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B. L. van der Waerden (world-renowned University of Zurich 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.)

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The Journal for Hysterical Astronomy

Is a University Enhanced by a History of Science Department?

Weak Science and Data-Fudgery Yet Useful Mining of Fresh Ore Diller’s Perfect Fit Shunned 84⁴ History of Science Society Theft BreakfastLunch JHA Refereeing $100,000 Prize EclipsePair Hunt Alltime Clumsiest Ptolemy Fakes Unseen Aristarchos&Archimedes

Table of Contents

1 Isis Stealing Discovery and BadMathing Greek Accuracy: 2015 and 2016 Egglaying
2 The Greatest Faker of Antiquity — Still Foolin’ ‘Em: Funniest Ptolemy AlmaJests
3 History of Astronomy’s Serial Data-Tamperers — Endangering Potential Advances
4 Ptolemists Lay Another Egg: 2014; Suppressing Referee-Urged Contrary Evidence

$100,000 AWARD for Eclipse Pairs Within Orthodox Time & Method Limits. See p.82.

Main changes: DIO’s letter to Isis, www.dioi.org/islg.pdf, has minigrown to §1; §8N added to §2; & §3 is a plainly blunter version of www.dioi.org/qio.doc (sent Isis), but has virtually the same content.

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References


1 Ancient Accuracy Vs History of science Society

To Isis Editorial Board: 2017 March 20 & April 1

[closely based on www.dioi.org/islg.doc’s improvement of www.dioi.org/isle.doc original]

Two recent upfront Isis papers have misunderstood or unattributedly repeated researches of DIO: The International Journal of Scientific History, which I publish.

Your 2015 March issue’s lead paper “The Two Earths of Eratosthenes” by C.Carman & James Evans [University of Puget Sound] Isis 106.1 pp.1-16 [advised by NYU’s A.Jones], www.dioi.org/cev.pdf, is founded totally (abstract-to-conclusion) upon the theory that, though Eratosthenes’ legendary Aswan-Alexandria experiment yields Earth-circumference $C = 250000$ stades for the Sun at infinite distance, it yields $C = 252000$ stades if parallactically adjusted for Eusebius’ finite Sun-distance of c.100 Earth-radii. But this result had already been published in uncited “Eratosthenes’ Too-Big Earth and Too-Tiny Universe”, DIO, 2008, 14 #1 fn 6, www.dioi.org/jet01.pdf, explored as an alternate explanation, even though reasonable traditional theory is that, whatever its origin, $C$ was finally adjusted to 252000 stades so that $1° = 700$ stades, Eratosthenes’ standard scale (Strabo 2.5.7).


Rawlins’ contribution to the 1984 Greenwich Centenary concluded that, ere astrolgers mangled them, accurate maps existed in antiquity with longitudes based on lunar eclipses. Shcheglov calls such maps a “delusion” since eclipse-use is “impractical”, citing in support “badly overestimated” (Shcheglov 2016.n.8) eclipse-based longitudes of Kleomedes, Heron, and Pliny. But Heron is long known to be irrelevant; and Shcheglov miscomputes ([4] below) the other two by treating a Pliny solar eclipse as lunar, and by putting Kleomedes’ Spain in the wrong hemisphere (likewise for Xi’an & Luoyang: see POSTSCRIPT below).

Shcheglov, particularly on his p.693, imputes several failings to DR’s and DIO’s work (the less spectacular are reserved for a footnote).1

Effectively libeling scientists’ standard theory-testing criteria (by which one looks for the simplest theory consistent with the most data), Shcheglov calls us “deluded” for Occamly choosing the popular, simple, datafitting hypothesis: that the 1.4-factor error (40% overestimate) in Ptolemy’s Earth-longitudes was from just multiplicatively stretching them to shift from Eratosthenes-Almajest’s 700 stades/degree to Marinos-Ptolemy’s 500 stades/degree. Shcheglov discards the 1.4-stretch theory by claiming that his true explanation for the 1.4-exaggerated longitudes “proves to be much more complex and intricate” than 700/500 simplicity. But nothing approaching the promised “proof” of the need for complexity ever actually appears in the article, where most complications are gratuitously, artificially injected, by his own myriad diversions from Occamite simplicity, and in his 20pp he never derives his 1.4-grault any other way (than a plain stretch), so he finally urges “further studies.” Whose results can never work as efficiently as plain, raw multiplication.

1 Curious examples of Shcheglov misharges: [a] The simple-stretch idea is alleged (Shcheglov p.693) to bear logical fallacies; none are produced. [b] The stretch-solution is said (idem) to follow R.Newton’s criminal charges versus Ptolemy. Though I agree Ptolemy faked I’ve never said his stretch was anything but a bad mistake (end of §6 below). [c] Ignorance of alternate theories is implied (vs DIO 6 #1 fn 47, DIO 20 #11 fn 2). [d] I’m mis-said (p.693) to claim accurate land-surveying underlay Ptolemy’s longitudes. (My spare proposal was a simple longitude-multiplication, without any connexion to Shcheglov’s amazing & valuably complete reservoir of centuries of stadelen guesses.)
B Shcheglov (p.705) calls early geography “a quixotic illusion” — and his Abstract [captioned Isis by promising] “Ptolemy’s reputation is rehabilitated in part, and the delusion of high-accuracy ancient cartography is dispelled.” The disillusionment is effected by arguing that Greeks couldn’t use eclipses for longitude, skipping all the evidence they did (Rawlins 1984 Greenwich). Shcheglov’s [Muffin fantasy] of bumbling Greeks also defines the broad context of their high physical science as revealed by us [e.g., www.dioi.org/jl09.pdf, for decades]. Our ordmag-estimates of Greek accuracy (check out each for yourself): 1° for big cities’ geogr. latitudes L (Strabo 2.5.7; Isis 73.3 p.264; DIO 16 §3 §[C1-C2]) 1° for scientists’ L (Isis 73.2 p.263; Centaurus 27 p.280; DIO 4.1 §3 §F; JAHF 17 p.326) 0°.1 for star declinations (ditto) 1% for Earth-circumf. precision (ArchiveHistExactSci 26 p.216; DIO 14 §1 §A & eq.28) 1% for Earth’s tilt or “obliquity” (Klio 27 p.266; DIO 16 §3 §[A-B] eq.2 and Tables 1&2) 1% for lunar mean distance (Almajest 5.13-17; DIO 8 §1 §H4; 59 Earth-radii vs really 60) 10° for lunar-eclipse-prediction (DIO 1.1 §6 eq.32) 1° for time of lunar eclipse (Greenwich 1984 in Vistas in Astronomy 28 pp.258&265) 0°.1 for lunar limb vs Sun separation (DIO 16 §1 fn 24) ditto or even 1° for star-vs-eclipsed-Moon gap (DIO 1.3 fn 288; DIO 16 §1 §A fn 22) 1° for solstices (Bull.A.A.S. 17.2 p.583; DIO 20 §2 eqs.21&25&Table 3; P.Fouda 267A) for – 145/3 equnox on Alexandria Palaestra polestar-set ring (Isis 73.2 p.263 n.17) 10° for sidereal year (DIO 6 §1 fn 38&§7I; DIO 9 §1 §Table 2; DIO 1.1 §1 fn 14-15) 1/century for mean motion of Mars and arguably Venus (DIO 11.3 §6 fn 26) 0°.1 for synodic month (DIO 6 §1 eq.2 & fn 12&18; DIO 1.1 §1 eqs.1-8) 1° for anomalistic month (DIO 6 §1 eq.13 & fn 12; DIO 11.1 §1 §A3 & eq.2) 0°.1 for draconitic month (DIO 6 §1 eqs.2&19 & fn 12; DIO 11.1 §3 eqs.1&3) Most historians-of-astronomy are, like Shcheglov, unaware of these symptoms of high Greek acccuracy, speculating without attestation. Copernicus, Ptolemy, Indians, Ptolomaids, & Rawlins (1992V p.326) that scientists kept only theory-accordant data [flatly contradicted by Hipparchos’ record: §3 fn 8], thus unwittingly modeling all ancient science on a blundering astrologer, Ptolemy. QUESTION: how could the above-listed measures have ever progressively evolved into accurate achievement by following a tradition of just keeping on confirming prejudice? [Classic projection from own behavior? See below POSTSCRIPT’s final line.] C That ancient geographers’ longitudes were based on eclipses is doubted by Shcheglov p.690 as “too impractical”. I’ve outdoor-eyeball-timed enough lunar eclipses to know their accuracy is ordmag 1° [anciently somewhat vitiated by sundial graduation limitations, ordmagy] agreeing (at 4°/1') with the well-under-1° accuracy of pre-stretch Geography longitudinal, DIO 16 §3 fn 1885. “Ancient Geodesy: Achievements and Corruption”, Vistas in Astronomy 28:255-268; p.265 (1984 Greenwich paper). Though eclipses are common (Ptolemy experienced 3 in 3 years: 133-136 AD, Almajest 4.6), Shcheglov’s n.8 accepts INDOOR (Rawlins op cit §10) astrologer Ptolemy’s giveaway-incredible claim (Geography 1.4.2) that few eclipse data were available. But outdoor Hipparchos (Strabo 1.1.12 or Shcheglov, n.7) says nothing for rarity or impracticability, instead recommending eclipse-comparison as the best method for scientific longitude-difference determination. Yet, revealingly, the sole eclipse-pair Ptolemy provides (Geography 1.4.2.), to illustrate this central method, is half a millennium old, the –330/90/20 “Arbela eclipse”. Ptolemy’s reported time (longitude) gap is 4/3 too big, so Shcheglov’s n.8 tries aligning Ptolemy and simultaneously attacking ancient eclipse-longitude-measure by asserting that, of four other ancient eclipse-pair reports, three’s longitude differences “also give badly overestimated results”: Kleomedes 4° Spain vs Persia; Heron 2° Rome vs Alexandria; Pliny 3° Campania vs Armenia (4° pair: Pliny’s correct 2° Sicily vs Arbela). D But Heron didn’t even try to gauge longitude-gap by eclipse (Neugebauer, History of Ancient Mathematical Astronomy, 1975, p.848). Kleomedes’ 4° gap is virtually correct since Cadiz at 25°W longitude and Persepolis at 38°32'E are 3°57' apart. Shcheglov just mis-signed Cadiz and found 3°07' (comfortingly consistent with Ptolemy’s false 4/3 factor — publication or even citation (§3 §A1 item [A]) of expert criticism of this Special Literature (especially DIO’s), revealing defenders’ fatal mistruth. No exposure or admission of JHA’s cringing Editor Evans’ 1987 parallax screwup & suspension of his experimental record (§B6 item [1]) will ever appear in the irredeemably dishonest JHA, though undoing this now-conclusive deceit is a required &essential part of any counter to Ptoleism, since it is the Pb-paper-prominent “empirical” centerpiece of the JHAD’s fantastic 1987-to-1998-to-eternity tenet that huge, Ptolemy-sized observational errors were normal in antiquity. Further, no mention is allowed of definitive evidence (fn 3) of JHAD crimes against academic decency (such censorship constituting just one more crime to be henceforth protected by (then onward), manyitere-hiding & now just relented, data-fudging & henceforth just (§3 §G-C) a half-dozen examples), slamming lies (§B1; §3 fn 5), thereby implicitly by revealing what has been for 40° the awful hidden truth, namely, that the entire Believer side of the Ptolemy pseudo-debate has actually long since become no side at all (as with creationists), their output having no coherent case whatever, thus resorting to tactics as cited, plus increasingly farout & embarrassing coulda—musta alibi-scenarios. (Deepseeks drags at §2 fn 11.) D5 Such childishly obvious illusionism, as delineated at §D4 above, utterly confounds the non-specialist part of audience, as well as the increasingly non-investigative (and even-semi-numerate) “science” press, and is the key to the endless pretense of Ptolemist historians—of-science — knowingly careless of concomitant harm to academe’s accurate perception of ancient history — that archons have not—either been proven as Notorious foolish as Raines: if just a few of puppeteer Gingerich’s claque can forever (§A2, & Rawlins 1992V §C24) keep publishing contrived even—if—laughably-transparent-to-scientists defenses, sapping an ever-befuddling lapdog press-corps, then the prime long-term public perception is secure: Not a single Ptolemy-defense archon was ever wrong on his honesty. (Gerald Toomer the admirable rule-proving exception: §C5.) To normal folk, this may seem a puzzling, feeble, even valueless achievement. But not to those who strive (& fiscally survive) on a vanity of judiciousness or infallibility that’s the antithesis of the scientific attitude of inquiry, and of humility to the rule of evidence. D6 Beyond Ptolemists’ lack of science’s attitude is the mundane matter of skills. Virtually every member of their clique, whatever his eminence, has no high scientific expertise relevant to the Ptolemy controversy. Non-specialists — unable to understand the debate’s central method, is the best method for scientific longitude-difference determination. Yet, revealingly, the sole eclipse-pair Ptolemy provides (Geography 1.4.2.), to illustrate this central method, is half a millennium old, the –330/90/20 “Arbela eclipse”. Ptolemy’s reported time (longitude) gap is 4/3 too big, so Shcheglov’s n.8 tries aligning Ptolemy and simultaneously attacking ancient eclipse-longitude-measure by asserting that, of four other ancient eclipse-pair reports, three’s longitude differences “also give badly overestimated results”: Kleomedes 4° Spain vs Persia; Heron 2° Rome vs Alexandria; Pliny 3° Campania vs Armenia (4° pair: Pliny’s correct 2° Sicily vs Arbela).
central point here is (as 1st revealed in Rawlins 1982G) that ancient scientists found L to ordmag 1° accuracy. For that reason, as well as Brandt et al 2014B’s p.331 advertising 1° accuracy, the most precise solutions for x are appropriate. This becomes important (fn 45) for the Clean Dozen, where x = 4’, closely reflecting the error in the observer’s adoption (independently demonstrated in Rawlins 1994L §8) of L = 31°/4 for Alexandria, which is 3’ (close to 4’±2’) larger than the reality: L = 31°12’. To go further, in order to find an integrated-probability 2σ focus in x-ε space, the student might profitably consult www.dioi.org/biv.htm#bnd.

D  Wacthing a Cen imminent Flay Resort (& Slipper) to the Bottom — How Archons Justify Printing&Printing&Printing Just One Side

D1  Observing unbroken consecutive decades of unexceptionally invalid defenses of the indefensible myth of outdoor Ptolemy, one may justifiably draw conclusions.

