & planetary ephemerides for the pre-eminent annual *Astronomical Almanac* of the US Naval Observatory & Royal Greenwich Observatory; recent Chair of American Astronomical Society's Division on Dynamical Astronomy): "a truly intriguing forum, dealing with a variety of subjects, presented often with [its] unique brand of humor, but always with strict adherence to a rigid code of scientific ethics. . . . [and] without pre-conceived biases . . . . [an] ambitious and valuable journal."

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."

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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."

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DIO

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#tqzs) 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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DIO & The Journal for Hysterical Astronomy:

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.)[1]

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 §O2 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 *DIO*s going to libraries at this time all received the correct edition.[2]

[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* fn 127), B feels this is unfair to ty-rex, who's a cuddly puppy next to the flesh-eaters of modern academe. After all, there is no record of a species other than *homo superbus contumax*, which consciously aimed (*DIO 4.3* ‡15 §I3) at the EXTINCTION of a fellow species.

[2] [Before *DIO 6*'s shipment to the great majority of our library subscribers, the 1997 printing has adopted slight improvements to the calculated ancient eclipses cited in ‡1. Though the several corrections 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.]

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DIO is primarily a journal of scientific history & principle. At present, a good deal of *DIO* copy is written by Dennis Rawlins (DR) and associates. However, high scholarship and-or original analytical writing (not necessarily scientific or historical), from any quarter or faction, will be gladly received and considered for publication. Each author has final editorial say over his own article. If refereeing occurs (only with author's explicit permission), the usual handsome-journal anonymity will not — unless in reverse. There are no page charges, and each author receives at least 50 free offprints.

The circumstance that most *DIO* articles are written by scholars of international repute need not discourage other potential authors, since one of *DIO*'s purposes is the discovery & launching of fresh scholarly talent. Except for equity&charity reply-space material, submissions will be evaluated without regard to the writer's status or identity. We welcome papers which are too original, intelligent, and-or blunt for certain handsome journals. (Dissent & controversy are *per se* obviously no bar to consideration for *DIO* publication; but, please: spare us the creationist-level junk. I.e., non-Muffia cranks need not apply.)

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

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

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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 throats of the frailer 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 fanging *throats*. (And where were both fatal wounds delivered? *Think* about it. Beginning to understand why no weapon was found?)

B4 So, *DIO* now scoops the truth behind the headlines: Saint OS is *covering for someone else* (just like F.Lea's other too-kind client: §A4), namely, his wuvable woofwoof pal Kato — who has a pathetic Special-Problem. (Were-dogs just can't help themselves.)[6]

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[7] — 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*[8] Moon but at Moon*Set*. (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[9] 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*.[10] The sky don't lie — you can't deny.

[6] Since were-dogs don't know 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.

[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 — and the verdict at trine.

[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-windtunnel with Kato's.) I went over to shake paws, and we chatted pleasantly for a minute. (An extremely likable fellow. Who will not resent DR pulling his leg & wagging his tale.)

[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 31

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

by Rawlins[2]

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

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

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

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

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

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

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

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

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

Muffia Omertà

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

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

Gnawing Holes in the Dike

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

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

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

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

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

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

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

[8] See below, fn 137.

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

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

‡5 Hero & Doppelfanger: A Shaggy Were-Dog Story

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

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

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

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

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

A4 The case will now solve itself, once we add-in some background: [a] OS's lawyer[2] first entered bigtime celebhood (this was even before he became a feminist publisher)[3] when he got murderer[4] Sam Sheppard off by inducing Sam to suddenly recall[5] 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.

[1] One is tempted to suppose that the werewolf is a *West* Hollywood creature, since there has only rarely been a female film-werewolf. (Though, see J.G.Melton *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.) But this apparent gender-bias may instead be merely an unintended side-effect of the US' lucrative depillatory industry's fashist veto-power over female imagery in films: after all, how saleably-charismatic would the public find a film werewolfess (*Mad Magazine*, are you ready for this?) — entirely covered with hair, excepting legs and armpits?

[2] F.Lee Bailey (*The Defense Never Rests* 1971 pt.1 ch.5, re yet another homocidal 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 *Playboy*-clone punnishingly 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: *ibid* pt.2 ch.1), suspect's infidelity, no time-alibi when wife killed, didn't testify before jury that released him, "real killer" never apprehended, police-competence&integrity put on ferocious trial. Sheppard's story: he saw an unrecognizable assailant (of indeterminant gender!) who got into his bedroom and killed his wife but merely injured Sam. (Jeff McDonald wife-murder: close copy. Chas. Stuart similar.) Curious "real" murderer: killed wife but permitted Sam to live because he knew that he had hit Sam *just* hard enough to destroy the precision of Sam's memory (of someone of allegedly "white form" or in "white garment": *idem*).

[5] See S.Sheppard *Endure & Conquer* 1966 pp.299-300, 310-311, 318. He promised (FLB 1971 p.2 ch.2) to work for 10^{y} after release, to pay off FLB's fees but (after converting from book-author to pro wrassler) he escaped by dying of alcohol & pills at age 46, after only 3 years of "freedom".

C7 Defense attorney Rob't Blasier (1995/10/3), alibiing his huge fee: "People who don't have alot of 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 Guinier[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 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 under*stand* 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.

[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.

[21] *Face the Nation* (1995/10/8). (Emph entirely hers.) Similarly schiz editorial by *MS* Editor-in-Chief M.Gillespie at *MS 6.3*:1 (1995/11-12).

[22] [This reasoning could (just as fallaciously) alibi 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-followup 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?]

[23] Note the observations of Christina Hoff Sommers *Who Stole Feminism?* NYC 1994 pp.97-98 & 109.

[24] See the astute comments of Jos. Agassi (*Centaurus 37.4*:349 [1994]) & Paul Feyerabend (*Studies Philos & Hist Sci 26.3*:353 [1995; from c.1961] pp.356, 381, 385, 387).

[25] See, e.g., F.Crews *The Memory Wars: Freud's Legacy in Dispute* NYREV books 1995 p.62 n.24.

Summary

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

A Hipparchos' Most Reliable Eclipse-Interval

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

a genuine hero, Ron Goldman.) I criticize Liberals[14] for projecting their mentalities onto others. But, in this case, I (more than once) did the very same thing. DR's most persistent failing is to wish-trust[15] that people will think fairly & rationally. Experience (outside my own circle of valued friends) repeatedly shows otherwise, but it's part of my incurable optimism to keep looking (*DIO*genes-like) for an exception to my own cynicism — because that is what I myself strive to be.

C OS Doubletakes

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

C2 Jo-Ellan Dimitrius, OS' charming-fox hireling-jury-slanter,[16] slipped & told a truth: "I'm a true believer in the jury system in the United States."

C3 It was obvious from the outset that the two OS murders would lead to a total incarceration of at least ten years. For the jury.[17]

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

C5 But the worst (& most doubletake-worthy) prediction was by DR, who — neglecting Mencken's dictum [no one ever lost a buck underestimating the US public's intelligence] — flippantly supposed (*DIO 4.2* ‡9 §F3) that even a cheap idiot-prosecutor couldn't lose all 12 jurors. (The suggestion's only plus: same verdict for alot less public cost.)[18]

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

[14] E.g., fn 26. The irony is that the admirably decent Liberal publisher Chas. Peters was all along far wiser than DR on the trial's outcome. E.g., he led off the 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 in orig): "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 were on the jury, HE WOULD VOTE NOT GUILTY. When I asked why, he replied, 'If it was good enough for the Menendez brothers, it's good enough for [Simpson]. And besides, I know the LAPD isn't above framing a suspect.' [DR: note that this last point's relevance to the OS case is contradicted by his previous (cop-independent) reasons for realizing OS' guilt.] I can't stress enough that this was a very level-headed guy saying this. The prosecution is doomed, I think." On 1995/10/13, my black (defense lawyer) neighbor said she was delighted at the OS verdict regardless of his guilt, because OS' conviction would have enforced the "stereotype" of blacks as criminals. (If a white mob cheered the release of a white killer for similar reasons, this 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 Muffiosi who are quietly embarrassed by their own cult's misbehavior (‡1 fnn 3&5), but I expect no public confirmation of that DR optimism.

[16] Dimitrius used potential jurors' handwriting (*Newsweek* 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-filterers systematically dummify US juries?)

[17] Total including rejects & alternates.