D2  The truth behind the unprincipled — sometimes (e.g., fn 18) even vicious — stubbornness of those determined to protect Ptolemy from public exposure by any means (e.g., fn 2o) is that they are still protecting their heretics — resorting to any sloppy argument, any curtailment of free discourse necessary to prevent the larger scholarly community as well as the public from learning that the field’s most powerful archons (controlling the funding and thus the career-security, rewards, & awards of those who volunteer to espouse and do battle for sacred myths) made two huge and related blunders (see §2M: “to fit him”), when they long ago prematurely announced Ptolemy an honest observer and misperceived Greek astronomy as non-empirical.

(See, e.g., ScAm 1979, discussed above at fn 22; and more thoroughly at §2M & fn 52.)

D3  Being politicians, Ptolemaic archons are the sort of people whose idea of intellectual engagement tends (for obvious reasons) not towards weighing scientific arguments but to [1] slandering (§2M) their opposites as fools, knaves, and nuts (before discussing evidence — if ever doing so at all), while [2] point innocent onlookers to the bemedalled, reputable people who’ve taken their side: after all (as we ask at above p.87, in the Text: For-The-Day intro to this article), how could such cynosure seem so Enormously Stupid as they must be or act, if skeptics are right?

D4  Well, here’s exactly how: just [a] keep smearing heretics behind their backs (details & photos at §B1 above) while continuing to [b] publish pseudo-defenses of Ptolemy’s honesty — no matter how ridiculous (§3 fn 66). Meanwhile, disallow — as too Disrespectful

48 But, to be fair, let’s admit that it’s not just the Ptolemy-defender side that uses Dirty Tricks in combat. In those apologists’s eyes, DIO uses Dirty Tricks just as cruelly and frequently — that is, whenever we resort to outrageously extreme arguments, like as, ceptic scholarship, ethical dealings, and defying Infallible Archons. (To pols, it just doesn’t get any dirtier — or extraterrestrially unfamiliar.) Don’t forget boldly-untrustworthy DIO’s prime motto (www.dioi.org/mot.htm#gbcs): a man who can’t be bribed can’t be trusted.

49 Schafer 2002 rightly deemed the Ptolemy Controversy the hottest in the entire field of history of astronomy. Which is why the decades-long near-hermetic suppression of one side of the debate is so: impressive. And as ethically repulsive as the tactics employed — by the chiefs of the field — as so unambiguously documented here and in, e.g., §2M & fn 5.

50 Without mentioning the various powerless evidential proofs that Ptolemy stole the star catalog, Schafer 2013 p.47 instead revealingly resorts to sociology to aver that we can’t KNOW so because herd-loyal Ptolemaic historians (like BZ) still exist: “neither side is able to produce decisive evidence to convince the other side.” (But one thing we can be sure of: inserting such archon-comforting & gratuitous irrelevancy is sure to get a paper published at JHA.) One trusts that jollyp Schafer is smiling as he watches the JHAD show. And one recalls Thurston’s quote from Bishop Berkeley, “I observed how unaccountable it was, that men so easy to confute should yet be so difficult to convince.” Another DIO motto (DR), which extends also to many other faiths: “[w]here he cannot we tenet he cannot defend in discussion?” (For these & other DIO gms, see www.dioi.org/mot.htm.)

51 Does the fatal crime, Ya-Disrespected-Me, sound familiar? Seen any mob or slackplo films lately?
DIO-J.HA 22 ¶4  Ptolemy Enormity  2018  D.Rawlins

horizontal) in Fig.7 magnify a tiny difference (just a few arcmin: less than $\sigma$) into a difference of most of a century in crossing-times. But, while Castor’s track crosses zero 8 decades too early (21 BC: p.335) for “verifying” the group E epoch (already established earlier in the paper at p.332 as +57), Pollux’s track actually crosses zero conveniently near the pre-desired date.

C21 A peculiarity related to the question of accurately locating the “Ptolemy” stars’ epoch $E$: Brandt et al 2014B repeatedly ignores (fn 45 above) the request, by the DIO referee report, www.dio.org/jau8q.pdf, to correct its repeated misrendering of Rawlins 1994L’s date (for Anonymous’ Clean Dozen stars) as +131 instead of Rawlins 1994L’s Table 3’s actual published value, +159. This is a 28’ difference, which matters, as we see in comparing $E = 40$ to S&O. (The misprinted epoch, +131, was merely Rawlins 1994L’s date for Hipparchos — not Ptolemy’s Anonymous — additionally shorn of its minus sign!) Importantly, +159 is an epoch which, for the 1st time, renders stark and unambiguous ($\pm$17) the Clean-Dozen-vs-SickSix split (by contrast to any of the many other Anonymous epochs $E$ that were flirted-with in the article or the referee report), and in doing so implicitly jettisons as needless (fn 46) Brandt et al elaborately-derived unorthodox groupings. (See fn 42 above, for the advantages of adopting what is after all Ptolemy’s own split.)

C22 A shock that might give historians-of-science pause before continuing weird promotions of the myth of observer Ptolemy: the declinations issue was solved with full competence over 30’ ago, by R.Newton & D.Rawlins. The 2 papers historical journals (Centaurus & JAHH) have published on the issue since have just messed it up while, discovering nothing new that’s valid. Indeed, as seen from §3 Summary’s conclusion (p.47), R.Newton really jettisoned primary these epochs under these codes: “irrelevant knowledge”. Which anyone could’ve foreseen by noticing that both papers inexplicably got deeply involved in graphical solutions by trial&error, the latter paper soberly treating the former as the prime prior research!

C23 As a final quietus to JAHH’s monumentally stubborn 2014 adventure, we now show how easily a scrupulous journal could have checked on whether its or our solutions for $x$ were correct. All JAHH needed to do was: vary the $x$&c of their solutions to see if their residual-squares-sum $S$ was minimal, i.e., equal to our minimum, $S_0$. (Which is the square of the appropriate $\sigma_x$ in Table 1, multiplied by the number of degrees of freedom.) Or: for any of the four ancient astronomer’s star-residuals, [1] subtract DIO’s tabulated$^{47}$ $x$ for that astronomer, and then [2] just re-run Brandt et al 2014B’s monovariate test for him. BJZ will then encounter a sorta-pleasant surprise: all four astronomers’ values of $S$, the sum of residuals-squared, will be found to have declined (comparisons in fn 35), showing that Brandt et al 2014B’s $S$ generally (except for Aristyllos) didn’t get very near optimal (extremal) solutions $S_m$. (Due to low correlations, the differences are not huge; but they show that true bivariate procedures were not applied by BJZ.) Even so, the suggested better solution being found by crude means (fn 36), results in $S$ usually near but not quite at the lowest $S$ possible. This goal can, however, be accomplished through a true bivariate least-squares (as in Rawlins 1994L), which efficiently finds the point in $x$-$\tau$ space where $S$ is a minimum. If BJZ have any doubts that DIO has found THE actual minimum $S$, they need only conduct the very same test, using our $x$&c values, to find an $S$ lower than their own. Using the slightly improved values (vs the referee report) of Table 1 above, the $S$ cannot be decreased further (more than micro-trivial noise) by varying either $x$ or $\tau$. Our solutions for $x$ are just 0.4 arcmin. The size may be small, but the issue isn’t: the
Ancient Accuracy Vs Hist sci Soc

102 | DIO-J.HA 22 14 | Ptolemist Enormity 2018 | D.Rawlins

Clean Dozen, eliminating ever-problematic.\footnote{44} Arcturus — leaving a consistent set we might as well call the “SickFive” — the resulting (unweighted) residuals are mostly about 1/2 degree, the smallest\footnote{45} being 17'. No overlap at all. A lovely split. So there’s just no need\footnote{46} to get fancy over dividing the “Ptolemy” 18 stars. Unless one is extremely, extremely determined to undermine acceptance of R.Newtonian skepticism about Ptolemy — by any sleight necessary.

C18 For finding epoch $E$, Brandt et al 2014B adheres to depending on each star’s “crossing time” (the year when its residual is zero) \\& “slope” (rate of change of declination/year). Though of some interest and utility as rough checks (on better procedures), these approaches are sub-prime (especially when compared to standard approaches — which are permitted by BZJ since they give results in accord with R.Newton), repeatedly necessitating debatable decisions on deletions and weighting. It’s almost as if it was decided to hunt up results every which way but the best: full bivariate least-squares. E.g., small-slope stars’ low weight (for $E$-determination) is automatically accounted-for by least-squares, so there is no need to delete such stars — additionally: doing so will only obfuscate the solution for $L$ (as already noted at fn 42), though the paper indicates no awareness of this as it deletes 3 or 4 stars (not quite the same ones), from one section to another.

C19 During their $E$-search analyses’ odd-option dependence on crossing-times (instead of obviously-preferable measure by residuals; reminiscent of www.dioi.org/ffl.htm#twsa), Brandt et al 2014B tries including weights by slopes’ absolute magnitudes (p.331 & Fig.6), the kind of Legendrian primitive that Gauss devised least-squares to obviate. (Again: this requires deletion of stars which Gaussian analysis doesn’t.) But when the paper moves into cluster-analysis, even this precaution vanishes.

The conclusions with a long, illustrated section (slightly altering §C17’s L-vs-E regroupings that replaced Ptolemy’s simple split) which tests for clusterings in stars’ crossing-times. This is a patently poor basis for eliciting anything valuable, for the obvious reason that the crossing-times’ reliability are highly disparate ($\S$C19), due to slopes that vary from nearly the full possibility (0').3338/yr) to virtually zero — the latter producing nearly valueless crossing-times, which lead to exclusions and inclusions based on virtually random happenstance. The cluster-analysis deletes (p.335) Castor, Altair, Betelgeux, & Sirius (not consistent with earlier deletions [p.332] of Aldebaran, Betelgeux, & Sirius ($\S$C19]), yet in both cases, stars with slopes weaker than some of these are retained. (See, e.g., fn 42 above.) E.g., why does the clustering section of the paper eject Castor but keep Pollux, whose slope is smaller? — probably because their mutually wan slopes (nearby

\[44\]To understand why Ptolemy faked his era’s Arcturus longitude to equal the exact false value he gave at Almajest 7.3, see §2 fn 37 — a precise vindication of R.Newton’s solution, which DR is ashamed to admit he did not fully accept until 2011, thanks entirely to Jack Brandt’s inquiry.

\[45\]If we re-check the residuals via monovariate test for $L$ at Ptolemy’s alleged observation-year, +137, instead of +159, we find the Clean Dozen more poorly fitting, residuals ranging from –8/12 (Zubenelengebi) to +15/12 (Betelgeux), the departure from zero of the worst is half again larger. This is one of several recommendations for using the full Clean Dozen and epoch +159 — otherwise: a lower median error (3' vs 4'), even despite a higher $e_0$ (6' vs 5'); an unattenuated sample (no deletions); and agreement with the Suda’s date for Ptolemy, Marcus Aurelius, +160, not the Ptolemy-claimed epoch: Antoninus, +137. (More exactly, the two epochs are 160/7/14 and 177/7/20 Alexandria App.Noon.) Note: a monovariate $e$ solution for the same stars leads to +150, a serious difference (see $\S$C13 on similarity). And, since such automatically assumes $x = 0$, we have $L = 31^\circ 12'$ (see $\S$C16 above, & Rawlins 1994L §59), which is 3 nautical mi north of Alexandria’s $L = 31^\circ 12'$, whereas the +159 bivariate solution $x = 4'$ closely reflects the +3' error in the observer’s overlap assumed $L = 31^\circ 14'$, and so is effectively right-on: $L = 31^\circ 11'+2''$ (ibid Table 3). All of these neatnesses render it doubly strange that Brandt et al 2014B persistently refused to recognize DR’s discovery of +159, misprinting it (over warnings) again&again as +131. See §C21 below.

\[46\]See at www.dioi.org/geometry, DIO #4 §1 §2, & www.dioi.org/gep03.pdf, ibid §3 fn 13, the parallel case of non-vendible metrical theories that keep getting proposed to weakly explain the already strongly explained ancient Earth-size of Sostrotas-Eratosthenes.

eclipse observers longitudinally 1° apart is no more or less accurate than for 100° apart. Which is why the unstretched 42° from Carthage to Persepolis is correct to order 1°.\footnote{P It should be noted that sampling here has ignored some civilized areas (e.g., the western Mediterranean) that are not even close to according with 4/3. But this anomaly can perhaps help date the original map through testing when nonfitting regions came under the rule of Alexander’s successors: was the original earlier? But that would not explain why London is in perfect accord with 4/3-stretch. I leave these tantalizers to other investigators.

Q So, do Ptolemy’s longitudes show a scientific origin while his latitudes simultaneously prove the very reverse?! Are we left in hopeless contradiction? No, “Achievement” showed otherwise 1/3 of a century ago, at the 1984 Greenwich Meridian centenary, the Longitude Zero Symposium, held at the National Maritime Museum, Greenwich.

R Contra Isis, the data are consistent with early currency of astronomically-constructed, accurate pre-Geography maps, which professional astrologer Hipparchos semi-randomly ruined through doctoring latitudes by lumping them into discrete klima-cubbyholes where all cities in a cell are force-assigned the same latitude ($\S$K; [Geography 1.4.2]; “Achievement” p.261; “Secrets” $\S$D) for handy astrologer-access to inevitably-too-wide-spread klimata tables: Almajest 2.6. (Three centuries later, professional astrologer Ptolemy ruined longitudes systematically, stretching them by factor 4/3 or 7/5. Summary: §3 [111].

$\S$ Given those Almajest tables’ Mediterranean $1/4$ klima-spacing: we can compute that the forced latitude-shifts would, for flawless cubbyholing, theoretically produce 0°3/4 rms error, ordmag-consistent with the 59' median already found above ($\S$K) for 13 major cities’ Geography latitudes, so providing the 1° (and so far only available) explanation consistent with the size of their degraded state, applying attested ancient klima-clumping practice.

T The history-of-science enterprise is proud of being nonjudgemental, e.g., of superstition. It sometimes implies that Ptolemy’s occultism undermines him. Ironically, this well-intended discipline has long blinded the field to the obvious: just as his Tetrabiblos was his religion’s handbook for horoscopic interpretation, his Almajest and Geography were also world astrologer-handbooks. (The 1° fully competent translations of Almajest and Geography called each a “handbook”. Ptolemy’s exact title of what most now call the Geography was actually Geographical Directory, as DIO routinely calls it.)

U Each handbook was compiled for the then-incooperatively-cosmopolitan Seraphic republic, in whose most famous temple Ptolemy lived and worked: near Alexandria, at Canopus, known for “medical” cures by dream and astrology. (D.Rawlins 1984, “Astronomy vs Astrology: The Ancient Conflict”; Queen’s Quarterly 91.4:969-989, p.973.) Every professional astrologer today uses parallel handbooks, one for natal celestial positions, the other for victims’ geographical locations. The damage ($\S$R) which astrology visited upon astronomy and geography is, however, partly compensated-for by its preservation of non-occultist ancient mathematics, science, and observations that would otherwise be lost.

V World maps interested navigators [Marinos’? [3 fn 105] & an expanding theocratic empire’s plagiarizing priests (e.g., Ptolemy) more than most commercial travelers. The huge factor by which astrogloers outnumbered astronomers, helps explain why our only extant ancient world map was most widely distributed by data-distorting occultists. Modern reconstructions can undo some of the harm visited upon the largest, rarest maps, originating from scientists for royalty; but not all can be repaired, e.g., the loss of all competent exact ancient latitudes except, e.g., north Egypt (Giza, Alexandria) & Phoenicia (Tyre, Sidon).

W Both Isis papers cite D.Rawlins 1982, “The Eratosthenes-Strabo Nile Map. Is It the Earliest Surviving Instance of Spherical Cartography? Did It Supply the 5000 Stades Arc for Eratosthenes’ Experiment?”, Archive for History of Exact Sciences 26.2 pp.211-219. But both fail to mention 3 unnimbly central and intensely relevant discoveries in that paper and/or “Too-Big” which Isis readers need awareness of:

[i] The Nile Map shows that Eratosthenes’ original circumference C was 256000 stades (later nudged to 252000, perhaps for 700 stades/degree-convenience).

[ii] Eusebius’ Sun-distance, 4080000 stades, is thus 100+ (Earth-rad), in the Aristarchos-
Archimedes-Hipparchos-Poseidonios tradition that too-big-for-precision Sun-distance is a power of 10: their 10^4 to 10^5.

By the correct (now generally-accepted, but still sniped-at) 185 meter stade, 256000 stades is 19% high, near 6/5 of real C; Poseidonios’ & Geography’s C, 180000 stades, is 5/6 low. All the three Rawlins papers which Isis’ authors have profitlessly consulted explicitly stress that air’s bending of horizontal light renders high by 6/5 the C gauged by lighthouse-flame-visibility, while the same air-refraction will make C obtained by timing sunsets (at different terrestrial heights) come out 5/6 low — the double-sunset method. See D.Rawlins, “Doubling your sunsets or how anyone can measure the earth’s size with wristwatch and meterstick.” Am.J.Physics, 1979, 47:2:126-128, p.127. Cited to discover Rawlins for years in the 1990s in the well-known textbook, Halliday, Resnick, & Walker, Fundamentals of Physics, as its kickoff example of applied science, illustrated by diagram (plus frontispiece sunset-photo). See also J.Gerver and Rawlins in Scientific American 1979 May. But uniformly silent historians-of-science will not so much as admit the existence of the airbend method, not even when they have probably read it (e.g., J.Dutka at AHES 46 p.64, 1993; F.Ragep, Archimedes 23 p.124, 2010; Isis 2015 & again in 2016). But, as we’ve seen already, Isis & Shcheglov in 2016 exceed their predecessors, by showing that all the above-noted scientists & forums are Deluded in finding precise ancient geodesy credible.

[above paragraph’s conclusion added 2017/5/24&6/22.]

X Shcheglov’s n.15 cites fn 13 of “The Ptolemy GEOGRAPHY’s Secrets”, DIO, 2008, 14:33-38, which describes this solution. And “Secrets” seven times cites “Too-Big”, our dedicated explanation of the refraction theory, right in the SAME volume 14 of DIO. (Also bearing the 2008 DIO parallactic derivation [above] of C = 2520000 stades, which Shcheglov n.14 credits to 2015 runupers Carman&Evans, never citing “Too-Big” at all.) Y Though C = 2560000 stades is unignorably prominent in all Rawlins material Isis cites, none of the authors ever mentions that number or DIO’s 1982 discovery of it, or do any mention that it’s 27 times Eusebius’ Eratosthenian implicit Earth-radius r = 408000 stades: [W ii], above, finally realized 26 years later in 2008’s DIO 14 ¶11 eq.11.

Z None notes DIO’s refraction solution (6/5, 5/6) their own citations prove they know of. No historian-of-science has ever shown grasp of its physics [13 §26]. Its triple-consistency (to 1%) with both 40%-disparate C (Eratosthenes-Almagest vs Poseidonios-Geography) AND the 185 meter stade (above), should be known, so that scholars can make up their own minds if it should at last mercifully end-the-endless, the ancient-Earth-circumference debate. If so, the vast literature Shcheglov has unexceedably compiled centuries 2 centuries of pursuit of a METROLOGICAL-solution chimera, while the ultimately-accepted solution should turn out to be not a complex spliced “chain” (p.705) of stade-juggling ad-hocery, but one natural (zero stade-manipulation) unifying PHYSICAL theory: refraction by air.

Following the December paper, may we propose a 2017 New Year’s Resolution to end the tradition of publishing papers promoting Ptolemy as a scientist by simply omitting all the massive evidence he wasn’t? Persisting in doing so can only degrade our discipline.

POSTSCRIPT [Below items originally “Not for publication” but: why shield shunners?): At least six Experts (p.689) vetted Shcheglov. Besides the foregoing weightier problems, how’s the following mostly-minor but mostly-obvious slips elude 6 putative readers? n.4 line 1: Xi’an & Luoyang are located in China not Montana (wrong hemisphere again). p.693 line 2: Eratosthenes’ Earth-circumference is not 25 miles. n.19: Engel’s central Am.J.Philol. vol.106 1985 article is pp.298-311 (as in our ¶3 fn 110). p.703: Publication date of Pliny’s 77 AD Natural History is confused with his deathdate. p.689: Best Isles’ location matches Cape Verde Islands, not the 800-MILE-DISTANT Canary Islands (one called “Kerne” at Geography 4.6.33) mostly found by Englishmen in centuries since Ptolemy called one Blessed Isle “Kanaria” (Language over coordinates?) Try a modern map vs Geography 4.6.34; or 2008’s DIO 14 ¶3 §F, which Shcheglov read. DIO’s Cape Verde Islands discovery just might be cited somewhere, sometime, in Isis. What does it say of tradition’s grip on the field that this simple fact wasn’t noted before? which by either version of the paper were previously unknown to Ptolemy or anyone else. The p.332 grouping is E (Early 6 stars) and L (Late 9 stars), which does not follow the traditional split, namely: the “SickSix” stellar declinations (which Ptolemy “deduced” his false precession from) versus the “Clean Dozen” real declinations (which his precessional math ignored): “our groupings have no simple connection to Ptolemy’s selected six stars” (Brandt et al 2014B p.334). Why? Well, R.Newton 1977 pp.220-225 rightly argues that Ptolemy typically fabricated the SickSix from 1°/cy precession in order to equally-typically then “prove” said precession from the fabrications: [2 §B2. The new grouping would undercut this view by fracturing the SickSix. Problems with the new split (as noted in DR’s 2011/3/15 letter to Brandt): the traditional split is simple, is Ptolemy’s own. Further, DR’s 1994-adopted +159 epoch (Rawlins 1994AL fn 45: all 12 Clean Dozen stars with no deletions) was understood in 2011 for the 1st time to establish no residuals-overlap (fn 7) in the Clean-Dozen-vs-SickSix split, a finding nowhere cited in Brandt et al 2014B, which keeps mis-rendering DR’s also-uncited +159 date, though it was repeatedly, www.dioi.org/bjr3g.pdf, www.dioi.org/jau8q.pdf, put to BJZ. As is obvious from Brandt et al 2014B’s Figs.7&8 (C Ω), star-residuals’ proximity to each other is time-dependent. E.g., the residuals of Alioth and Aldebaran are 18° apart in +128 (Ptolemy group L date of 2014B’s Abstract, Table 2, and pp.332&334) but are within 2° of each other in +159. Indeed, as DR’s 2014/8/26 ref report noted, if we eliminate none of the Clean Dozen and run a bivariate least-squares on them exactly as they stand, the solution is E = +159±9°, I = +4±2° (L = 31°1°±2°). Alexandria) and the extremest residuals are symmetrically within 10° of zero: Betelgeux +10° & Pollux –10°. For contrast, one may examine the results of applying, to the Sick stars, the very same test just done on the actually higher than that of the non-eliminated stars Altair, Castor, Pollux, & Regulus. So the final published version (p.332) expanded the justification for eliminating the Unhelpfuls to include that their zero error occurred later than 200 AD (a criterion eliminating Alair [p.334 p.335&Fig.8], though it was also attended as a number of groupings as a whole (by a daily grid b) which would not, of course, give them 1°/cy of declination, those small-declination-speed stars — though the date is the very entity one is seeking. (In dropping Betelgeux, DR was guilty of a similar mis-step in 1982 [repeated in a different way by BZJ in 2014]). But this was explained in DR’s 2011 letter to Brandt, to no effect.) The ref report also noted a mistaken claim (p.334) that one of the SickSix stars is among the Unhelpful, though none are (by either version of grouping). Yet no correction was made before publication. Is this odd slip vestigial of an early trial-version of the selection process, during the sample-forming’s shopping-around period (similar to the slip at Duke 2005T p.173, noted at Rawlins 2009S fn6; and don’t miss fn 22) — before settlement upon the final versions of L&E? This further hint of arbitrariness is one of the factors vitiating the paper’s lengthy, impressive-searching-appear for groups of stars different from the skeptics’ simple acceptance of Ptolemy’s own groupings. In the final paper, no correction occurred for the above-cited ref-noted fact that Aldebaran was eliminated from Brandt’s selection L&E, though, again, it was moving faster in declination than non-eliminated stars: the selection of the Unhelpful Thessereone was published unaltered at p.332. (The paper’s last version of groupings [in Fig.10] restored Aldebaran while booting Altair & Castor.) More important than these errors is the general misconception that slow declination motion is ground for dismissal (even while Brandt et al 2014B believes it is looking for latitude “accuracy”) — these are the very stars that least-flexibly measure latitude-error. Real bivariate investigation would know that and would know that getting them right cannot occur without simultaneously doing likewise for x, since all the correlations are non-zero, and some are non-trivial.

43 But labelled O—C. The various Brandt et al 2014B Figures confuse O—C (Observed-minus-Calculated) with C—O (evidently a routine weakness among historians-of-astronomy [though here fortunately harmless]), e.g., [2 §F8, & www.dioi.org/ifl.htm#bnm]. And Brandt et al 2014B’s eq.1 (p.331) is founded upon a confusion of errors with residuals (possibly just a misunderstanding of the Rawlins [1982 eq.2]), thereby equating Observed-minus-Calculated with what is actually just Calculated. If taken seriously, this makes Observed equal to twice Calculated!
Table 1: Ancient Observers’ Epochs, E, Adopted and Actual Geographical Latitudes, L

<table>
<thead>
<tr>
<th>Obsrvr</th>
<th>E ± σE</th>
<th>Adop L</th>
<th>Its Error x</th>
<th>Actual L ± σa</th>
<th>σo</th>
<th>σr</th>
</tr>
</thead>
<tbody>
<tr>
<td>Timocharis</td>
<td>−302±08'</td>
<td>31°12'</td>
<td>+1°5.5±1.9</td>
<td>31°10.5±1.9</td>
<td>±6.1</td>
<td>±5.9'</td>
</tr>
<tr>
<td>Aristyllus</td>
<td>−258±10'</td>
<td>31°15'</td>
<td>+1°0.4±2.7</td>
<td>31°14.0±2.7</td>
<td>±6.1</td>
<td>±4.2'</td>
</tr>
<tr>
<td>Hipp</td>
<td>−131±05'</td>
<td>36°08'</td>
<td>+0°2.0±1.2</td>
<td>36°07.8±1.2</td>
<td>±5.2</td>
<td>±5.0'</td>
</tr>
<tr>
<td>Anon</td>
<td>+159±09'</td>
<td>31°15'</td>
<td>+4°4.4±2.0</td>
<td>31°10.6±2.0</td>
<td>±6.0'</td>
<td>±5.6'</td>
</tr>
</tbody>
</table>

*Historical case at hand: even after the answers are discovered and computed for them, some historical journals just can’t cope.*

**C16** In 1994, 12 later than 1982, DR discerned a new method for finding each observer’s *assumed* geographical latitude: from nulls in his data’s fractional-endings’ frequency-profiles (as explained in Rawlins 1994L §(F) which, by subtraction of x, easily produces each observer’s absolute *actual* latitude L. All four least-squares-fitting E and epochs L (Timocharis 11 stars; Aristyllos, 6; Hipparchos, 19; Anonymous, 12), along with their standard deviations (σE & σx), as well as single-datum standard deviation, raw (σo) and with the effect of rounding removed (σr). All these desiderata are produced here in Table 1, slightly improved (see fn 38) vs the values of §3 Table 2 above or Rawlins 1994L Table 3. Strangely, Brandt et al. 2014A, the refereed version of Brandt et al. 2014B, www.dioi.org/bzj0.pdf, claimed that Rawlins 1994L had latitudes “close to our values” — this, even though BZJ have to this day never solved for any of these latitudes. So www.dioi.org/jau8q.pdf, the DIO 2014/8/26 referee report, at pp.3-4, suggested that this point be clarified and that Rawlins 1994L’s x values and absolute L values for all four observers be printed, since BZJ had brought up the point, and had supplied 41 of DR’s other numbers (the majority correctly) — preferably along with a sentence on the novel though-triumpy method, which DIO had invented while pioneering this entire line of inquiry. But, probably because DIO’s x values especially & hugely disagreed with JAHF’s “accuracy” values, the published article did none of these things.

**C17** Following such odd doings, Brandt et al. 2014B performs somersaults of arbitrariness, 42 and unorthodox implicit weighting, while splitting the “Ptolemy” 18 stars into two groups (after dropping three stars at p.332, then a reshuffled four at Fig.10) — groupings

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38 The sole non-fit for the dozens of data in the nulls experiment was Timocharis’ Aldebaran. Rawlins 1994L fn 39 suggested that the original North Polar Distance may have been 81°15'/15, recorded (conventionally for unit-fractions) as 81°15', but later misrecognized (like §3 fn 44) as 81° & 15 arcmin, thus δ = 8°3/4, as at *Almagest* 7.5. Thus, reconstructed true δ = 8°14/15 or 8°56', which also shows a poor residual. And Arcturus obviously bears a 1° scribal error; restoring the original and eliminating outsized-residual for Zubelenagubi (slightly-rounded δ = −5°), we have the Timocharis entry in Table 1 here. (For Timocharis’ results based on non-reconstructed data, see §3 Table 2.)