[18] But the Clark-Darden experiment was better: mass-racist black-cheering of a wifebeating killer's release was an invaluable wakeup-shock to even the densest traditional civil-righteous Lib. It may assume the same rôle which N.Chamberlain's 1938 peace-in-our-time naïvete served, for 1938-1940 observers (who might have blamed England for war, had hostilities started in 1938): when give-'em-enough-rope 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 & Cochrans, one couldn't today convict even JFK's killer, L.Oswald.)

[b] Most US inner city blacks have relatives who've been arrested by unsmiling police. (On the other side: many have relatives who've been knifed.)

[c] If we discount the influence of OS-team money corrupting the press, then: just for *entertainment* (profit) purposes, the media fanned more public paranoia[10] 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 sportsbiz-style gotta-watch cliffhangerhood. (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 defense lawyers — i.e., *the* most ingeniously deceitful species produced by millions 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 revulsion from the very TV-ad-execs who demanded an OS cliffhanger?)

[d] Given the kinds of politicians US whites elect, they are in a weak position to condemn blacks who fall for plausible con-men like OS' chief-lawyer Johnny Cochran, who clearly deserves rapid elevation to: Congressman JC. If one believes a 1995/10/3 ABC-TV 'snews poll, only 70% (±4.5%) 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,[11] one wonders: is the remaining 30% any relation to the [data-mass-discounting & nit-obsessing] paranoid bunch who believe that WW2 Germany didn't systematically kill millions of Jews?) Taking this number to the 12^{th} power, we find that the odds of picking a unanimous guilty-voting jury, even from whites, would be barely 1%! The odds of finding such a dozen in the black community is found by taking 17% or 18% (§C8) to the 12^{th} power: 1-in-a-*billion*. Yet despite this, I hoped[12] that my long discouragement at black culture's mean state[13] 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 bod 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 suggest 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 establishmentaryans, 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.)

[11] See *DIO 2.1* ‡1 §A1.

[12] I did better in the S.Smith 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 occasional evidence-planting-cop — rather than the pathology of inner city neighborhoods — is akin to blaming a rotting bandaid for a deep, life-threatening wound.) 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 lawyer-protected mobster-druglords corrupt the police, then: why be surprised if many idealist cops depart — and the LAPD's remains are alloyed 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 *any*one for immorality is the height of comedy.) Further, given spectacular nationwide black crime rates: attempting to expunge statistical race-postjudice in cops is akin to asking them 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 logic to all groups except cops. (Since TV 'snews 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-refreshment of race-hate target-practice.)

fundamental[22] Babylonian ratio of yearlength[23] to monthlength:

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

Almost.

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

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

$$4267^{u}/(12^{u}22'08''/1^{y}) = 344^{y}58'42''\ldots = 345^{y} - 0^{y}01'17''\ldots = 345^{y} - 7^{\circ}42' \tag{5}$$

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

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

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

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

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

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

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

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

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

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

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

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

C1 On 1995/4/23 (even while editing this paper), I independently came upon an explanation of the $-7^\circ 1/2$ 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 geocentrists, of the Sun). Note well: ancient eclipse-analysts would have had more motive than anyone to know the motion of the Earth's apogee, since (as Ptolemy says at *Almajest* 4.2) solar anomaly-inequalities hindered their search for integral-return eclipse cycles. (The smallness of the solar remainder in eq. 1 was primarily what made it preferable to eq. 11 or eq. 12 — and to the various cycles cited in fnn 19&57.)[30]

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

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

[29] Neugebauer 1975 p.312 seeks solace by citing an attribution of -8° to Hipparchos in an unreliable (*ibid* p.310 n.6) late Greek source. But -8° is a rounding of $-7^\circ 1/2$, 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 $-7^\circ 1/2$ remainder could be Greek.

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

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

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

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): heeee'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)[5] 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 superlative,[6] namely: *the* clumsiest torture-murderer[7] ever to be voted innocent. (Thanks to the master truth-shaders of the US lawyer-clique — and its transformation of justice-seeking from a noble profession into a dark art.) I.e., OS' once-lovably-inept "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[8] jury&court system. My own contribution in this direction — a semi-facetious hung-by-the-jury-until-you-are-dead scheme (*DIO 4.2* ‡9 §F) — in truth was as short-sighted (& 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[9] 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] overswiftly 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 (1995/10/3) gratuitously opposed 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 carefully-reasoned prosecution-case): "garbage in, garbage out." Does anyone seriously believe that mean US juror IQ (white or black or whatever) is

[5] Barabbas may have gotten a bad deal from conventional history. He is just called a "robber" by John 18.40. But Mark 15.7 ascribes to him (murderous) insurrection. Luke 23.19 calls it sedition. (Matt 27.16 says he was "notable".) It sounds like he fought brutal Roman oppression openly & directly instead of with Jesus' doubletalk (e.g., Matt 22.21). So, was the Jewish mob right to pardon him instead of Jesus?

[6] Besides already being the most famous pre-event domestic-killer of the century. The nearest thing previously: Booth also was a top celebrity-actor before killing Lincoln on 1865/4/14. That's showfolk.

[7] OS' technique was reputedly used upon the 9th Caesar by the 10th's troops (Suetonius *Vitellius* 17): "The soldiers put [Vitellius] through the torture of the little cuts before finally killing him".

[8] Our rich rulers aren't going to overhaul a court system that favors the rich. So, instead of reform, justice will be determined by the elite media, case-by-case. I.e., OS will be damned individually. Lawyer Cochran's overkill ploy of using Nation-of-Islam troops offended the Jewish lobby. Thus, one outcome of the OS case might be a realignment: that lobby switching from its traditional alliance with blacks to a more genuinely progressive link with the women's movement.

[9] Neglecting interest & inflation.

‡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 Indian[1] — 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 Smith[2] (the South's answer to drought) and OSimpson (Hertz Corp. boardperson & NBC-TV commentator)?

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, alibiing his own failure to prevent the suicide by protesting in anguish, "This did I *fear*, but thought he had no weapon." (Emph added)

A3 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, avoids threatened wildwest-lynching, by suddenly grabbing a gun, putting it to his head & counter-"**threatening**" to: do exactly what the crowd had wanted to do, namely, kill Cleavon Little. The crowd shrinks back: Careful,[3] men — he means it. . . . (Cleavon after escape: boy, are these whites DUUUUMB.) Question: if Shakespeare was really F.Bacon, then: was F.Bacon really Mel Brooks?

A4 When audiences first encountered the Mel Brooks DUUUUMB 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 Dezzy-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.[4] 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.Yeager, who was a close friend of my late father (Lou 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 U.Md & 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 Yeager.

[2] Smith is the me-first social-climbing S.Carolina yuppie who drowned her innocent, terrified, strapped-in children in her car — and then tried to drown the rest of the neighborhood in crocodile tears. My wife Barbara suggested right after the S.Smith confession that the US has become so lawyerized with alibi-artistry that Smith might try excusing her deed on the Grouchoesque 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 dramatics 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, explaining (as recounted by police): "Their future would be pitiful with no mother and a father who was a criminal."

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

[4] And, after a dedicated 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 shove-it. (After encountering similar nit-logic from Ivy League Muffiosi, in reaction to similarly meticulous DR high-tech inductive reconstructions of ancient astronomy, I am more aware than most that cultist prejudice's damage to clear thought is a problem not restricted to ethnic ghettoes.)

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

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

C5 *Almajest* 4.2 treats the $-7^\circ 1/2$ remainder in eq. 1 as if it is longitudinal; not only longitudinal but: *sidereal* longitudinal. (See *Almajest* 4.2: "with respect to the fixed stars"; transl. of Toomer 1984 p.175.) This is patently inconsistent with Ptolemy's solar orbit, whose $65^\circ 1/2$ 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, $-7^\circ 1/2$ would be solar anomaly, not longitude. Startling fact: *the $-7^\circ 1/2$ remainder is correct for solar anomaly, not for longitude.* Only one potentially contentious question remains: was this correctness due to Greek skill or to luck? If the former, then high Greek astronomy was more advanced than previously believed by anyone — most definitely including myself.

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

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

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

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

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

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

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

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

$$G_s = 365^d 1/4 + 1/100 \tag{6}$$

and divide it into eq. 1:

$$4267 M_A / G_s = 345^g - 7^\circ 33' - \tag{7}$$

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

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

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

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

C13 And this adds more credibility to the position that the famous geocentrist astrologers, Hipparchos & Ptolemy, drew much of their astronomy from often-unnamed[44] but able[45] astronomers (probably heliocentrist)[46] — not politically well-connected — whose continuous-function solutions fit all the 3 trios of *Almajest* data under discussion in the *JHA* paper and (§D5) that all 3 of these orbit-solutions are derived & presented[21] in *DIO 1.2-3*.

H4 Adding to these credits, the *Journal for-the-History-of-Astronomy* has also refused even to inform its readership of the existence of *DIO 3*'s 1993 Tycho star catalog, published by DR, with whom Hoskin still insists on total noncommunication — a unilateral breach that persists (it's been over 12 years) only & entirely because Hoskin is afraid that ending it under criticism might create a Wicked-Witch-style meltdown of a long-nurtured image of: Bigness. He remains sadly short of understanding the deeper meaning of the word.

H5 It's inspirational to realize that (contra the naïve mild optimism[22] of §A5), despite the daunting challenges presented by the *JHA*'s ghastly "Muffia Orbituary" (*DIO 1.2-3*) episode, nonetheless, that curious journal's degree of devotion to integrity, impersonal equity, *and astronomical history* has overcome all — and thus (§E3) miraculously emerged essentially intact.[23]

I Postscript C: Priorities & Sentences

I1 Hoskin's last letter (1983/3/3) before condemning DR (3/21) to an indeterminate[24] 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 final sentence.

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

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

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

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

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

[43] Rawlins 1991H fn 1.

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

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

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

[21] See *DIO 1.2* fn 33.

[22] See the more prescient suggestion at *DIO 1.2* fn 30: "In reaction to Hist.sci's current ['Muffia Orbituary'] incident, no Hist.sci institution will effect any changes beyond the cosmetic."

[23] The standing causes of Muffia niggardliness & non-motion are discussed at *DIO 4.3* ‡15 §§E4&E7.

[24] *DIO-J.HA 1.2* §B3. Note: Hoskin hasn't many more years left in which to fulfill the *JHA*'s written 1981 agreement (fn 12) to publish Rawlins 1999.

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-lost 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 inexcusable behavior, and [ii] refusing to publish Thurston's very brief §H1 notice, instead publishing Hoskin's own ultra-brief correcting note (pleading carelessness,[20] contra §§C3-C5 and §§D7-D9), thus, directly refusing to publish an unambiguous notice that DR has contributed significantly to scientific history. (*JHA* can hardly claim that it has made DR's contributions clear, when *its own Editor* pleads so much confusion about the matter, that *JHA* has had to correct itself!) The latest *JHA* note could've simply said that "As Rawlins shows" was meant. But Hoskin sees black at the middle word: after years of Hoskin attempts to 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 vital shortcoming here is not of math but of character.

H3 Hoskin's §H2 gyrations add to those described earlier here — and have the same bottom lines: embarrassment-minimization, & the *JHA*'s squandering yet another opportunity to partake of the bracing & cleansing experience of honest, open, uninhibited generosity. (In extenuation: [i] Even though posing as the type of deity known as "editor", Hoskin is human; no one enjoys self-embarrassment. [ii] He has, for years, had untrustworthy advice from archons whose eminence superficially implied trustworthiness, and whose hefty censorial bigotry constrained editorial options.) Plain facts: [a] Hoskin tampered with Thurston's text in a way that tended to cover *JHA* shame. [b] *JHA* then prevented correction of this inexcusable alteration by failing to send proofs to Thurston. [c] Now, it must prevent publication of the further §H1 embarrassment — failing yet again to print (as DR repeatedly urged: fn 7 & *DIO 4.2* ‡7 §B42; see also §§D5-D6) clear *JHA* acknowledgement of *DIO*'s undeniable priority in proving that (contra *JHA*'s lead paper) Greek-style high-technical-level research texts were much less widely circulated and thus now lost (Rawlins 1984A p.984).

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

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

D Doubling the 345^g Interval

D1 Returning to the question of the empirical base for eq. 1: whether Hipparchos was discovering or checking it, he might (fn 18) have preferred having as long an interval as possible, since that is astronomers' secret for determining accurate heavenly motions. The catch with the eq. 1 cycle is that (as Ptolemy correctly points out at *Almajest* 4.2) the number of draconitic months corresponding to 126007^d01^h is not quite semi-integral: as one sees from eq. 1, it misses precisely $4630^w 1/2$ by over 11°. Since (even for lunar perigee) an eclipse can only occur within c.12° of a lunar node, there will be few eclipses having a 345^g-cycle-match two cycles ago: 690^g into the past. (Odds against: ordmag 1-in-100.)

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

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

[19] Indeed, despite the Muffia Orbituary disasters (and DR's perfect-fit Hipparchos-based solutions to all the 3 data trios involved: §H3), Muffiosi continue lockstep-swearing that the solar data of eclipse-trios A&B are Babylonian: *DIO 4.3* ‡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 *incuria*."

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

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

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

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

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

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

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

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

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

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

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

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

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

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

[54] We are not sure whether Hipparchos found his lunar tables' anomalistic month $V_{\rm H}$ (eq. 8) — & thus anomalistic motion $v_{\rm H}$ (eq. 9) — from the $M_{\rm A}$-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 $M_{\rm A}$-multiple method. See fn 110. (Also: fn 56 & fn 79.)

[55] More precisely: $-11''''46'''''39'''''''$ (explained at *Almajest* 4.7). Note the discerning comments of R.Newton 1977 p.179 on the transparency of Ptolemy's pretense to empiricism & originality at *Almajest* 4.7. (But see here at fn 61.) RN's charge is bolstered by our realization that the Ptolemy $v_{\rm J}$'s actual error (ordmag 1 part in a million) is ordmag 10 times larger than the above-cited tiny ($-12''''$), supposedly-empirical difference between Ptolemy's $v_{\rm J}$ (*Almajest* 4.3-4&7) and Hipparchos' $v_{\rm H}$ (eq. 9)! (Ptolemy's shift from $v_{\rm H}$ to $v_{\rm J}$ = $13^\circ03'53''56'''17''''51'''''59'''''''$ made a very slight improvement in accuracy, though his analogous *Almajest* 4.2&9 correction, of Hipparchos' draconitic motion, degraded its accuracy by a similarly trivial amount. However, it is hard to fault Ptolemy in the latter

(1995). [c] Direct communication (199?). [d] Acknowledgement of some heresies' truth (20??). [e] Acknowledgement of their value to the field (2???).

E6 I remain (as always)[17] prepared to sit down with Muffiosi (not in terrified-Muffie [*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 unrenounceable as the years pass.) One side continues, as ever (for over a decade: fn 17), to be ready for this. Just as one side has for years repeatedly acknowledged the value of some of the other's output (despite receiving largely noncitation and hit&run sniping in return). And too much of the Hist.sci community continues, as ever, to despise-exile the accessible, appreciative, & non-party-line side.

E7 Indeed, during the 2 decades of the ancient astronomy Controversy, not one Hist.sci scholar has ever once expressed a word of appreciation for DR's consistent policy of praising & encouraging (& pointing out his intellectual debts to) the valid work of snobster-enemies, even those attempting to murder his career (*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-Orbituary 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* ‡1 fn 25 & Rawlins 1991W §O8. DR continues to await its consummation — and Hoskin's attainment of sufficient maturity to communicate with DR.) Further: the following §§F2&F3 compliments reflect some credit also upon Hoskin, since the *JHA* printed the material.

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 — this despite the fact that this paper was none too gentle on him & his Muffia colleagues. (I.e., it was written rather in the fashion[18] 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] *DIO 1.1* ‡3 fn 7, *DIO 1.3* fn 269.

[18] With the critical difference that *DIO* explicitly recognized the worth & high scholarship of some of Jones' work (*DIO 1.2* §J4) 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 untenured Hist.sci scholar dares submit papers to *DIO*, for fear of cult ostracism.)

E2 We understand that K.Moesgaard (Univ Aarhus, Denmark) is reluctant[12] even to review our Tycho star catalog (*DIO 3*, 1993) — which he privately deems valuable — so long as it contains anything[13] 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 archonal aloofness & conference-exclusion (and years of Hist.sci's total-blackout-noncitation 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[14] negative 1995/6 *Isis 86*:309 Muffia review of the work of H.Thurston, who'd disobeyed Muffia orders not to support or even cite DR: *DIO 4.3* ‡15 §E3.

E3 As I have made known to several scholars recently, the Muffia&clo seem to have no wish (fnn 7&22, ‡1 §I2) 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., §I, ‡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* fn 280.] Nothing[15] has come back. After two decades of such, I'm catching a trend here.)