39 Timocharis & Hipparchos used a precision of p = 12 intervals/degree; for Anonymous, p = 8; Aristyllos, p = 4. The inverse of p · 1/12 is the rms of the effect of average rounding, in degrees.

40 We thank Jack Brandt for rightly urging use of modern satellite-based star-placements. Versus the Rawlins 1994L results: the maximum effect on epoch E was 1° or, on L, just a fraction of 1°; but the improvements are welcome.

41 E.g., at Brandt et al. 2014B p.331, for all 3 observers, our 1982 ms’ epochs E & σo are relayed, conspicuously omitting our x & σx. E & σE covered less than 1/2 the sky longitudinally, which is not a recommended sort of sample when trying to avoid bias. One of the most revealing pecularities is elimination of three “unhelpful” stars, Betelgeux, Aldebaran, & Sirius on the ground (www.dioi.org/bzj0.pdf, refereed version) that they change slowly in declination. The DIO referee report advised that Aldebaran’s declination-speed was

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**Afterword: The History of science Society Hunkers and Bunkers**

Due to cultish historians-of-astronomy, Greeks’ patient-won accuracy is unknown. (Perverse-ironically: it’s widely believed that semi-literate Mesopotamians were better!) Given Editor H.F.Cohen’s haughty rebuff (p.45), a Letter-to-the-Editor (pp.3-8 here), with cover letter www.dioi.org/isa.pdf, was sent 2017/3/20 to the 30-person Isis Editorial Board; separate emails to ordmag 10 board-members (requesting all 30 be informed of the letter), including Maria Portuondo (history of astronomy), head of Johns Hopkins University’s History of science Department, plus a message left on her answering machine 2017/6/11. No response. (Asked later to review these doings, her JHU colleague R.Kargon [history of physics] plopped too “rusty”: 2017/9/5.) Having heard from neither Editor nor Board, DR wrote the latter 2017/4/1, www.dioi.org/isb.pdf, hoping (emph in original) to encourage communication while correcting [Isis 107.4’s] unfortunate December misinformation, unwary Isis publication of which might have been avoided, had Cohen possessed the humility to recognize he didn’t understand Shcheglov’s 2016 December Isis paper except that it enticingly attacked one who was upsetting Cohen by asking Isis to publish too-accurate criticisms of his fellow pols. For Shcheglov, Cohen should’ve sought refereeing from not just the usual suspects but from DIO (re, after all, a huge attack on DIO&DR) during a period when Cohen was actually exchanging emails with DIO, but preferred secrecy. Now, instead of owning to errors, he’s cover-upping for not just Ptolemy but for Cohen, taking you all into hiding with him.

The Isis board’s non-reply so far risks being interpreted as . . . doing nothing — almost mere plagiarism, and miscounting demised and accurately scientifically refereed [p.45 below] Greenwich-Centenary scholarship.

Less speculatively, we know exactly what Cohen was up to, when he did not tell us he was sending the large paper [www.dioi.org/qjo.doc; now less diplomatically transformed into paper §3 below, here] to a referee until he got a negative report safely in hand [a report again not evaluating any scholarship]. Does he imagine such tactics are not noticed by serious academe? Truthseeking institutions communicate. And will not hide their demonstrated miscalculations. And don’t doubly (2015/3 & 2016/12 n.14), knowingly unenlightened observers to conclude that your society is unprincipled, and you will not hear directly from DIO again.

Out of dozens of potential HsS respondents, Isis’ sole burp was a 2017/4/2 email from former HsS chief Lynn Nyhart (Vilas-Baltitch-Kelch Distinguished Achievement Professor) of the Univ.Wisconsin History of science Department, reading (in its entirety): I received your note and have read the attachments. In my view, the decision of what to publish (or not) in any specific case is the prerogative of the editor. So I am afraid I cannot help you out here. Sincerely yours, Lynn Nyhart

**So: what exactly does Isis’ windowdressing “Editorial Board” DO? Why have one?** Since nothing in Nyhart’s note is responsive to DIO’s scholarship or Isis’ above-documented sneers at elementary academic ethics, the History of science Society evidently hasn’t even cared that critics will notice that its board is complicit in Editor Cohen’s display of how brave its journal is, and just might conclude that the Society is more political than ethical.

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38 Cohen email to DIO 2016/9/27: “Never ever is Isis going to publish a paper which already in its very first sentence . . . contains the phrase ‘smothered by a chauvinist battery of destructive, data-disrespecting — even data-fudging — papers’.” (See [3 p.46 below].) Whether the charge was accurate? The point held no visible interest at all for Isis.

The Greatest Faker of Antiquity: Still Foolin’ ’Em

[On 2014/8/26&12/22, a somewhat restrained&spare version, www.dio.org/pt.pdf, of the following paper was submitted to the Journal of Astronomical History&Heritage. Its referee report, while admitting our obvious expertise, included personal remarks (fn 1) echoing religious Ptolemaic O.Gingerich’s various past slanderous referee reports upon our work, naturally requesting removal of anything embarrassing to his clique, even offering to take another later look at the paper (fn 35: “If . . . DR revises . . . I would be happy to look it over.”) to confirm that the censorship he was ordering had been satisfactorily carried out. So our 2015/9/30 resubmission added extensive notes, responding to such typical intrusion by reviewing — at least for JAHH’s info — the long, revolting history of such stifling of open discourse, but giving JAHH permission to delete these or anything else it thought inappropriate, with our encouragement at the prospect of such helpful assistance. In reaction, JAHH has followed the Journal for the History of Astronomy in permanently severing communication with DIO: suggesting, as later confirmed, www.dio.org/oww3I.pdf, that JAHH’s initial request for cuts was made in vain hopes (encouraged by years of Gingerich-circle slander of DR) that DIO would adamantly refuse revision or edits, thus killing the paper without JAHH being indictable for censorship. DIO instead agreeably refused to fall into that trap; thus, fleeing was JAHH’s only escape-option to effect pre-ordained rejection. The 2015/9/30 version follows, very slightly enhanced.]

ABSTRACT

Over a hundred years, simple independent evidences demonstrate that the history-of-ancient-astronomy subfield rulership’s decades-long insistence on the integrity of its ultimate icon Claudius Ptolemy has never been defensible by reason. Thus it resorted to other means. A sampling of subsequent chauvinist tactics provides an educational case study in how a subfield can be hijacked for the better part of a century by a determinedly-careerist cult, at the expense of the attitude, skills, and tolerance characteristic of science, eventually more resembling a church than a research enterprise.

A INCURABLE DENIAL OF THE UNDENIABLE

Claudius Ptolemy’s Almajest is the central document (§11 below) of our valued heritage from classical antiquity’s mathematical astronomy. Though Princettite’s Neugebauer 1957 (p.191) has called it “one of the greatest masterpieces of scientific analysis ever written” the present paper will prove that in truth Ptolemy was not a scientist, but a mathematician who faked science. In an astrology-saturated era, he worked for the occultist Serapiac state religion at Canopus (near Alexandria) where stood its major temple, which specialized in curing illness through astrology&dreams. Ptolemy also authored astronomy’s bible, the Tetraibiblos. His employers no doubt appreciated his consistent demonstrations that reality was in perfect accord (§M2) with divine celestial theories. However, for centuries, historically savvy astronomers have known that this famous 2nd century AD astrologer-geocentrist-mathematician accomplished said illusion by indoor-faking or plagiarizing all his allegedly-outdoor “observations” of celestial phenomena, to force precise accord with indoor mathematical models (some of which had already existed for centuries) and tables computed therefrom. Ptolemy’s depredations even included stealing and mis-precessing Hipparchos’ immortal 128 BC 1025-star catalog (R.Newton 1977 pp.239-242), a deed which for over a millennium polluted astronomers’ attempts to gauge precession, until Tycho in 1598 detected (Rawlins 1993D fn 141) and threw out Ptolemy’s fakes & was thus able for the 1st time in history to predict star-positions — to ordmag 1st accuracy! — 100s in advance (ibid Table 23: 100 select stars for 1701.03). But a few invincibly innocent & deeply committed (§B2; fn 11) archetypical-historians-of-astronomy keep intemperantly trying to breathe life back into their longstanding tradition — e.g., Neugebauer 1975 p.284 & Pedersen

DIO-J.HA 22 ¶4 Ptolemaic Enormity 2018 D.Rawlins

Maeyama 1984 are clear from Brandt et al 2011, though Brandt et al 2014B p.331’s false presumption is that rs will serve instead of x. But what then of x and its standard deviation σx — neither even mentioned by BZJ? While Maeyama 1984’s estimates of σx are (fn 36) off by serious factors, Brandt et al 2014B’s misconception produces nothing at all! — no σx whatever.

C11 The invalidity of Brandt et al 2014B’s procedure (above, §C9) is easily established by performing it in reverse: assume an E0 (instead of an x) and solve for x via monovariate least-squares — then find rs by summing the residuals towards finding e: but the sum is flat zero! (A hint that gauging accuracy here requires rms, not means.) Thus the error in E would be deemed zero. And any linear function in Brandt et al 2014B eq.1 would produce the same result. This for any assumed E0? E0 = zero, by the same reasoning Brandt et al 2014B used for finding rs, we must conclude that all starting values for E0 (before launching the foregoing monovariate analysis) turn out to be (erroneously.

C12 Moreover, any of those who’ve since 1982 tried vainly to improve upon DR would have remarked (had they done a valid 2-unknown simultaneous least-squares) that the rs for the Greek observers is zero in all four cases. None has. A useful extra check: in the cases where correlations happen to be tiny (e.g., Timocharis & DR’s Hipparchos analysis where n = 19 stars), one can come quite close (since x has a unity coefficient in Brandt et al 2014B’s eq.1) to finding x’s error σx through just dividing σo by √n. C13 In a true bivariate solution, e & x are least-squared simultaneously.37 All the figures given in the 1982 manuscript and in Rawlins 1982G were so accomplished. (By hand, incidentally. The later computerized solutions, 1994, 2011, & present Table 1 here [identical to § Table 2 above, except for Timocharis] barely differed at all.)

C14 Before 1982, no one had ever used these data to find the accuracy of the four Greek astronomers’ observatory-placements. Misled by the crudity of the data of most of Ptolemy’s Geographical Directory (GD) & the rigid infectious mantra of certain history-of-science archons, that ancient Greeks were non-empirical (Rawlins 2008R §A), many had long ago gotten the idea (persisting to the present in the History of science Society’s rulership: §1) that ancient geography was typified by position errors of ordmag 1°. That was why Rawlins 1982G — whose main analysis showed 1° precision in the solar transit work of 3rd century BC Alexandrian astronomers — emphasized this revelation (in a brief footnote on stars: fn 27 above) to a History of science Society audience: DR’s 1982 discovery that bivariate least-squares had determined for the 1° time, from the Almajest 7.3 declinations, just how well Greek astronomers could know their geographical latitudes L — which of course led on to the question of why the GD’s coordinates were so awful (§1; §3; §111; Rawlins 2008S). DR’s papers have called L’s error x, and epoch-error e (epoch E solution minus tester’s assumed E0); if the mutual solution is done truly bivariately, it will find not only e (thus E) and e’s standard deviation σe, but x and its standard deviation σx, as well as the single-datum standard deviation, σo. All these solutions are displayed here in Table 1.

C15 If it seems odd that, previous to 1982, no one had found the accuracy of the four ancient observatories’ location (see Rawlins 1985G §3 conclusion), let’s expose something even more revealing: in 36° since 1982, no one else has computed it correctly, either. The two post-1982 papers both waste precious journal-space extensively on graphs & histograms, all to do the analyses inferiorly, e.g., monovariately finding that E where S is minimal, S0. (And the archons of history-of-ancient astronomy imagine they have the capacity for right to judge, shun, condemn, & censor the scrupulous, competent researches of scientists in such matters? Note the parallel to the Journal of Astronomical History &
The erroneous figures for "accuracy" $rs$ in Brandt et al 2011 and Brandt et al 2014B were an ordmag too small, presumably because they were mistakenly found (as hinted at in Brandt et al 2011) by [a] searching monovariately for the $E$ that minimizes the sum $S$ of the squares of the residuals, [b] subtracting the subsequent mean leftover from each datum, [c] with the adjusted data, re-computing the problem nullivariately for an independently estimated best $E$, [d] computing "accuracy" $rs$ by meaning the minuscule mean of the residuals. (Our reconstructions of data via this procedure are in fn 35.) Perhaps we could dub this the "least-non-squares test". The impossibility of $BZJ$'s numbers is easily seen: when Brandt et al 2014B p.331 puts the "accuracy" of Aristyllos & Hipparchos at $0$, O.004, this transcribes to 141&142 1967 S.002PS, resp — obviously a fantasy (and a $BZJ$ was warned of this on p.4 of www.dio.org/jaqua.pdf, DR's invited referee report), considering that the data's rms is admittedly 0.01 on the same page: Brandt et al 2014B's Table 2. (Equally incredible: idem lists rms values for methods of Maeyama, Rawlins, & $BZJ$ — that agree with each other to a 1000th of a degree!)
date; A.Jones 2010A p.202, noting that Ptolemy was actually an honest outdoor astronomer (even below, at §N3) rating Ptolemy a better observer than Hipparchos!); or at least (Schafer 2013 p.47) that there is still a serious question about whether he observed outdoors: classic the-controversy-continues resort (last example: fn 1), ever dear to those fearing faces-loss in a prominent controversy. Even after a 50¹ avalanche of discoveries consistently (fn 12) contradicting his position, Earth’s most loyal Ptolemist responded to Thurston 2002S p.69¹'s prominent challenge by unqualifiedly verbatim-reaffirming (Gingerich 2002 p.70) his original 40¹ ago verbatim echo (Gingerich 1976) of the once-unopposably-dominant mantra (§M2) that Ptolemy was “the greatest astronomer of antiquity” (Neugebauer 1975 p.931).