E4 By this time, Muffiosi know just as well as DR that: [a] their horrid behavior will be embarrassingly repulsive[16] 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 gesturelets 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

[12] This, though he (unofficially) helped inspire a last-minute 1994/6/30 phonecall to DR from the well-intentioned Editor of the eminent (& very high quality) Danish series *Acta Historia Sci Nat&Med* (which has published Neugebauer & Pedersen), offering to [a] publish the *DIO* Tycho catalog, [b] distribute thousands of advertisements, & [c] make DR famous. [Curious. DR never sought such spectacular intercession. *Index Librorum Prohibitorum* Vat City 1948 p.xv: It is the faithful's duty to report dangerous (fn 13) literature.] Contracting for this *Imprimatur* would risk: [i] "editing" at the hands of those who will condone no defilement of Muffia majesty (§E2), & [ii] removing [credit from *DIO* &] control (see *DIO 2.1* ‡3 fn 8) of the work's date [or even act] of publication into said hands. (DR just went with imminent distribution of the uncensored *DIO 3* rendition of the Tycho catalog.) DR had already been through this process before (again: *instant* DR-assent requested), with Moesgaard & *JHA*, as regards the latter's written 1981/9/17 acceptance of a paper now known as Rawlins 1999: see *DIO 1.1* ‡1 fn 25. (On the former, see: *J.HA 1.2* fnn 56&170 & *DIO 4.3* ‡15 fn 41.) Over 10^{y} later, the paper remains unpublished. (General rule: don't rush into publication-offers connected to cliques who've spent years ignoring and-or slandering everything you've *already* published. Approaches are more convincing when they aren't flagrantly cart-before-horse [e.g., *DIO 1.1* ‡1 §A9, *DIO 4.2* ‡7 §B42]: i.e., partial-respect citations come *first*, & then *later* we get to publication-cooperation.)

[13] E.g., the upsetting if critical revelations at *DIO 3* fn 54, fn 141, §§L8-L11, §§M4-M5 (D675-80, 971, 1001-4).

[14] *DIO 1.2* fn 92. [A *Muffia* complaint of insufficient notice to *its* literature is funny and-or nutty: *DIO 1.2* §I14.]

[15] See *DIO 1.2* fn 25! Near-exceptions (all late 1970s): a return-phonecall monolog from Britton, a too-busy-to-check-anything note from Neugebauer himself, and a contentless note from Swerdlow's secretary. More recently: at the Dibner conference, Alan Bowen creditably attempted (evening of 1994/5/7) to have a leisurely conversational exchange of ideas privately with DR but was swiftly warned by B.Goldstein to cease.

[16] A point of absolutely null weight for scholars [i] primarily interested in present funding, & [ii] having complete [& revealing] trust in a corrupt system's flawless capacity for shrugoff-repelling considerations of reason & equity. Indefinitely (§I).

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

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

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

$$3277^{u} = 3512^{v} \tag{10}$$

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

E7 Note: the actual interval between the 2 eclipses of §E6 was 290315^{d}02^{h} (5^{h} shorter than 9831^{u} in eq. 11). Division by 10536^{v} (eq. 11) produces V = 27^{d}.554583, ordmag

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

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

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

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

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

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

achievement, which used the same Greek-trig-orbit idea to solve **all three** of Hipparchos' eclipse trios.

D6 Regarding ref-comment §D2, on historical support of Greek-trig solar solutions: [i] All three orbits (fitting trios A, B, & C) are shown to be founded upon seasonlength data which are historically connected to Hipparchos. See *ibid* §M5 (trio A), §§K4-K9 (trio B), & *DIO 1.1* ‡6 §§C6f (trio C). [ii] Moreover, the long-mysterious amplitude of the error curve of the zodiacal stars of the (*late Hipparchos*) Ancient Star Catalog is perfectly matched by the amplitude of the error curve of the (*late Hipparchos*) trio C orbit. (See *ibid* §§F3-F5.) [iii] Finally, the *Almajest* 5.5 mean longitude of the unrecomputed 2nd position of trio C agrees on the nose with the *DIO* solution for trio C. (See the astonishing match at *ibid* §H5.) Again, *JHA* readers are not informed of any of this — *despite the urging of Hoskin's chosen referee that such material be brought in.*

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

D8 The results of Hoskin's behavior (which placed accuracy not quite atop the *JHA*'s list of priorities [§I]): [a] A potentially confusing slip[9] never got corrected. [b] An astronomical immortal's name was mis-spelled. [c] The 2nd observation's time of day was 5 2/3 hours, but the *JHA* printed it as 5 hours. (Such errors will create problems for any *JHA* reader who tries to check the math of the situation. But, then, as Thurston has often pointed out, Hist.sci readers seem to be an amazingly trusting lot: whereas he instinctively checks out numbers in papers [including DR's] while reading them, this appears to be a rare trait — which Alex Jones & DR are one in admiring Thurston for.) [d] An erroneous attribution was inserted (§D9).

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

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

[9] Thurston trustingly took the year-numbers of the three observations from p.415 of O.Pedersen's valuable (as Thurston rightly notes) but error-riddled (*DIO 1.1* ‡5 fn 6) 1974 *Survey of the Alm*; however, all these numbers are low by unity. Thurston intended to make such corrections when the proofs arrived. Which they never did. Fortunately, Pedersen's flub has no effect upon deduction of eccentricity & apogee (which are the only elements Thurston treats).

[10] The whole point of the 1991/5 *JHA* paper (which the Thurston note undoes) is the claim that Greek eccentric motion will NOT account for the data.

[11] A.Levine (ContribEd, *WM*) "Have You Got What It Takes to Write for the *Washington Monthly*?", *WM 21.1*:54. (Editor Chas. Peters not only had the integrity and self-critical humor to run this in 1979 — he then *re-ran* it in the 1989/2 issue). "Writers for *The Washington Monthly* sometimes complain that we're a little too, shall we say, uninhibited, about urging our point of view upon them — and their manuscripts. . . . [Are you sitting at home wondering:] How can *I* become a *Washington Monthly* writer? . . . Could I get published in your magazine? Welcome aboard! Our top-notch editors will be glad to add [to your ms] . . . loads of *thought-provoking opinions* without troubling you with the dreary task of doing it yourself. Many contented writers have said that there's no surprise quite like seeing a manuscript of theirs end up as an article in *The Washington Monthly*. Often they find themselves espousing ideas they've never even heard of, much less agree with."

C3 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 (& published: *DIO 1.1* ‡6) the orbit which fit trio C.

C4 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.

C5 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 flubbadubs.) 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 §I2]), and its critics are delighted to watch *DIO* keep bloodying that nose.

C6 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.

D Secrecy's Costs, or: How Not to Publish a Retraction

D1 Though the main initial referee report on Thurston's paper was something of a credit to the *JHA*, Hoskin's close-to-the-vest approach then undermined the opportunity for progress — and even (§D8) needlessly degraded accuracy a bit.

D2 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.
>
> **D3** 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].

D4 Had the *JHA* merely possessed the emotional calmness to seek input from an exiled (§I & *DIO 1.2* §B3) party, Hoskin would've had the answers to these comments.

D5 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 bear on 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*

$0^{s}.1$ different from reality (fn 12), — i.e., much more accurate than either eq. 8 or eq. 13. Therefore, eq. 10 could be a remnant of the ancients' very best value of the anomalistic month V.

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

F The Precessing ss-Bound anomalistic-Triangle

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

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

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

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

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

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

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

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

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[65] The three cited eclipses' lunar mean anomalies were: $+1^{\circ}$ ($-830/2/4$), -1° ($-140/1/27$), & $+1^{\circ}$ ($-36/12/7$).

[66] See *Almajest* 4.2 or Geminos 18.6. The $10^{\circ}2/3$ remainder is rendered as $10^{d}2/3$ at Neugebauer 1975 p.502, from dividing the Babylonian yearlength/monthlength ratio 12;22,08 into 223. (The discrepancy once temporarily misled a highly eminent Hist.sci referee.) Speculations on the original Babylonian figure, 12;22,08 (eq. 4, regarded by the Muffia as "perhaps the most fundamental parameter in Babylonian astronomy", Aaboe 1955 p.123; see Dicks 1994 fn 29 & its concluding emphasis): [a] Was it caused simply by an ancient's division of the mistaken version into 223? (The remainder is misrendered as 10d1/3 on p.vi of Menzel & Gingerich 1962, where we also find an example of the most elementary of ancient astronomy errors: −412 confused with 412 BC.) Or [b] Did an eclipse-investigator find it by dividing the anomalistic year by the synodic month? — a ratio which, if then divided into 223, would have given $18^{g}10d39'$ or (eq. 14) $18^{g}10d2/3$. Regardless, note that the Greek version (using $10^{\circ}2/3$: eq. 14) is peculiarly hybrid: Kallippic & Aristarchan. It was based upon dividing the Kallippic yearlength (eq. 15) into $223M_{A}$, where M_{A} (eq. 2) is non-Kallippic (*DIO 1.1* ‡7 fn 1). See Heath 1913 pp.314-315, Neugebauer 1975 p.603, & Rawlins 1985S. Aristarchos' saros-based Great Year was 2434 yrs (Rawlins 1985S suggests effectively 4868 yrs), using a very-nearly Kallippic year — 1st reconstructed by the adventurous genius of P.Tannery.