B CRIME AND SHUNNEMENT AND HISTORY-OF-science: SMEAR-REVIEW AS JOURNAL NORMALCY

B1 In the history-of-astronomy community, for nearly half a century, the issue of Ptolemy’s honesty, originality, and competence has been a trigger for blackballing (Rawlins 1991W §B, shunning (Gingerich 1990; Schafer 2002 p.40), censorship (Rawlins 1996C p.4, DIO 8 p.2), and intimidation (idend in 1). E.g., the field-central and highly esteemed Journal for the History of Astronomy¹ (JHA) may be just a minuscule short of perfection in its

none of these data, long-separately-recognized and separately treated by Ptolemy. Significance of this for Brandt et al 2014B is obvious, but Editor Wayne Orchiston (WO) has not felt the need to inform⁴⁰ them. So much for the integrity of the paper’s Ptolemist conclusion. And of the Journal of Astronomical History & Heritage. C7 Brandt et al 2014B p.331 claims that its analysis is a bivariate repeat of DR’s 1982 analysis but (as warned in www.dioi.org/jau8q.pdf, the 2014/8/26 DIO referee report on the paper) it is really¹¹ just a try (like Maeyama 1984) at solving a bivariate problem monovariately. Except for Ptolemy (where different samplings⁵⁵ obviated a comparison), the resulting epochs E and their standard deviations are mostly about the same as those of DR 1982, after the standard deviation of Aristyllos’ epoch E was fortunately brought into near-agreement with DR’s recommendation, www.dioi.org/jau8q.pdf, before publication (compare www.dioi.org/bjzj0.pdf, vs www.dioi.org/bjzj.pdf, for Aristyllos). A peculiarity of Brandt et al 2014B is that values for x & its standard deviation σₓ are not given anywhere: not even when BZJ attempts recouping and repeating Rawlins’ analysis (which explicitly supplies and tabulates both x & σₓ). Instead, BZJ present miscalculated (§C9) values for a single entity, “accuracy”, which they confessedly seem to regard as sufficiently equivalent.

B6 JAHH readers have a right to know §C5’s four central considerations, but Editor Wayne Orchiston (WO) has not felt the need to inform⁴⁰ them. So much for the integrity of the paper’s Ptolemist conclusion. And of the Journal of Astronomical History & Heritage. C7 Brandt et al 2014B p.331 claims that its analysis is a bivariate repeat of DR’s 1982 analysis but (as warned in www.dioi.org/jau8q.pdf, the 2014/8/26 DIO referee report on the paper) it is really¹¹ just a try (like Maeyama 1984) at solving a bivariate problem monovariately. Except for Ptolemy (where different samplings⁵⁵ obviated a comparison), the resulting epochs E and their standard deviations are mostly about the same as those of DR 1982, after the standard deviation of Aristyllos’ epoch E was fortunately brought into near-agreement with DR’s recommendation, www.dioi.org/jau8q.pdf, before publication (compare www.dioi.org/bjzj0.pdf, vs www.dioi.org/bjzj.pdf, for Aristyllos). A peculiarity of Brandt et al 2014B is that values for x & its standard deviation σₓ are not given anywhere: not even when BZJ attempts recouping and repeating Rawlins’ analysis (which explicitly supplies and tabulates both x & σₓ). Instead, BZJ present miscalculated (§C9) values for a single entity, “accuracy”, which they confessedly seem to regard as sufficiently equivalent.

The debate’s existence is mentioned, but without the key indicting details. Perhaps the authors & editor feel that their admirably full and neutral bibliography suffices to provide representation for dissent. But there is no excuse for silence in the text (which is all most readers see) on the undeniably significant issues listed above at §C5.

Language like that at Brandt et al 2014B p.331 makes clear the monovariativeness: “With the epoch determined, the accuracy immediately follows.”

31 Slightly true also of Hipparchos, where Rawlins 1982G & Rawlins 1994L included two stellar declinations from non-Almajest sources, a supplement ¹⁴ suggested by H.Vogt.

32 BZJ were helpfully provided sufficient advice to inspire proper caution: [1] were given all the right answers for E, x, and both’s standard deviations, [2] were told that their own values for “accuracy” looked remarkably too small, & [3] were repeatedly warned not to do the problem monovariately.

30 The debate’s existence is mentioned, but without the key indicting details. Perhaps the authors & editor feel that their admirably full and neutral bibliography suffices to provide representation for dissent. But there is no excuse for silence in the text (which is all most readers see) on the undeniably significant issues listed above at §C5.

2 Unless CalTech's Noel Swerdlow, not Jones, wrote this section of the preface — though Editor Jones is responsible for publishing it, while Jones 2005 p.21 knows that Ptolemy’s solar data were faked, and that his 180° ekumene was rigged (ibid p.55; Berggren & Jones 2000 p.76 n.53).
Greek observers’ epochs $E$ were eyeballed not computed.

C4 Brandt et al 2014B attempted modest improvement & useful checks on previous work by bringing in modern satellite-determined data, and providing independent (if shaky) statistical indication of the separation of Timocharis & Aristyllus (the split 1st statistically proposed in Rawlins 1982G). DR was asked to referee the paper: DIO’s report, www.dioi.org/jau84.pdf, is on the DIO website (as are our letters29 in this connexion), and that report (looking for any possible basis to be positive about) recommended publication of the new material.

C5 But the DIO referee report, www.dioi.org/jau84.pdf, also emphasized that the paper should not suppress a few extremely germane items, fully known to JAHJ (through the referee-report, which is a point in a distinct direction other than its inexplicable Ptolemist conclusion. (Though R.Newton is cited as a skeptic on Ptolemy, none of his or DR’s damning evidence applies anywhere in the paper, and the reader will not even learn that DR doubts Ptolemy, much less what his reasons are. Surely an odd way to treat a conscientiously helpful referee.)

A [Brandt et al 2014B never even attempted to explain its theory that, when computing precession in Almagest 7.3, Ptolemy ignores the reliable data of his own time and instead uses an unknown’s data from a century past! — without mentioning it.]

B [All of the four ancients assumed a geographical latitude $L$ when they observed stars’ zenith distances $Z$ by transit instrument, then converted the $Z$ data into declinations $\delta$ via the equation

$$\delta = L \pm Z$$

(minus-sign for southern transit, plus-sign for northern upper transit, where $Z$ complements altitude $h$: $h + Z = 90^\circ$, so $L$’s error carries directly, additively, fully into the $\delta$ data, the systematic error of whose mean is therefore the error of $L$. This would seem to be obvious, but the 1st researcher ever to perform the test upon star data, to show contra-conventionally the admirably small error in ancient star-observers’ $L$-error, was DR, for the History of science Society: Rawlins 1982G. From the $\delta$ data contemporary with Ptolemy, all analysts since (including Brandt et al 2014B) have concluded that there is but tiny error (ordinarly $L$) in the observer’s assumed geographical latitude $L$. So it should not be hidden from the reader (as it is, throughout Brandt et al 2014B) that when Ptolemy reduces transit data (via eq.1), he uses an Alexandria $L = 30^\circ 58'$, which rules him out as the declinations’ observer since this $L$ is in error by $-14'$ (Alexandria being at $L = 31^\circ 12'$.)

C Some of the star-declinations allegedly observed by Ptolemy (c.+160) are so bad that Brandt et al 2014B p.332 invents a hitherto-unknown observer for them at 57 AD.29 But that date for ibid’s Lone Mystery Observer (‡2 fn 37) just-so-happens to be within $1^\circ$ (!) of the shortfall-date that the “Ptolemy” Catalog’s stars would end up at (§B6 item [2]),

else say that DR was (ibid) merely “interested in checking” the latitude-errors? — as if the discovery of these had been around for years. Why, throughout, is Maeyama 1984 usually cited ahead of DR’s earlier 1982 works, when both are mentioned? It seems especially strange to find DR’s unambiguous priority, in computing separate dates for Timocharis & Aristyllus reported thusly at Brandt et al 2014B p.334, www.dioi.org/bjr3.pdf, “Until the early 1980s [their dates] . . . were taken to be the same. Currently, the dates are considered to be different (Maeyama, 1984; Rawlins, 1982a, 1982b, 1994).” This comes even harder to explain when we find that the earlier, refereed (otherwise nearly identical) version of the paper, www.dioi.org/bjr3.pdf, has the verbatim-same wording except for the citations, which are simply chronological back then: “Rawlins 1982c, 1983, 1994; Maeyama, 1984.”


29 Were there a case for a +57 observer, the most tempting identification would be Heron, who recorded an Alexandria-midnight +62/3/13 lunar eclipse. But, except in the minds of the most refined of Ptolemy’s alibi-artists (don’t miss JAHJ Editor James Evans at ‡2 fn 11), the case for non-fabrication vanished long ago: §B6.

refereeing (fn 3) but is alert as can be in not permitting any author to argue in its pages that Ptolemy was a liar, even while allowing the defense clique to contend he was not (Evans 1993 p.145; Rawlins 1999 §§F7-F8). That the controversy has been rife with “unprofessional” acts, such as shunning, has been nationally published (Schaefer 2002 loc cit), followed soon after by a direct appeal (fn 35) to the head of the American Astronomical Society, urging supervision of its shamelessly shunning Historical Astronomy Division (H.A.D.), but the unprofessionalism of 2002 has only worsened since. History-of-ancient-astronomy’s lengthy communal monolithic and craniologic insistence upon defensively maintaining — by character-assassination (fn 5) & the threat of exile (idem and fn 1) for dissenters — an evidence-defying, perception-inverting (SN), logic-flouting (SM), holey-sacred honest- Ptolemaic myth as its ultimate herd-sacred tenet, can only weaken the eld’s cred. Same DR: www.dioi.org/bjr3g, www.dioi.org/owt2, & www.dioi.org/oww3l.pdf, the last promising not to contact WO further if no reply.

JHA Editor M.Hoskin, as quoted at Rawlins 1991W [B1]: he actually had the fantastic gall, in a 1980/7/30 letter to Hoskin, to suggest that the JHA was refereeing “by the swiftily-gauged, as against the substantial.” Considering the solid scholarship the JHA keeps publishing, like its unmatched discovery of the Winter Equinox, among so many other pearls (www.dioi.org/jha.htm#Dkqz), this advice was indisputably a “damned lie.” (So claimed patient, sedate, judicious [Schafer 2002 p.40] Hoskin in his 1983/3/3 letter to hothead-horribilis [Rawlins, announcing his exile from JHA, while suit-threateningly rejecting Rawlins’ gentle mathematical criticism of JHA refereeing for a 1982 October JHA paper — Hoskin’s letter mailed, ironically, just before JHA received the ethical author’s advice was indisputably a “damned lie.” (So claimed patient, sedate, judicious [Schafer 2002 p.40] Hoskin in his 1983/3/3 letter to hothead-horribilis [Rawlins, announcing his exile from JHA, while suit-threateningly rejecting Rawlins’ gentle mathematical criticism of JHA refereeing for a 1982 October JHA paper — Hoskin’s letter mailed, ironically, just before JHA received the ethical author’s agreement that, after all, the paper in question was just as invalid as Rawlins had told Hoskin: see fuller quote, and comments on Gingerich’s various imaginings.) Branding as paranoid someone’s...
will merely (!) require the opening of discourse and minds.

B2. Ptolemy regularly fabricated data from which he perversely claimed he derived his models' elements, even while practicing the very opposite. Loyalist Pedersen 1974 p.23 originally disagreed but summed up the esteemed astronomer J. Delambre's view of Ptolemy as "a scientific cheat, swindling with the very method of science and betraying the empirical character of astronomy, setting forth results computed from theory disguised as empirical data in support of this same theory." *Ibid* p.258 also originally rejected the charge that Ptolemy stole from Hipparchos the thousand star-positions in the Ancient Star Catalog — even while suppressing citation of Delambre's 1817 discovery of proof (§K1) that Ptolemy's argumentation (i.e., the entire catalogs) is a reckless and malicious fabrication.

As a skeptic of empirical astrology, I will merely (!) require the opening of discourse and minds.

C Latest Into the Lists

C1. As to the ancient star-declinations issue: what is history-of-science journals' record? Well, both *Centaurus* (in 1982) and the *Journal of Astronomical History & Heritage* (in 2014) received competing solutions to the *Almajest* 7.3 data. In both cases the journal reliably chose the partially inaccurate solution (featuring amateurish procedure and mis-math), while refusing to publish the expertly computed, completely accurate one — presumably because of its heresy in showing (§C5) Ptolemy faked data. Further, both journals refused to acknowledge the content of subsequent communications demonstrating their folly.

C2. *Brandt* et al 2014B is the most recent attempt to exonerate Ptolemy, arguing that the fact that some among *Almajest* 7.3's star-declinations are about right for his own time is (Brandt et al 2014B p.332) "unlikely to be a coincidence. Hence, [these] observations could have been taken by Ptolemy himself." But said chronological fact is hardly either new or probative, and the reader is deliberately (§C5) not told of other data which are both — and which definitively contradict Ptolemy's observatory, all of which were communicated to the *JAHH* editors and peer publication.

C3. Of *Almajest* 7.3's 54 star-declinations δ reported by 4 ancient observers, BZJ's 2014 project examined 53: Timocharis 11 stars, Aristyllos 6, Hipparchos & Ptolemy 18 each. These data had already been studied by Pannekoek 1955 (1° to appreciate the accuracy), but chose not to impart it to his readers. [See §3 fn 100].

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26Maeyama 1984 is graphical by trial&error and is monovariate ($\delta = \bar{x}$), finding nearly accurate values for $\delta$, though with estimated (nonmathematically guessed) standard deviations $\sigma_\delta$.

27Maeyama 1984 p.308 acknowledges that he had seen DR's paper in 1983. It appears possible that, until noticing this, BZJ were in some doubt as to whether the DR 1982 ms (unmentioned in *Brandt* et al 2011) was really done then (perhaps supposing that Rawlins 1982G was just based on guesswork not statistics), as suggested by their ultimate omission to state in *Brandt* et al 2014B that DR was specifically the establisher of the Timocharis-Aristyllos split (earlier presciently guessed by Neugebauer 1975 p.34), as well as of *the whole case* of finding the 4 observers' latitude-declination errors from the data. Why else say (Brandt et al 2014B p.331) that DR "quoted the Timocharis-Aristyllos dichotomy?" (In fact, Rawlins 1982G split Aristyllos off from Timocharis and gave both astronomers' dates, explicitly on the basis of star declination studies [calculated & tabulated in the unpublished 1982 ms], adding the novel finding that all five precise ancient Greek star catalogers showed that their observers knew their geographical latitude $\delta$ to ordmag 1°. See [§C14 below].) Or why-
to $\Delta \lambda = -29^\circ \sin \lambda \tan \beta$, which gets substantial in the north. In the star catalog no such 1°/2-amplitude waves exist, so neither did an outdoor star-collecting Ptolemy. Of course, ever-opinionated Ptolemites robo-countered this new shocker with their usual standard-weapon: scientific inability so truly embarrassing as to raise the question of whether impenetrable Ptolemitism has become a medical problem. Exhibiting the science-grasp of Ptolemy’s fellow-crane-liar F.Cook, MacArthur-Genius N.Swerdlow attacked $29^\circ \tan \beta$ as indefinable near the celestial North Pole, where $29^\circ \tan \beta = \infty$; this, merely from his own innocence (§2 fn 8) of undergrad math: celestial (DIO 3 §[A2] (d)) or terrestrial (DIO 21 §3 (C1))! Longitude’s conversion to great-circle measure entails multiplication by $\cos \beta$, but gt-circ $\Delta \lambda$ can’t exceed $29^\circ$. Swerdlow’s response: he hides (DIO 5 §§[34]). Moreover, Evans’ attack on the (§B5) absent-error-waves argument confused sine waves with cosine waves ($\pm [H1]$) blowing off a $63^\circ$ degree-phase-difference with: “the phase is not exactly right” (Rawlins 1991W fn 152). All this to obscure that he can’t find a cosine wave in the Catalog’s zodiac stars, with amplitude even 1/3 as large as the $1^\circ$ he sought.