[67] See fn 13.

[68] Causes examined in Rawlins 1985H.

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

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

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

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

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

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

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

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

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

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

and the 221 yr cycle

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

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

$$5458^{\mathrm{u}} = 5923^{\mathrm{w}} \tag{19}$$

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

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

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

[75] See §F4 & §F11.

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

B2 From DR's 1995/3/14 letter to a Hoskin-circle Hist.sci figure (footnotes in orig):

> Why is it that I want refereeing of the ancient astronomy controversy — while, by contrast, the Muffia has fled it for decades? (Why has Hist.sci condoned this?) . . . *JHA*'s Hoskin will not [even] look at *DIO* (see *DIO 4.2* ‡7 §B6) . . . . Perhaps some think that this ploy will help excuse a deliberate policy of nonciting[3] *DIO*. Moesgaard has told[4] DR directly (1994/5/6) that he swore over 10 years ago to have nothing to do with DR, so his noncitation-policy [see bizarre instance remarked parenthetically at *DIO 1.2* fn 56] is deliberate by his own account. Is this honest scholarship? Is it considered ethical in Hist.sci [a] to fake the nonexistence of *existing* discoveries by Unapproved scholars, and [b] to fake the nonexistence of *existing* ironclad[5] refutations of Approved scholars' attacks on heresy?[6] — attacks which are then cited [e.g., *Centaurus 37*:97, p.149 n.1] . . . without informing the reader of *DIO*'s demonstration of these attacks' fatal blunders. . . .
>
> **B3** [In the ancient astronomy controversy, one] side (*DIO*) is citing all parties, frequently praising[7] hateful enemies, publishing papers from both sides (even if attacking *DIO*), promptly & frankly admitting its own errors,[8] and insisting on maintaining the substance (if not always the appearance!) of high scholarly standards. As regards both competence & ethics. (So: Hist.sci hates the free-speech practitioner, and kisses up to the suppressors.)

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

C Thurston's Proof

C1 In the summer of 1994, Hugh Thurston (Prof. Emeritus, Univ Brit. Columbia Math Dep't) found, by geometric (ancient) means, the orbit which fits the *Almajest* 5.3&5 threesome of Hipparchan solar positions: "trio C". Used to Keplerian orbits (where direct solution is impossible), DR had unthinkingly presumed that the solution would be iterative.

C2 Thurston quickly solved the problem — noniteratively. He sent his result to the *JHA*, adding a note which pointed out that it confirmed the (iteratively-derived) DR results published at *DIO 1.1* ‡6 §eqs.17-18.

[3] See *DIO 1.2* §C11 [d] end, *DIO 2.1* ‡3 fn 15, *DIO 4.1* ‡4 fn 1, & *DIO 4.2* p.57 top & ‡7 fn 12.

[4] Most bizarre: Moesgaard's words showed anger at DR ! So did an unexplained crack by OG (also 1994/5/6). Is this the 1st case in history where the muggers affected haughty moral outrage at the muggee's choice to resist?

[5] The papers Evans 1987 & Swerdlow 1992 (both *JHA*) were direct assaults on DR's ancient star catalog paper (*PASP* 1982). Both were riddled with (and destroyed by) several entertainingly elementary blunders, as pointed out at *DIO 1.2* fn 288; *DIO 2.1* ‡4 §§F2-F3, fn 18, & fnn 65-66; *DIO 2.3* ‡8 fn 25, fnn 31-32, & §§C8-C15. All this *DIO* material was sent to the Muffia. No reply.

[6] Heresy may not be published or cited until proper refutation has been concocted [even if this takes many years (e.g., *DIO 4.3* ‡15 §I3), and the eventual "refutation" collapses from freshman-math botchery (e.g., *DIO 2.3* ‡8 fnn 24&31); thus, the citation can (& must)] be immediately followed by counter-citation of an anti-thoughtcrime source. [E.g., van Dalen 1994: see here at ‡1 fn 31.] See also [*DIO 4.3* ‡15 §H1 and] *DIO 1.2* fn 15.

[7] See, e.g., lists of examples at *DIO 1.2* fn 16 & fn 174. I am also preparing to publish an exceedingly generous note if the *JHA* openly admits (what it now knows, to its chagrin) that the 3 Hipparchan Greek-style orbits, which its lead 1991/5 paper decreed unfindable, have been found & published by *DIO 1.1-3 — where all 3 orbits have been related to Hipparchan data*. (This doesn't mean *DIO* will cease criticizing the Muffia & others. But, the more Muffia credits we can admire, the better.) [Note added 1995/12: This message (& *DIO 4.2* ‡7 §B42) towards *JHA*'s incommunicado commandante had no effect upon the 1995/5&8 mini-retraction — one more measure of *JHA* educability & remorse-depth.]

[8] E.g., *DIO 1.1* ‡1 §C3, ‡6 fn 35, *DIO 2.2* fn 98, and more to come in upcoming issues, e.g., [here at §C1 & p.2]. [Note added 1995/12: See also §§C1-C2, ‡4 §C5, & *DIO 4.3* ‡12 fn 2.]

‡3 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 1.2-3*'s "Muffia Orbituary".[1]

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

A3 However, what *JHA* Ed. Michael Hoskin has done so far is depressingly minimal[2] — 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 Muffiosi (notice at *DIO 4.2* ‡7 §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 Orbituary 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* ‡4.) There has been no Hist.sci notice of any of these miscues, 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* ‡1 §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* ‡4 §A; more here at ‡1 fn 1. Enjoy also the Muffia's inadvertent gross slander of its own godpop: *DIO 1.1* ‡5 fn 15.) This censorial 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 Muffiosi 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 Muffiosi insist (§C5, §E, fn 15, §H4) upon noncommunication.

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

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

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

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

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

[79] It should be pointed out that the eclipse-pair (−719/3/8-9 to −278/6/19-20, the latter event half-visible) proposed by Rawlins 1985S is not ideal as a basis for eq. 19: [a] The eclipse-magnitudes are different. [b] The number of anomalistic revolutions is not integral — thus, the actual interval is about 4 hrs smaller than eq. 19 predicted, and the distances from Earth are different (which affects comparative magnitudes). Such effects can be compensated-for (as Ptolemy mercifully notes at *Almajest* 6.9), but otherwise, Ptolemy would justly criticize such an effort (as at *ibid*). (And he might do so, regardless. Ironically, Ptolemy's own much-more-sophisticated *Almajest* 4.6 alleged determination of the anomalistic lunar motion can be faulted for using an interval not anomalistically-integral: this approach makes the result sensitive to numerous needless uncertainties. Unless one is forcing the result. See fn 51 & fn 55.) If Aristarchos used intelligently the −719&−278 pair (as Rawlins 1985S suggests), then he was: [i] doing such primary research that refinements were yet to come, [ii] compensating (requiring an anomalistic theory already: see fn 56 & Rawlins 1991W §N17), or [iii] averaging a whole collection of similar 441 yr-cycle pairs. (See fn 110 for a hypothetical example of such averaging, applied to 781 yr-cycle data.) Such pairs are common. Indeed, the −720/3/19-20 & −279/6/29-30 pair was also available — though the interval was almost a full day less than eq. 19's 5458^{u} = 161178 days. This is the price one pays for non-integral anomalies: here, the lunar eq.ctr-difference minus the solar eq.ctr-diff equals the negative mean lunar elongation diff = 9°3/4, an amount which lunar synodic motion will require most of a day to compensate for. By contrast: for the −719&−278 pair, the lunar eq.centers are nearly equal (within a half-degree), and the mean elongation discrepancy — predominantly due to solar-anomaly-inequality — was barely 2°, or 4 hrs of lunar synodic motion. Thus, the pair is merely c.4 hrs short of eq. 19's 161178 days. (By my calculations, the Babylon mean times were 00:10 & 19:44, resp. These figures are, of course, subject to modest non-independent uncertainties: see fn 48.) Note also that, over a 441 yr interval, the effect of a 4 1/2 hr discrepancy is ordmag 1 part in a million (though, in fact, eq. 19 is alot more accurate than this: §F8), thus, a hypothetical draconitic yearlength based upon the −719 & −278 pair (proposed in Rawlins 1985S) would probably be more accurate than any value known to exist previous to Aristarchos, even without any compensations: simply dividing 5923 into 161177d20h yields the equivalent of about 27d.2122 (which was low by about 2 timesec). At fn 110 (781 yr cycle), we see that ancients (including Ptolemy, 4 centuries later) would indeed use long-interval eclipse-pairs, despite nonintegral anomalistic returns. (Nothing new about this. Indeed, the most famous & widely-used solar-synodic period-commensurability relation of them all, eq. 30, has a huge anomalistic remainder. Using the real [not Metonic] tropical year, we have: 235^{u} = 19^{y}+02h = 19^{g}−06h = 252^{v}− 53° = 255^{w}+ 8° = 6939d17h.) Note that, for both situations, further data may have been available.

[80] Fn 43.

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

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

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

G Identities

G1 Ptolemy alleges (*Almajest* 3.7) that ancient astronomical records were generally rather complete from Nabonassar 1 (−746/2/26) onwards.[84] Thus, the current findings extend (§D6) the period of useful Babylonian records backwards by roughly a century.

G2 But we are left with the question: who discovered eq. 10? — based on the 795 yr eclipse cycle. It could have been Hipparchos. There are two 795 yr pairs of which he might have seen[85] the latter member (both were part of the same ss, ending at −36/12/7), namely, −957/11/20-21 & −162/9/22-23 and −921/12/12-13 & −126/10/14-15. On the other hand: [a] No attested Hipparchos eclipse observation is part of a visible[86] 795 yr pair. [b] The earlier end of any hypothetical Hipparchan pair must be more than 200 yrs previous to the −720 eclipse observation attested by Ptolemy — but resorting to postulating 10th century BC material is unnecessary, since later material (§E6) can explain eq. 10 just as well. (Nonetheless, see the speculation of §§H6-H7.)

G3 So I prefer the least sensational of our options here, one which also ties Hipparchos' 345 yr cycle (double: §D1 & eq. 8) and the 795 yr cycles together — with the −830/2/4 eclipse representing the knot.

G4 Accepting this, we ask: who could have used the −36/12/7 eclipse? (Certainly not Hipparchos, who was long dead by then.) We are now peering into the period between Hipparchos (2nd century BC) & Ptolemy (2nd century AD), a time whose high science has hitherto been a virtual blank[87] in history: now as poorly-attested as it is critical to understanding the flowering of the grandest achievements of ancient science, many of which are reflected in the *Almajest*.

G5 Rawlins 1985K traces the *Almajest* 9.3-4 Venus & Mars tables to the reign of Kleopatra (52-30 BC);[88] so the suggestion (§§E6 & G2-G4), that eq. 10 was discovered in 37 BC, is consistent with the supposition that high astronomy was being maintained & improved at this time by a figure or school(s) whose name can for now only be guessed at. Possibilities include (Neugebauer 1975 p.575): [a] Serapion, who is the earliest figure cited (fn 32) as a compiler of tables for equation of time (which indicates the existence of sph

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

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

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

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

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

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

[7] for further reading. Of all the theories that I have come across about the use of the table, this is the only one that has any degree of plausibility. The statement, made all too often,[5] that the Mayas could predict eclipses is definitely false.

References

[1] J E S Thompson, *A commentary on the Dresden codex*, Washington, 1972.
[2] A Pannekoek, *A history of astronomy*, London, 1961, page 61.
[3] O Neugebauer, *History of ancient mathematical astronomy*, New York, 1975.
[4] *Astronomical Cuneiform Texts* [ACT], ed. O Neugebauer, London, 1955.
[5] M Kudlek, *Solar and lunar eclipses of the ancient near east*, Neukirchen, 1971.
[6] John E Teeple, *Mayan astronomy*, Washington, 1930, page 65.
[7] Harvey and Victoria Bricker 1983, *Current Anthropology 24*:1-18.

[5] [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 11958. 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 11958, 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.)

177 177 177 148 177 177 177 178 177 177 177 177 177 148 178 177 177 177 148 177 177 177 177

177 177 177 148 177 177 178 177 177 177 177 177 177 148 178 177 177 177 148 177 177 177 177

177 177 177 148 177 177 178 177 177 177 177 177 148 177 178 177 177 177 148 177 177 177 177

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 (and was) 173.31 days.[4] 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

[4] [Note by DR: $173^{d}1/3$ is precisely one-third of two rounds, thus it has been reasonably (if controversially) speculated that the Mayas may have chosen their 260-day round-interval out of interest in eclipses.]

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

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

H1 Anonymous is certainly now the name of the 9th century BC Babylonians who took such care to make the precious early −830/2/4 record. Why was it preserved? Striking fact: the grazing −830/2/4 lunar eclipse *ended an extremely long ss*. At magnitude[89] m of merely 0.5 digits (about 1 arcmin of visible umbra), it might hardly have been noticed if not anticipated and looked for at Babylon. Simple (speculative) explanation: the Babylonians had already isolated the saros phenomenon by the 9th century BC. Since such awareness doesn't develop overnight, the hint[90] is there that: ss were being studied in Babylon in the 2nd millennium BC.

H2 Going to the latter end of the particular ss under examination, we find that it *lingered* to an unusual degree. It contained 57 eclipses, spread out over nearly 1010 years[91] — almost the maximum possible duration. (See §F3 & fn 73.) Though the typical ss (e.g., that ending at −1036/3/2-3) dies out with magnitudes fading at about a digit per saros (18+ yrs), those eclipses comprising the peculiarly persistent ss under discussion had a magnitude less than 2 digits for almost *two centuries* before finally dying on −830/2/4. Taking only eclipses visible at Babylon, we find that, as early as −1029/10/8-9, the magnitude was m = 1.9 digits, and it had only fallen to m = 1.7 by −1011/10/18-19. Thereafter, instead of declining, the magnitude stabilized at about 1 1/2 digits and then even *climbed back* a bit: resurrecting from m = 1.6 on −975/11/9-10, up to m = 1.7 on −957/11/20-21 (fn 85) & −921/12/12-13, then down to m = 1.6 on −884/1/3-4. By −866/1/13-14, the magnitude had fallen to m = 1.3, and the next eclipse of this ss visible[92] in Babylon was the last (−830/2/4-5, m = 0.5).

H3 In connection with careful ancient preservation of the −830/2/4 eclipse-record: one might also tenuously speculate that Babylonians of this early epoch additionally were aware (whether from direct speed-estimation[93] or from theory — or perhaps just from the §H2 linger-factor)[94] that the −830/2/4 eclipse was a perigee event. (See also §D6.)

H4 I am grateful to H.Thurston's current paper (in this issue) for bringing to my attention the extreme rarity of the occasional 5 month interval between successive *visible* eclipses. (I.e., visible from one site.) See ‡2 §§B8&B10. (He notes the analysis at Neugebauer 1975 p.130, containing an impressively ornate full-page "proof" [repeated uncritically from *Almajest* 6.6] of the superficially-plausible-but-unfortunately-false Ptolemy-Princetitute proposition[95] [emph added]: "An interval of five synodic months is possible for lunar eclipses, provided that the total length of these five months *is as great as possible* .... the greatest possible solar motion [combined] with the smallest possible lunar motion." Neugebauer 1975 p.130 n.2 cites a 5 month pair as an example, but had our Princetitute immortal merely applied grade school arithmetic to *his own example* [the Oppolzer 1887

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

[90] And see fn 30 on the 1301^{8} cycle's accuracy-compatibility with eq. 2. For counter-hints, see fnn 98, 99, & 103.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

(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.[3] 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 *San 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 ‡1 fn 83.] The Mayas had similar figures. From inscriptions that gave the age of the Moon at various dates, which must be calculated not observed, because some of the dates are mythical, John E Teeple [6] deduced that the Mayas were using 81 months = 2392 days at Palenque and 149 months = 4400 days at Copan.

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

[3] 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.