B6 In the 1987 JHA, Evans took his 1st large step towards proving he had the appropriate irreproachable honesty & ideological loyalty to succeed (as he did in 2013) then-Editor M.Hoskin, by taking-up no less than sixty-four handsome JHA pages with two successive Pb papers (Evans 1987), wielding the brand of original math we saw in the previous section, arguing Ptolemy might have been honest. (After the revelations of Johns Hopkins University Press ’The Crime of Claudius Ptolemy [R.Newton 1977: deftly summarized by Thurston 2002S], acceptance of even this weak possibility was about all that Ptolemites could hope to accomplish among informed scholars. Still the case: e.g., Brandt et al 2014B.) Evans’ main arguments (see also §§[2 fn 47]):

[1] In 1981 Evans used a cross-staff to measure the longitudinal distance of a star from the mid-eclipse Moon. “I find on examining my notes from that evening” the longitude’s error was $c. -40^\circ$ (Evans 1987 p.275), proving Ptolemy-skeptics were wrong to deny that ordmag 1° errors were not unusual for ancients. However, Rawlins 1991W fn 288 and Rawlins 2009E later showed that if Evans’ reduction hadn’t bungled his lunar parallax correction’s sign, the error would’ve been merely ordmag 1°. Evans’ response: he’s hidden “my notes from that evening” ever since, hiding also from questions on the incident put by Rawlins and Thurston. (See §§[3] fn 11. Comman Frederick Cook ducked investigated inquiry [Rawlins 2017A] §B13. Again: one can see why Evans is the ideal choice to carry on the JHA tradition of spotless integrity.) When later retelling the same argument (nearly-verbatim: Evans 1998 p.259), Evans conveniently forgets to discuss that 1981 eclipse at all (switching instead to adding a previously unmentioned 1777 eclipse he hadn’t outdoor-measured with) — and continues ineducably contending[25] for ancient errors of ordmag 1°.

[2] Ptolemy claimed (Almajest 7.4) to have outdoor-observed all 1025 stars of the Almajest’s Ancient Star Catalog. R.Newton 1977 proved that Ptolemy had indeed stolen the

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[25] Seeing his own 1981/7/16 record proved DR right on that observation, Evans 1998 nonetheless repeats his Evans 1987 argument that Hipparchos’ ordmag 1° errors in his 2 observations of Spica (−145 & −134) bolster the case for large ancient observational errors. Evans 1998 doesn’t tell his readers that Rawlins 1991W fn 288 had long since shown these observations had also been reduced by Hipparchos with the same parallactic sign-flip & that when corrected for this, both his hugely erroneous placements of Spica were merely bunglings of raw observational data accurate to ordmag 1°. Later, Rawlins 2009E [E] showed Hipparchos had made the very same parallactic reduction-slip when seeking Regulus’ longitude, causing the worst error of all his fundamental stars (−35°). When corrected, the erroneous ordmag 0° error merely ordmag 0°. In 1977 eclipse switch renders it undeniable he’s seen DR’s detection of his flub. Has he found fault with it? No. So he & his equally ethical colleagues combine to fake that detection’s non-existence, by cult-wide non-acknowledgement. Their “reply” is their usual: run away and hide. And all Reputable forums (societies, journals, pop-sci mags, & press), which are seen by the naive public as Watchdogs of academe, bow away for 1/3 of a century, & still counting. Understand the stakes here: Evans’ 1987 JHA & 1998 Oxford University Press outdoor “evidence” & sermon constitutes the most prominent & solid-looking of all arguments for archons’ precious central myth of ancient science’s fumbling non-empiricism.

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DIO-J.HA 22 4 Ptolemitism Enormity 2018 D.Rawlins

29 In fact had stolen it. Pedersen 1974 p.258 added (emphasis added): “Our general impression of [Ptolemy’s] moral and intellectual integrity would be damaged beyond repair if we had to believe that he simply derived his catalogue from a previous work by Hipparchus without the slightest acknowledgement of the fact.” Yet when it later became obvious that the theft had indeed occurred, Pedersen flexibly decided that stealing stars didn’t really prove Ptolemy was dishonest, after all (Pedersen 1993 p.559). Agreeably reverting to the 1974 Pedersen, Evans 1998 p.262 says: “At stake is Ptolemy’s reputation as an astronomer; at issue are his honesty and reliability as an observer.” At stake? Well, not-reecently — for either 1974 Pedersen or JHA Editor Evans. Each could always be counted upon to exclude embarrassment. He hides the tiny bit: his mistake / the issue: mis-statement / untestable thus irresolvable, so never having to admit their original mistake in defending it. And far and wide: to face the shame of having for decades (mostly behind-the-back: fn 5) gang-condemned as fools and cranks who turned out to be more prescient (§3M and fn 12) than their denigrators. Pioneer skeptic Robert Russell Newton is deceased. Nothing can now compensate him for the hateful, largely-whispered bile heaped upon him to prevent his case from getting a fair hearing while he lived: sampling at fn 35, plus MacArthur-Genius and amateur* scientist Noel Swerdlow’s branding this brilliant physicist a Velikovskian “con-man” (see DIO 1.1 §§[D2-D3]; and for who’s really Velikovskian, see below at §[N19, and Worlds in Collision p.330]. Given such a heavy long-term investment in their position, it’s an easy prediction that not even the nine ultra-obvious evidential items set out below will cause Swerdlow or Evans — or anyone else in their shrunken* chauvinist
B3 Notable features of modern Ptolematians are: [a] Consistent preference (fn 18K&; §N15) for the inherently unlikely10 over the inherently likely. [b] Failure to notice that serial-proposal of a disjointed collection of odd-ho theories, each tailored specifically and entirely for dodging the latest11 individual Ptolemy- indicting bullet (see also §D6 and fn 18) defies probability — as well as Occam’s Razor, which seeks the single coherent13 theory without confessing. (As with the late Frederick A. Cook Society, some dementia’s only cure is death.)

10 Ptolemy-doubting scientists like Sam Goldstein, R.Newton, B.L.van der Waerden, have all freely admitted in print their occasional mistakes; Rawlins even did so by self-lampoon, right on the cover of www.dio.org/vols/wh2/pdf, DIO 11.2 (2003) [and p.30, p.33 items 5&6]. We await the century when Swerdlow or Gingerich can display in print the same humility as the “pompous” (fn 1) skeptical side.

11 For examples of downright embarrassingly improbable apologia to defend archons’ pro-Ptolemy commitments, see here at, e.g., §§K&L2, fn 37. (As well as Rawlins 1985G n.12; Rawlins 1991W §E3 & fn 99. Cumulative oscillation-oscillation historical review at Rawlins 1992V §C31.) The ultimate far-fetchedness in service to orthodoxy was devised by Evans and promoted by Swerdlow 1992 p.177, attempting evasion of the fractional-ends argument (R.Newton 1977 pp.245-5). The incredible result (Evans 1987 p.243) is spoofed at Rawlins 1992V fn 46 (emphasis in original): “Let’s see, we start by setting [the armillary astrolobe’s] ring 5 NOT on the chosen fundamental star’s ACTUAL [Catalog] longitude at ring 3 but rather at the nearest whole-degree value LESS than [this longitude], for which Evans prefers a 40’ ending; then, after sighting the stellar quarry with ring 2, we read where ring 2 meets ring 3 AND THEN [“mentally”] ADD BACK, ONTO THIS READING, THE AMOUNT [40’] WE JUST AS NEEDLESSLY SUBTRACTED OFF IN THE FIRST PLACE. . . . Got it?” And don’t miss that this bizarre Evans scheme not only causes the unnecessary trouble highlighted here (and expands the amplitude of the absent error waves [S11] from 1½ to 3½,4/1, but would (Evans, loc cit) further commit Ptolemy to the extra bother of having to fudge over a hundred (Rawlins 1994L Table 1) stars’ resulting 25′ and 55′ endings (into 20′s and 00′s, respectively) in the manner shown at §44. As one encounters no less than 64 pages of such desperate resorts, we recall ([§B2 & H2; Rawlins 1992V §§C31-C32) it is intermittently contended that Ptolemy’s plagiarizing the Catalog doesn’t prove dishonesty. But, if so, then: why did the JHA waste over 100 pages fighting academe’s acceptance of a Catalog-theft that (whenever JHADists momentarily admit it) doesn’t-really-matter? (Iraq war apologists dodge similarly: DIO 18 §E.) For further imaginative excursions, see (Gingerich 1976 in Science) “On Ptolemy as the Greatest Astronomer of Antiquity” and ([§2] Scientific American, 1979, “The Myth of Ptolemy” In the 4 decades since these words were written, certainly not since the 1970s, all said scientia-scholarly Ptolemy-advertisements, neither Science nor Scientific American has printed a word on Ptolemy’s frailties; likewise, Sky and Telescope, which has instead repeatedly defended him: 1976 Feb-to-February 1992. The vanity of Free Press at work.) Extra community-embarrassments: massive double-Pb-paper Evans 1987 (below, in fn 47); top Newton-exiler (below, fn 35) Swerdlow 1989, on whose invincible math-innocence (repeated 1979, 1981, 1989, 2010 while reaching for Ptolemy-exculpations), see above, at fn 85. On the other hand, the most popularly scientific element of S. Schaefer’s (Swerdlow 2001) aforementioned 1992 paper disputing Hipparchos’ assertions, Schaefer had never consulted Hipparchos’ Commentaries, as Graßhoff 1990’s brilliant examination had already shown years earlier. None of these definitive findings has caused Schaefer or JHA to retract anything.

B5 It might seem suspicious to some that Ptolemy’s entire celestial system (Sun, Moon, planets, stars) has accurate mean longitude only for Hipparchos’ time, the mean error growing until it reached ~1° by Ptolemy’s +137 epoch. However, ignoring R.Newton 1977’s devastating new fractional-ending proof (summarized: 12 §I of Ptolemy’s catalog-theft, uncomprehending loyalties kept insisting (e.g., Gingerich 1976 p.477) that this was just an innocent solar error that infected everything else, so Ptolemy’s star catalog was actually observed by him with his armillary astrolabe, unfortunately with that surely-honest error in his zero point in celestial longitude λ. Until Rawlins 1982C noticed a flaw in said vision: the armillary astrolabe (Almajest 5.1 & 7.4) doesn’t spin about the ecliptic plane but instead about the equatorial plane; so an outdoor Ptolemy’s longitudinal astrolabe-ops-mis-set by m = −1°.1 would’ve caused the instrumental & actual eclipses to tilt-separate from each other by m tan ε = 29’ (idem eq.2, where ε = 23°.7, the obliquity then), thus causing an error wave in celestial latitudes Δβ = cos λ & an error-wave in celestial longitudes equal

10 Ever-fertile Evans 1987 p.166 even argued that 6° of rocks or trees might’ve blocked Ptolemy’s southern view. The easily testable flaw in this alibi is explored in mathematical detail at 12 JL.

12 Don’t miss www.dio.org/gp3.htm, longtime (1970-2013) “premier” Journal for the History of Astronomy Founder-Editor Michael Hoskin’s efficiency: refereeing&verify between breakfast&lunch! Must be read to be believed. Lucky nobody will ever find it quoted in our vaunted watchdog “Science Press” whose ever-advancing investigative impetus has carried its transformation into lapdog ever nearer the ultimate intimacy it aspires to: lapdancing a needy establishment.
hour. Similar giveaway factors for his three other solar “observations”, all of which agree just as closely with indoor calculation. (Interim question: given this stark un.questioned circumstance about Ptolemy, think carefully about what kind of scholar would dedicate himself to defending him, even to the extent of calling all skeptical scientists insane? The answer has been, for nearly 1/2 a century: virtually anyone who said anything. And this field expects to be taken seriously by scientific scholars? Seriously?)

B3 History-of-science’s notion of a MacArthur-Genius, mathematically-challenged Noel Swerdlow, rejected the all-too-obvious explanation for Ptolemy’s rigged 140 AD solstice with two imaginative excuses:

1. He misconceived at a junior-highschool level. The 2nd was a clumsy fantasy.19

1. Near a solstice, N. Solis, it is impossible to measure accurately the time of maximum height of the noon Sun, since from day-to-day it’s virtually not changing20 then. So referring by Phi Beta Kappa (fn 20) and by Reverend Gingerich, as usual (one might almost say: as-always, given the reliable brand of sheeple who man or oldboy Hist.sci.’s most prominent forums)21 has approved an argument implying that if we toss a ball upward and catch it 4 seconds later, a 9th-grader (or younger) can’t tell that it peaked at 2 seconds?

Ptolemy observed, playacting which is intended to prevent the public from catching on to the field’s long-running thespian obseness in failing to admit publicly the ultra-obvious. The present paper started by stepping on an Ozzie egg — which reminded us of that old-time Easter tradition of planting hidden Easter-eggs all over a garden before unleashing the kids. Well, this kid is ever entertained by an Easter-hunt’s worth of prior Ptolemist article-eggs all about us (§B), every one a squishable plant, indeed, regularly planted during a half-century of establishment insistence on transmuting a clumsy data-faker into not only an honest scientist but a genius,14 no less — a proposition as believable as a rabbit-laid egg. So we will next turn to examining the decades-long history of this desiccated field’s transparent pretend-conviction (fn 50) that promotion of planted apologia for Ptolemy’s observship is any more credible than adding planted eggs to prove the Easter Bunny is real.

B Those Skeptics Are the Crazy Ones! Shun ’Em Outta Town! Refereeing at the Journal for the History of Astronomy

B1 Does it tell us something about the state of soft academe that the Journal for the History of Astronomy, the “premier”15 journal of its field, has for decades used pseudo-science and deception in the shall-we-just-say-extreeemeely peculiar cause of protecting the reputation of a fellow pseudo-scientist (and grant-cow), the ancient Greek astrologer Claudius Ptolemy? — known for centuries to informed scientists as the most notorious liar in astronomical history. JHA’s longtime #2 official, Harvard’s Astronomy 101 teacher & deft Disraeli-esque trowel-wielder Owen Gingerich, has repeatedly billed B2 this ancient faker and superstition-muddled16 as “The Greatest Astronomer of Antiquity”, claiming that all who question this eminently-sensible-to-him proposition are the insane parties to the simmering-if-generally-suppressed dispute inevitably triggered by such superlative saleshype. We will first briefly examine a sample of the succession of careerist soldiers who disgrace themselves by eager enlistment in the JHA’s decades-long War-for-Ptolemy, who knew that publishing their genuflections boosted prospects for favor with the field’s dissent-burying archbishops, thereby improving odds for future conference-invites, publication, posts, grants — all with confidence that no matter how outrageously unlikely their arguments, they had nothing to fear from contradiction, in any of their captive journals.

B2 Ptolemy claimed to have outdoor-observed a 140 AD solstice, though his report (Almajest 3.1) typically disagreed with the real Sun by 1½° or a degree and a half, which is over FIVE TIMES the angular distance from the Sun’s center to its limb (edge) — this, while his report agreed with Hipparchos’ indoor tables to within ⅓ or a fraction of an14 AAS-darling Gingerich 1980 p.264. Quoted at www.dio.org/jf33f.pdf, DIO 4.3 §15 fn 43.
16 Gingerich 1976, Gingerich 2002. Disraeli (L.Strachey Queen Victoria 8.3, 1921 [Harbrace pbk p.244]): “when you come to royalty you should lay it on with a trowel.” Gingerich summed-up succinctly: DIO 2.3 § 16 F. To enjoy Rob Peary’s like suplications, see www.dio.org/cot.htm#dtw.
17 Ptolemy authored astronomy’s bible, the Tetrabiblos, the tripe in which has got to be (but usually isn’t) read if one wants a measure of how seriously to take Ptolemy as a scientist. See Rawlins 1977 pp.70-71 879 for analysis of the book’s escape-hatchery, plus a fundamental Ptolemy gender-miscalculation (ibid p.69) which has escaped detection & thus survived for millennia, persisting even in today’s horoscopes, e.g., those on sale in our grocery-store-checkout-counter literature-departments.
18 Psychologist Gingerich already intended-to-be-anonymous 2000 referee-report to Isis (outed in Rawlins 2003X) called Ptolemy-skeptics just a tiny bunch of paranoids — thereby inadvertently and delusionally smearing most of the scholars in the field, even WHILE he is echoed in the 2015 JAHJ referee-report’s complaint that DR doesn’t respect which should have yielded with. Can it get any weirder? Well, actually, yes. As we see from www.dio.org/pn2.htm, a secret Gingerich referee-report slander of DR’s character (so relevant to the subject paper’s mathematical development!) on a basis which Gingerich had forgotten he’d already revealed, at www.dio.org/pml.htm, applied to himself.
On the reality of cohesive shunning of Ptolemy-skepticism: see §3 fn 6.)
tantrum of the other non-US journal in this strange field: the *Journal for the History of Astronomy*, whose Editor Michael Hoskin, upon receipt of DR’s constructive criticism of *JHA* refereeing of one of its papers, responded with threat and correspondence-cutoff. DR’s criticism was supplemented by a polite, *admittedly valid* DR referee report: see the paper’s re-computation at *JHA* 1984 June, which happened only because the *scientist* author preferred honest accurate results, and cast seed upon stone by (1983/4/27) recommending DR’s refereeing skills to *JHA*. Likewise, when Curtis Wilson and Hugh Thurston insisted (e.g., Thurston 1995) on correcting a flock of *JHA*-unreferred errors in Jones 1991H. In these and dozens of other instances (www.dioi.org/jha.htm/#hsbk) of serious DR-appraised *JHA*-errors, that journal has never taken the initiative in correcting the situation, to produce accurate information — as if it really didn’t care at all about such trifling considerations. A strict rule at *JHA* (at least vis-à-vis DR): if the erring author is no more honest than the *JHA*, then — no correction is made. (Prototype for *JAHH* 2015 and [1] Isis 2017.)

Embodying a third of a century of the field’s proud progress: *JAHH* in 2015 exiles a referee who expended extensive time and labor to respond scientifically to *JHA*’s S.O.S. for assistance in cleaning-up a paper which was beyond that journal’s technical capabilities to observe, as thoroughly demonstrated below. Also: some among the authors may have found themselves rushed or only partially consulted, and out-of-available space for their paper, as a deadline approached — and out of time, having waited too long to call in expert advice. (But this doesn’t excuse the central omission: Ptolemy’s — 14° error in latitude L, emphasized in www.dioi.org/jar3g.pdf, DR’s 2011 letter to Brandt, for its fatal contrast with the trivial L-error shown by the data.)

[b] Even while banning the party providing well-intended potential protection against the likely-upcoming charge that the *Journal of Astronomical History & Heritage* is no more able than the *Journal for the History of Astronomy*, to test submissions for competence and accuracy, the *JAHH* hides the name of and adopts as last-word arbiter a referee who produced no scientific analysis whatever of his quarry. *Isis* did likewise in 2017: ¶1 Afterword. (Again, nothing new: Rawlins 1994S §H3 & fn 44.)

Question: what is it about the majority of the field’s journals, that they treat intrusions of honest disclosure & competent science like leprosy?

A 4 During the silent months that followed submission to *JAHH* of the paper that ultimately became ¶2 above, ever-cheerily-optimistic DR had entertained the possibility that WO was seeking a 2nd, more neutral referee. Finally, DR emailed a friendly 2016/2/29 letter, www.dioi.org/oww2t.pdf, to WO and learned of an arbitrary act which at last nackedly unveiled a proud new addition to *JHAD* covering-archondum (§A3). This confirmed that the usual heresy-containment info-control (standard for the last half-century: ¶2 fn 35) was being governed by the priorities of, in this instance, two colluding politicians. A final 2016/3/21 DIO letter (successfully sent to WO by alternate email address), www.dioi.org/oww3l.pdf, again tried collegially to allay hypothetical shyness (about editing a DR paper) by pointing out that DR could hardly complain of any WO deletions if he’d asked for them! As DR had, in his 2015/9/30 letter. The new letter ended with an easy test (In 13 of the theory that the paper had been dead from the outset. No reply. Which is perfectly consistent with the theory.

A 5 On 2016/3/25, DR happened upon the final published version of Brandt et al. 2014B and was disappointed that various key required corrections, which DR’s ref report had taken the trouble to point out, had never been made. The paper appeared in 2014, long before WO revealed his all-along intent to suppress DR’s paper for heresy, not style. I.e., he can’t in 2014 blame the *JHAD’s* gross failures of editing (& math!) on DR’s not-yet-written 2015 paper (expanded to ¶2 here). www.dioi.org/pm.pdf, which ultimately granted total freedom to WO. (See www.dioi.org/iss.pdf for *Isis’* like achievement in time-disjunction.)

A 6 We will shortly move on to putting *JAHH’s* 2014&2015 sellouts into the context of the disgrace of worshippers’ ever-more bullet-hole-ventilated half-century pretense that actual 365° 2425 year then) to Hipparchos’ — 1469/27 00° Autumn Equinox (Alm 3.1), and one finds 132/9/25 13°46°. Ptolemy reports (Alm 3.7) 132/9/25 14°. All 4 of his solar data agree with such arithmetic, to the 3° precision he displays for each of the 4 — the precision itself a revealing farce, since in all cases based upon Hipparchan cardinal-point times 6-fold rounder: each expressed to the nearest 1/4 day. (For historically valuable detection of yet another Ptolemy solar longitude fake see Thurston 2002S pp.65-66 & fn 14.)

D 6 We have now encountered the following telltale Ptolemy solar curiosities: [1] mean positional error exceeding a degree ([D1] for epoch 137 AD: [2] and null only ([D3] for Hipparchos’ epoch: [3] Ptolemy “observations” tightly ([D4] and overprecisely ([D5] theoretical not empirical. No matter how obvious the implications of items [1]-[3], each has received far less than half-hearted efforts at explaining them away. However, as in [B3] [b], we find no substantial connexion between chauvinists’ several desperate and disparate [17] alibis (other than the common aim of rescuing Ptolemy), while by contrast all three oddities are mutually-corroborative of each other through the single simple theory that simultaneously, coherently, and fruitfully explains them: Ptolemy faked.

E. PTOLEMY’S GEOGRAPHICAL LATITUDES: MORE CONTRADICTORY AND DOUBLY FALSE DATA

E 1 At *Alm* 5.12 and 13, The Greatest Astronomer of Antiquity provides and computes celestial positions using his assumed geographical latitude L for Alexandria: 30°58′ — an erroneous value swiped from Vitruvius 9.7.1 (probably based on observation by asymmetric gnomon, not transit circle). For c.8000 sites, Ptolemy’s *Geographical Directory* (GD) lists, in Books 2-7, geographical latitudes L and geographical longitudes E east of the Blessed Isles (discovered at Rawlins 2008S §F, to be obiously the Cape Verde Islands), uniformly rounded to the nearest twelfth of a degree. At GD 4.5.9 he gives 31°05′ for his religious home, the Serapic temple at Canopus. The Alexandria and Canopus values are each too low by 14°. *No regular celestial observer — Ptolemy’s pretense (at, e.g., *Alm* 7.4) can be this far off and not know it.*

Go to *New York Times* Science’s 2009/9/8 exam of a century of establishment promotion of another scientifically unverified mystery, a study in cenital immunity to oncoming evidence, analysed in the context of other *DIO*-shunning bad-loss cults. Previous day’s online edition: http://tienyelab.blogs.nytimes.com/2009/09/07/who-was-first-at-the-north-pole. (Ptolemy’s all-time record-success at hoax-longevity is noted only in the 9/8 version.) The honest exception to the pattern described is Gerald Toomer who, though previously much-committed to belief that Ptolemy didn’t take the Catalog from Hipparchos, immediately changed his mind on seeing Griffith’s analyses.

[17] From chats with Ptolemites over many years, the following alibis are recalled: [1] Ptolemy’s — 65° solar error has been speculated as due to his having constructed his system before most or all of his “observations” (pretty dumb, since the system depended on the Sun: §D3), so he was stuck with the error and decided [see Ragep at §3 fn 9] not to re­do his whole scheme. (How is this a defense against a charge of knowing pretense? And plagiarism, since [§D3] the system’s error is just that of Hipparchos’ solar tables, 2 2/3 centuries later.) [2] The adherence of “observations” to theory (Sun, Venus, etc) is explained as due not to fraud but to Mere fudging or “adjusting” of real presumed data, wherever whatever positions the “observations” were being fudged to agree with were *SECRET* indoor calculations, so either way it’s fraud. [3] The embarrassment that Ptolemy’s tabular mean Sun was correct only for Hipparchos’ time has been explained by presuming that Ptolemy thought Hipparchos’ observations were better than his own putative outdoor observations, so [§D1] he instead reported Hipparchos-accordant data. (Was it honest to commit this appropriation? — and without saying so, which makes it a theft.) [4] See also rocks and asymptometrically unclear air at [L1] and fn 42, respectively. Would that the energy and ingenuity expended upon these 4 joke-fantasies had instead been applied to open-minded, undirected, exploratory research.

Rawlins noticed this unsubtle point (that no regular celestial observer could be 1°4 off in his adopted geographical latitude L and not realize so) immediately upon entering the controversy. Thurston 1994P noticed something just as glaring back in the 1940s. How could Ptolemy-specialists,
Moreover, such an error (see math of science or history — or anything else — and thus (as reported at Almajest star data from Ptolemy’s 1025 celestial latitudes of the 7.5–8.1 star catalog). Thus, the so-called error cited, 11 excluding no stars when bivariate-least-squaresing the Clean Dozen produces the 103 result? Just run away.

Therefore, one can only admire Dennis Duke’s witty new translation of Table 3) — consistent with Alexandria’s 159±8’, in fine accord (ibid fn 45) with the 10th century Suda’s dating of Ptolemy to epoch Marcus Aurelius 1 (160/171). Previously unnoticed vastness of Almajest’s leapfrog anachronism: the Clean Dozen δ were observed c. 160 AD (as just shown) and then merged with the SickSix 12 which were precess-faked so inadequately for stated epoch 137 AD that their coherent 5 stars’ mean is (fn 37) instead correct for over 100 before 160 AD! There are two unknowns when analysing ancient Star-dellection-statements: the observer’s epoch E; and the error x in his assumed latitude. For the four observers whose star-dellections are discussed in Almajest’s 103 curious paper, Brandt et al 2014B discussed also in fn 37), gets mostly non-outlier results for epochs E yet for all 4 cases messes up the other unknown, x, the error in the observer’s assumed latitude. For the 4 observers, the figures given (op cit p.331) for the latitudes’ “accuracy” (which the authors compute instead of x) are: Timocharis 0’72, Aristylos 0’18, Hipparchos 0’24, Ptolemy (?) 0’3 — values which are oversmall by an ordmag. Likely-wasted-at-present wake-up to the history-of-science community: outside of DIO (3 Table 2, or Rawlins 1994L [Fig & Table 3] no paper on the Almajest’s 103 dellections has ever correctly understood how to find both E and x [ & associated standard deviations]. The 2014 paper also errs in dropping near-solstitial stars for being weak indicators of epoch — forgetting that they are superior indicators of x, and thus matter in gauging the accuracy of the 4 men’s adopted latitudes for their observatories.

Along with §§C, D, and F, the star-dellections analysis proves that Ptolemy’s overservish (or his authorship of the models he reports) is not established by the mere fact of some of his purported observations dating back to his time. Yes, Ptolemy clumsily double-lists L values for sites other than Alexandria (e.g., Helipolis-vs-On and Syene-vs-Elephantine: details at Rawlins 1985G p.260 and n.6). But none were the world’s cultural center he is supposed (by his defenders) to have been familiar with and from where he reports 1st-hand astronomical observations (Almajest 5.12 and 13), an occupation which if real would have quickly and accurately provided Alexandria’s L — and had already done so (Rawlins 1994L [Fig & Table 3] for genuine outdoor astronomers Timocharis, Aristylos, and (above, §§E2-E3) Anonymous.