B3 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, which I will explain later; and a row, D, of numbers.
B4 The numbers in row D are all 177 (sixty altogether) or 148 (nine altogether). You can see (page 31 here) three 177s, a 148, and another 177 along the bottom of Dc page 54. Each 148 in the table is followed by a picture.
B5 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

6666666566666656666666566666656666665

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.)
B6 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.
B7 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 angles.
B8 Without a theory of latitudes, how would anyone ever light on 5-month and 6-month intervals? It is easy enough to say glibly 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 §H4.]
B9 Let us look at an example. It is no use taking eclipses from Oppolzer, Meeus-Mucke, or Liu-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.
B10 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.
B11 The Mayas may 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 fantastic 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.
B12 This reasoning is reinforced if we compute the intervals between every pair of eclipses, not just successive eclipses. The intervals, in months, that occur more than once between the eclipses listed on page 146 of [5] are:

I The 800 Sidereal Year Eclipse Cycle & its Metonic Nest

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

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

I2 When first discovering eq. 20, I naturally wondered if the ancients were aware of the cycle (whether or not they knew it was the shortest). And I quickly found the humbling truth: yes, the 800^y eclipse cycle had been discovered by ancient scholars — 2000 years ago! To be specific: they certainly knew of it by the late 2nd century AD, and most probably already had it by 62 AD. (See also fn 110.) [One-fifth of eq. 20 is cited at Geminos 8.40-41.]
I3 Unlike the 690 yr & 795 yr cycles discussed here previously (eqs. 8 & 10-11, respectively), the 800^y cycle does not comprise an integral number of anomalistic returns; thus, its appearance is diffused on either side of the 800^y-mark. Since the discrepancies (vs. the exact 800^y figure) are at 19 yr intervals, we will give this family of returns the name: "The 800 Year Metonic-Nest". Eclipse-pairs in this snug Nest can occur at the following intervals: 743^y, 762^y, 781^y, 800^y, 819^y, & 838^y. (Examples of the extremes:[102] a 743 yr pair, −878/9/19 & −135/9/24; an 838 yr pair, −967/6/17 & −129/6/22.)
I4 The earliest explicitly dated trio of eclipses whose records have come down to us is the Sargon-era Babylon threesome[103] of −720-719, whose times[104] were preserved for us by Ptolemy. The −719/3/8-9 midnight lunar eclipse observation was Ptolemy's favorite[105] early eclipse (see *Almajest* 4.6-9, 6.9). We're about to learn (& see fn 105) that this eclipse was likely central in ancient astronomers' secular reckonings, well before Ptolemy.
I5 We will now demonstrate that the −719/3/8-9 eclipse was probably (though see fn 110) used by the ancient scientist who discovered the perhaps-since-forgotten fact — highly convenient for gauging sidereal yearlength — that lunar eclipses return to the same star in eight centuries (§I1). This scholar (maybe Heron or, more likely, a contemporary)[106]

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

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

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

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

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

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

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

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

$$9660^{\mathrm{u}} = 781^{y} = 10483^{\mathrm{w}} - 2^\circ = 285265^{\mathrm{d}}13^{\mathrm{h}} \tag{21}$$

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

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

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

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

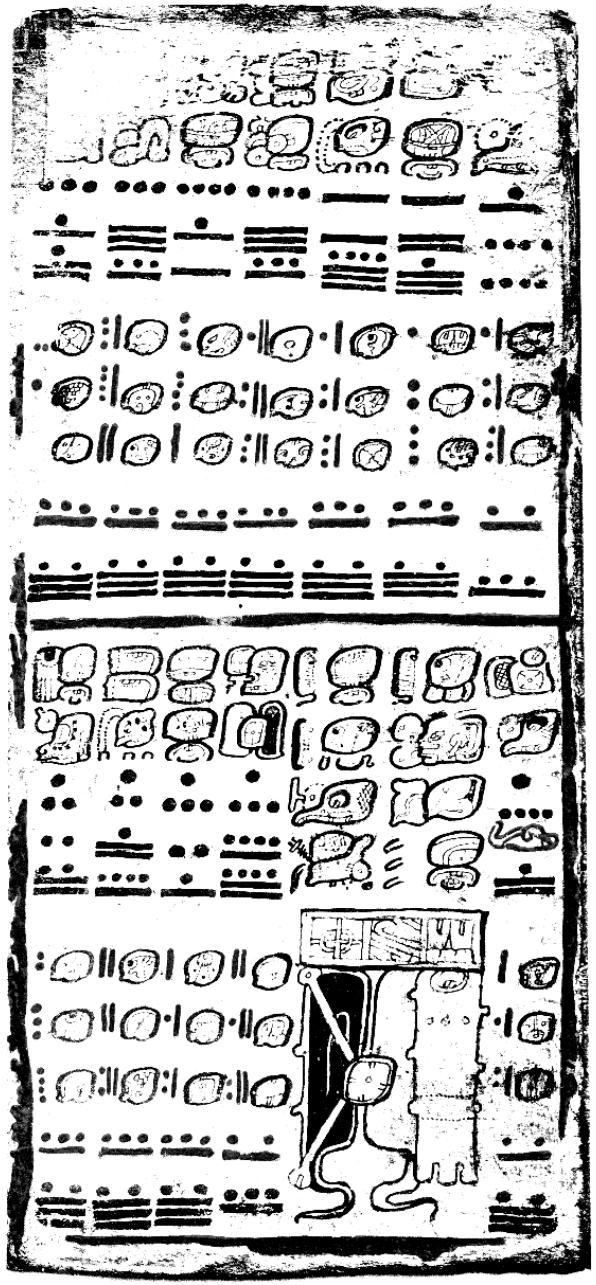
$$Y'_{\mathrm{J}} = 36000Y_{\mathrm{J}}/35999 = 365^{\mathrm{d}}1/4 + 1/147 \tag{23}$$

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

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

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

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



Page 54 of the Dresden codex (Dc)

‡2 A Mayan Table of Eclipses

by Hugh Thurston[1]

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 0 3 5

(written in a column).

[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*.

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

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

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

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

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

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

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

$$781^{y} + 7^{\circ}4/5 = 9660^{u} \quad (27)$$

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

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

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

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

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

[111] This is the very same sidereal→tropical transformation-procedure which was central to Rawlins 1985S and Rawlins 1987 p.237 & n.27.
[112] See *DIO 2.1* ‡3 fn 18. [**NB: eq. 26 appears in the Ptolemy paragraph just preceding that containing eq. 31**.]
[113] If we simply add 1°/century to convert sidereal years to tropical years, we are effectively converting eq. 26 into 36000 sidereal yrs = 36001 tropical years. The error caused by this procedure is about 1 part in 36000 squared — or less than 1 part in a billion.
[114] Eq. 31 approximates eq. 27 to a precision of 1 part in ordmag 10 billion. [Implicit cont'd-fract precision: a billionth.] And the tiny rounding at §I10 [b] (which converted eq. 21 into eq. 27) affected our math by barely 1 part in 30 million (& see fn 113). (Muffiosi who've accepted the false, nontrivial, atypical and-or unknown roundings which are exposed at §§B6&B7 and eq. 33, will have difficulty consistently objecting to our perfectly ordinary & trifling ancient rounding here.) In sum: all the approximations, leading from the original empirical 781 yr cycle equation (eq. 21) to the attested result (eq. 31), corrupt the original ratio by less than 4 parts in 100 million. (These degradations were trivial compared to eq. 31's empirical error, which was roughly 1 part in a million: see fn 110.)

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

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

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

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

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

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

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

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

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

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

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

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

[121] At §I6 item [b].

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

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

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B.van der Waerden 1974. *Science Awakening II* (contrib. Peter Huber), NYC.

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

M Greek, Babylonian, & Princetitution Foundations

M1 Which segues us to a Babylonian-vs-Greek contrast that needs to be made explicit.
M2 There is a cult of modern scholars (Muffia&O.Gingerich) who impute serious sophistication[135] to late Babylonian astronomy, and who have thus for decades intermittently hoped[136] to find connexions between Babylonian tables and empirical sources. However, nearly a century after Kugler 1900 launched this idea (i.e., nearly 5% of the vast timespan since the end of Babylon!), not one Babylonian astronomical parameter or ephemeris (of hundreds) has been successfully related to any specific, dated Babylonian observation.
M3 Indeed, to Muffia catatonic horror, the only Babylonian parameter ever precisely connected to anybody's empirical data is based entirely upon Greek[137] observations!
M4 Two comments: [a] It is obvious that the Muffia's energetic Babylonian advocates have nothing at all to compare with the precise connexions exhibited, e.g., in the present paper, showing Greek use of empirical data. (Which is why funny arithmetic so often enlivens briefs for sacred Muffia viewpoints: e.g., §B5 & §J2.) [b] Babylonian-obsessed Muffiosi's *uniform* unwillingness (even while fitfully conjuring up vaporous[138] speculations of Babylonian empiricism), to acknowledge the simplest evidence of §M3 (fn 137), betrays such truly pathetic intellectual-shock paralysis[139] (how-do-we-get-out-of-*this*-one?), and such a hilariously inverted sense of what has and what has not been established (by modern investigations of ancient science), that it is now obviously long past time that the more fossilized members of this strange cult be relieved of the power to determine[140] who does and who doesn't get funding[141] in the History of science community, a power which: [i] is the primary reason Muffia follies are catered to by young scholars (who are thus forced into accommodation with that debate-shy cult's traditional hide&suck approach to the daunting task of achieving political advancement without intellectual advancement), & [ii] has been so arrogantly misused that it's now just an ongoing embarrassment to modern academe.
M5 Who would ever have predicted that the Princeton Institute — the last intellectual home of Albert Einstein — would become involved in promoting[142] idée-fixe kookery, while blatantly going for suppression[143] of legitimate & highly-recommended scholarship?

[135] See Rawlins 1991W §E3, fn 87, §G3, & fn 266.
[136] E.g., contrast the noble aim expressed at Brack-Bernsen & Schmidt 1994 p.187, with the despair of Neugebauer quoted in this paper's opening text-for-the-day (fn 4).
[137] The −431 & −135 solstices of Meton & Hipparchos, resp. See Rawlins 1991H §A6 (& eq.6) — and (despite attempted Muffia suppression: fn 7) its acceptance by, e.g., van der Waerden (Rawlins 1991H fn 4), Moesgaard (*DIO 2.1* ‡2 §D2), Thurston 1994E pp.123&128, & Dicks 1994 fn 37. See also fnn 15&16 here.
[138] Needless to say, §H here is plenty speculative. But it's hard granite compared to some of the Muffia's gas about, e.g., Hipparchos' nonexistent Babylonian confabs. (See skeptical critiques by: Dicks 1994 §C2 and *DIO 4.2* ‡9 §K9.)
[139] Which explains the seemingly wild §I14 DR prediction that the Muffia will probably refuse even to admit the *possible* worth of the current paper's extraordinary fit at eq. 31. DR's long acquaintance with evidence-immune cultists (in&out of the academic establishment) accounts for this paper's title — as does the Princetitute's continuing effective-endorsement of Muffia arrogance, suppression, cult-fundamentalism, mismath, & false slander (e.g., §B5, fn 5, & *DIO 4.3* ‡15 §I3). Other obviously-valid *DIO* findings Muffiosi still noncite: fn 137, *DIO 3* fn 54 [Toomer vs. Manitius or DR], *DIO 4.2 Competence Held Hostage #2* Table 1, & Rawlins 1991W eqs.23&24.
[140] If such power were turned over to younger scholars, these would include budding Muffies — which would permit testing the optimistic hypothesis that the rigid behavior of some young Mufflets is simply due to fear (*DIO 4.3* ‡15 §G14) of losing funding now controlled by petrified-brain archons.
[141] As is clear from Rawlins 1991W fn 266, I am not trying to starve enemies. (That's the Muffia's tactic: *ibid* fn 16 & *DIO 4.2* ‡7 §B10.) I hope that Muffia research will continue (fn 126) — but without the usual censorial (fn 7) arrogance, and without fiscal threat (*DIO 4.3* ‡15 §G14) against even the *consideration* of dissent.
[142] Stark example at *DIO 4.2*'s *Competence Held Hostage #2* (Table 1 at p.56). And see here at fn 5 & fn 27.
[143] See, e.g., fn 7, §J2, & Rawlins 1991W fn 170.

J Testing History-of-Silence's Archons

J1 But, for reasons given previously,[124] the Princetitute-Muffia-O.Gingerich [PRIMO] Hist.sci cult cannot own that DR has contributed to "their" field.
J2 Therefore, I offer the foregoing reconstruction (eqs. 21&26→eq. 31) to the PRIMO cult, as a simple integrity-test: Is any member (§J4) of this clique, ANY member, willing merely to agree that the relation of eq. 31 to eq. 21 is sufficiently likely (to be a real not chance connection) that it should be cited as merely of *possible* historical value? (Or should Muffiosi continue to recognize the merit only of misarithmetic that confirms its own cult's totally-flawesome prejudices? — e.g., eq. 5, Rawlins 1991W §C7, & *DIO 4.1* ‡4 §A.)
J3 No matter how sensible this request may seem (to the uninitiated), it cannot be acceded to — because Muffia-reaction-to-DR is a rigid proscription-prescription (fn 7). Reason: if a single R.Newton-DR discovery is admitted by Muffiosi to be of potential worth — an epochal event yet breathlessly awaited by *DIO* readers — then it becomes impossibly difficult to justify a continuation of two decades of religious (fn 124) Muffia condemnation of RN-DR. Better to try pretending (despite multi-layer contextual consistency: §I15) that eq. 31's exact match to Ptolemy is just a fluke — and not just *probably* but *certainly* a fluke. (How else justify *total* nonacknowledgement of RN-DR contribution? And, an increasingly dominant factor: how else avoid admitting a 1/4-century of false & not-excessively-competent[125] Muffia slander against important contributions to the very field where Muffiosi pretend to ownership and exclusive expertise?) Note well the cult's inflexible rule (fn 7): the Muffia doesn't say that DR is *almost*-never convincing. It says unqualifiedly and all-encompassingly (as past & future are revealed with equal clarity to the most wise): NEVER convincing. The omniscient for-all-time surety is nothing short of a mental & spiritual miracle (revealing why DR so earnestly promotes[126] solicitous, *packed-in-cotton* preservation of Muffiosi). *DIO*'s readers are invited to join us in awed obeisance:
where outside the Princeton Institute-Muffia can such god-like intelligence be found?
J4 Copies of this *DIO* will be sent to: O.Gingerich, N.Swerdlow, G.Toomer, A.Aaboe, B.Goldstein — the usual unusuals — with *DIO*'s ever-so-humble request here that they take the §J2 test. (I.e., can Hist.sci archons attain to the calmness & impersonal fairness required to recognize merit *even in a scholar they despise & a journal they fear*?) Surely, mailing *DIO* the word "Yes" or the word "No" should not tax the literacy-limits of the ancient astronomy field's Ivy League deities. [Note updated 2013: Muffia-circle reaction to the astonishing eq. 31 match? No replies. No citations. More succinctly: no surprises.]
J5 OK, it's not that I have much confidence that any PRIMO scholar possesses the intelligence (primarily a feel for probability)[127] and the integrity that would be required to pass this test. However, [a] There's no harm in trying. (Yet again.) [b] *DIO* wishes to leave behind as clear a record as possible of proud academic archondum's antibody-rich ability to remain unifiedly immune to evidence, logic, & equity — even for decades on end. [Muffia-vetted, *DIO*-nonciting B.Goldstein 2003 p.70 tests (fn 119) Hipparchos vs eq.31, but won't ask: how (eqs.26-31) *did 105416&8523 occur in the 1st place*? Pass the cotton!]
J6 The operative principle here at §J3 is a solid piece of academic-climber-reasoning: in any controversy, the scholar who does business (and soirées) with the most archons, is the one who's right. No exceptions to this rule can be admitted without implicitly defiling archonal majesty, most dangerously by creating impious infirmity (even skepticism) about whether academe REALLY NEEDS ARCHONS. Remember *On the Waterfront*'s labor-gangster-boss, Johnny Friendly, reacting to the horror of just one person's defiance of his fiscal control of commerce on the docks: "First, [this guy] crosses me in public and gets away with it, then the next joker — pretty soon, I'm just another fella around here."

[124] E.g., fn 120 and *DIO 2.3* ‡6 §F4 & *DIO 4.1* ‡4 §B2.
[125] See ‡3 §B1 and-or *DIO 4.1* ‡4 §B4.
[126] E.g., fn 141. Also: Rawlins 1991W §H3 & *DIO 3* §L8.
[127] Rawlins 1991W §H3 & *DIO 2.1* ‡3 fn 23.

K How Toppe Scholars Discover a Lunisolar Cycle

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

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

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

G&B then round this to the unattested expression

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

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

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

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

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

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

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

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

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

L The Long View

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

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

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

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

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

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

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

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

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

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

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

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

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