In light of such sloppy-copy, one can only admire Dennis Duke’s witty new translation of Almajest’s Greek Text, Syntax (3 fn in 15), as: Cut & Paste. Not in Liddell-Scott-Jones. Yet to a fellow politician, who despite “careful” reading could come up with no errors of science or history — or anything else — and thus (in stark contrast to DR’s ref report), offered no scientific guidance at all (unless one delusionally regards shrunkenanalysis as science), instead — even while acknowledging that DR is “clearly quite knowledgeable in the astronomical history involved” — insisting on removal of anything embarrassing to his clique, adding gratuitous psychological evaluations including accusing DR of an “apparent need to disparage those with different views”. This from a cult which has for a half-century repeatedly (and reliably—always behind-the-back) smeared, as insane, anyone differing from its own reality-detached view of Ptolemy — a genuine, mentally-disabling insanity which The Leader is now at the last almost alone—in-the-bunker with (publicly) loyal J.Evans, J.Brandt, & possibly B.Scheafer. (Ptolemy’s name well-known doubts — their consensus not at all well-known — are extensively listed here at ¶2 fn 1, though JAHJ’s guardian [“referee”] is still stuck dreaming-on of a 1/2 century ago, in calling skepticism an extreme position: “worth hearing” he pseudo-tolerantly offers, even while continuing its suppression for a 4th straight decade at his JHA). The ref added a death sentence to the paper, telling an editor who obviously wishes to stay on the good side of History-of-science’s Archbishop of TruthBury: “If this were my journal, I would not like to see this paper in it.” When JAHJ supineely granted him full veto power over the paper, it had been determined that it was not going to appear in any form in JAHJ. But censoring editors (& refs) are ever pretending not to be, so JAHJ’s initial tentative approach to exploring for an excuse for nonpublication was to find out if the durable myth, that DR would not accept1 editorial revisions, would suffice to dodge publishing archon-loathed heresy. A3 DR’s reply, www.dioi.org/owu8qg.pdf, tried [A] to test whether demanding the paper’s softening was in hopes of making DR go away; and [B] to check out JAHJ’s biased chumminess with its mentor (who has loathed and libelled DR for decades). So [A] DR unexpectedly refused to enter into any argument over content, granting full veto power to JAHJ, instead of its 1st referee (as if there were a difference, as we learned). [B] The paper was expanded to provide information about the referee’s claque — vainly asking, www.dioi.org/owu2w, JAHJ to point out DR errors — which would have caused a neutral journal to choose a different referee. The JAHJ’s brave reaction to this disappointment? Just run away. JAHJ went silent, even blocking DIO’s email address. Which is why DIO is distributing the present DIO issue, with the offending paper right here at ¶2. Nothing new about this: it’s just copying the equally scientific, receptive, & ethical 1983
A The Shy Archon Triggering the Present Paper: Politics vs Science

A1 In 2011, DR belatedly responded to much-decorated astronomer Jack Brandt’s welcome request to consult a 1982 unpublished DR ms on the 54 star declinations observed by ancient astronomers Timocharis, Aristyllos, Hipparchos, and (allegedly) Ptolemy — reported and analysed at Almajest 7.3. In 2014, much-too shortly before the resulting paper Brandt et al 2014B went to press the Journal of Astronomical History & Heritage’s Editor Wayne Orchiston asked DR to refrain from doing so, though WO didn’t mention that its progress was already so far along towards publication that serious changes appear in retrospect not to have been feasible at the late date of JAHH’s request. (Not the 1st time [e.g., Rawlins 2008 fn 42] Ptolemites have asked skeptics to help them avoid blunders, even while underdeterminably determined to promote more cultism.) And, indeed, no changes were made, in response to central points challenged by DR’s scientifically detailed 2014/8/26 referee report, www.dioi.org/jau8q.pdf, though an irregular sprinkling of (non-space-expanding) alterations was effected. The timing suggests that the paper’s case for Ptolemy as outdoor observer was not going to be derailed by mere evidence, much less a full discussion of issues. Some other referees might care enough to regard such treatment as insulting which would only divert from the main point: it’s counter to a journal’s obligation — and one best interest is not to put the pains to do the necessary to take the whole of an issue’s respects and competent articles possible. (Not exactly an infectious ideal at brother history-of-astronomy journals, either.) The irony here is (as is obvious from correspondence: fn 28): DR went to plenty of trouble in a cooperative, generous attempt to help JAHH to be a more accurate and competent journal. It was disappointing to find that such considerations rank nowhere at the Journal of Astronomical History & Heritage, probably (despite JAHH’s pathetic ultimate cultishness and non-bravery) less from iniquity than from JHAish inability (increasingly typical of the whole ever-less-scientifically-skilled history-of-astronomy field) even to begin to tell balanced, competent technical research from cultist apologia.

A2 Brandt et al 2014B p.332 claim that the 2nd century AD star-data of Almajest 7.3 “could have been taken by Ptolemy himself.” The evidence for this politically-convenient falsehood? Ptolemy was alive when they were recorded! — a fact which did not require a new copy, for recording, since it’s been published for decades (at least) and has never been in dispute. No other evidence is brought forth favoring the claim, because there isn’t any supportive data whatever — all relevant evidences on the point are to the contrary (§C5 below). These were imparted to JAHH but never entered into its paper, which instead took seriously Ptolemy as observer, and promoted a fact-immune Ptolemy-alibiting pure-careerist like Brandt’s Puget Sound neighbor J.Evans as quotable Neutral Expert. (Brandt et al 2014B p.333: “The situation has been nicely summarized by [Evans 1998 p.262].”) So DR responsibly submitted a paper, “Ptolemy’s Fraudulence” (§2 above), to the JAHH, whose chief, W.Orchiston (formerly established in Oztrollya, like JAHH, but lately transplanted to Thailand) turned it over not to a specialist in the relevant science

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24 When asked to send Brandt his 3-decade-old star-declinations ms (later slimmed, revised, augmented and with some new discoveries of absolute latitudes, and published as Rawlins 1994Al), DR took the time to profitably review his 1982-1994 conclusions, sending his further-revised 2011 thoughts in a letter, www.dioi.org/bj/hg3.pdf, accompanied by the requested 1982 ms. Brandt certainly deserves credit for updating star-data (fn 40) and for stimulating DR’s 2011 discovery (§C21), which everyone including DR had missed right along (even though Rawlins 1994Al, had already concluded: (fn 159) was the Clean Dozen’s epoch): for epoch +159 (unlike for +137) the split between Clean Dozen & Sick stars was overlappedly clean: §C17. (But Brandt et al 2014B didn’t cite any of this.)

As we mourn the passing of technically able contributors to scientic history such as B.L.van der Waerden, C.Gillispie, W.Hartner, O.Neugebauer, C.Wilson, H.Thurston, A.Aabo, R.Newton, & S.Goldstein, we realize that they are being replaced (as JHU’s Harry Woolf warned DR 50s ago) largely by non-scientists. The new breed has proven admirably industrious, but too-often inadequately trained in science’s skills, criteria, standards, principles, and especially approach to evidence.

4See below at, e.g., §B4.

F IMPERVIOUS MERCURY

F1 The Alm 9.10 “proof” of Mercury’s mean synodic motion is purportedly based upon a 4-centuries-separated pair of geocentric longitudes: one of them at -264/11/15, the other at 139/5/17 (allegedly observed outdoors with Ptolemy’s putative armillary astrolabe). Using several Alm orbital elements for Mercury, Ptolemy mathematically derives the planet’s synodic longitude for each date. The mean synodic motion is then found by dividing the number of synodic degrees traversed during the interval, by that interval’s number of days.

F2 But the difficulty for Ptolemy’s loyalists is this: his Canobic Inscription’s pathetic ultimate cultishness — as for his home Serapic temple at Canopus from where he had only to look down the Mediterranean coast after dark to see the 12 nautical mile distant Pharos ame was slightly south of due west, so the two sites’ L could not possibly be the same. (Real L difference: 12 sin 30° = 6°.) Further evidence that Ptolemy “doesn’t seem to have allowed his eyeballs out at night” (Rawlins 1985G p.266).

G THE ARBELA ECLIPSE: FUMBLED PLAGIARISM: AND YET ANOTHER TWICE-FALSE FRAUD

G1 At Ptolemy’s GD 1.4.2, it is rightly contended that the most accurate then-available method for determining the longitude difference between 2 sites was astronomical: taking the difference between the local time of a lunar eclipse at site 1 and the local time of the same eclipse at site 2.

G2 Unless isolated from scientists of his world (a serious probability [fn 26 & 1 §F], with serious implications), Ptolemy had dozens of contemporary eclipse-comparison reports at his disposal. (Alm 4.6 and 9 use several eclipses of the 120s-130s.) But corresponding

values for his hometown Alexandria (fn 22) which were [1] seriously inconsistent with each other, and [2] even more seriously false in both instances (by -14° and -8°, respectively). In other words, an emphatic repeat of the Venus disaster of §C — the distinction being only that the Venus clashing-data-pair were faked while the Alexandria clashing-data-pair were plagiarized. Unwarily copying or mimicking others’ errors (e.g., §§D4-D5 and E1, fn 37; Bryce 2017A §§D2-D3) is the ever-lurking but ever-just pit that all plagiarists risk falling into.

E6 Also revealing of Ptolemy’s degree of empiricism is his astonishing listing of the Pharos (§E4) at exactly the same L (§EI) — 31°05’ — as for his home Serapic temple at Canopus from where he had only to look down the Mediterranean coast after dark to see the 12 nautical mile distant Pharos ame was slightly over thirty degrees south of due west, so the two sites’ L could not possibly be the same. (Real L difference: 12 sin 30° = 6°.) Further evidence that Ptolemy “doesn’t seem to have allowed his eyeballs out at night” (Rawlins 1985G p.266).
Text for the Day:

In the 1946 Alfred Hitchcock film Notorious, German spy Claude Rains suddenly learns he’s oops-unknowingly been consensually sleeping with a U.S. spy, and realizes that his fellow German spies would snuff him yesterday, if they discovered his security-breach. So a hushing-aide, who not from his wise mom, who consoling put out that it would never even enter their heads that their own choice as the ring’s most-exalted chief could possibly ever commit the ultimate espionage bladder. As she comfortably puts it:

You are protected by the enormity of your stupidity.

The point might be kept in mind by observers of the decades-ongoing spectacle of the history-of-ancient-astronomy field, whose most eminent journal and most prominent society — in tandem with a MacArthur Fellow and a Harvard prof serving as untruthful propagandists behind backs (in 18) the slander that no-one but a CRAZY2 person could suspect dishonesty of the history-of-ancient-astronomy field’s ultimate hero, ancient astrologer Claudius Ptolemy, who 4-times-out-of-4 reported observations of the Sun that were undeniably but captive-journal-unprintably 50 times closer2 to Hipparchos’ 280-old indoor tables than to the real Sun. The prime forums perpetrating this fantastical but profitable joke on academia and the public, for consecutive decades, are the Journal for the History of Astronomy and the American Astronomical Society’s knowingly2 unsupervised Historical Astronomy Division (HAD) — which we shall refer to as the “JHAD” combine. Seemingly incredible fact of the last 4 decades of the Ptolemy Controversy: not a single published defense of Ptolemy has ever been valid and most have not been particularly smart2 or honest,2 as we are about to see again/kagain below. But as with off-crafty Rains, the partisans of this fantasy-literature are protected by the very incredibility of the idea that such ultra-eminent forums and scholars could seem so stupid. (Also invisibly back-stabbing, slanderous, & deceitful: fn 18.) The gulf between the pompous mask and the dumb arguments that are insistently-put-upon (by people some of whom are normally as smart as Rains) is so beyond the comprehension — the very universe — of emotionally normal onlookers & pressfolk, that the latter have not, cannot, will not believe the solid reality of what has been the dispute’s historicity, even though oft meticulously documented in DIO.

H STAR CATALOG TESTS AND ANOTHER DOUBLE: PTOLEMY AS LOSER-MAGNET

H1 Had Ptolemy observed the Ancient Star Catalog via armillary astrolabe (described at Alm 5.1) with its ecliptic ring off by his notorious —1°.1 mean longitude error, the real and instrumental eclipses would be tilted by 1°/2 vis-à-vis each other (since the instrument foreign eclipse times couldn’t have supported the longitudinally-stretched geography (§G4) he borrowed (with credit)25 from Marinos of Tyre. Instead, Ptolemy’s vast opus provides (GD 1.4.2) but one26 example: two longitudinally-much-separated reports — 500’ old! — of the famous Arbela — 330/9/20 lunar eclipse’s start, saying it was seen there at 23° and in Carthage at 20°, thus proving that the 2 places are 3° or 45° apart in longitude.

G3 However, Pliny earlier reported the same data very differently: 20° (8 PM) for Arbela (modern embattled oil-city Irbil) & 18° (6 PM) for Sicily, whose west end (big city Lilybaeum) was part of Carthage’s empire, & of longitude anciently-known to be similar to Carthage’s: www.dioi.org/500.pdf cities D67&D131. Modern calculations27 show that non-astromer Pliny was quite accurate, while The Greatest Astronomer of Antiquity was amazingly wrong, over 2° off for Carthage, 3° off for Arbela. The former error nearly equals the actual entire 21/4 longitude gap between the sites, and the latter error far exceeds said quarry. But the weirdest part is yet to come: Ptolemy’s own lunar tables put the eclipse just about as much in disagreement with his reported times as modern tables do: 2° Carthage and 3° Arbela. How explain such an entertainingly disastrous fabrication? Start by consulting Pliny 2.72.180 on the —330/9/20 lunar eclipse. Reading the passage carefully, one sees that no numerical hour is given explicitly for the western apparition in Sicily, merely: moonrise (“exoriens”). By contrast, the Arbela time is given as the “2nd hour” after sunset, or about 20°, which is the very time Ptolemy gives for the Carthage report. Why? Well, look carefully at the Pliny passage cited: by a fluke of grammar, “secunda hora” appears nearer in the sentence to “Sicilia” than to “Arbelam”. This obviously suggests Pliny used Plinius or his source but (evidently unable to read Latin well) took Pliny’s 20° time to be Carthage’s.

G4 But how did Ptolemy arrive at 23° for Arbela? Since Gesellin 1790, it has been obvious that multiplication by an expansion factor (Diller 1984 §C5) had been applied by Marinos or Ptolemy or their source to a prior map’s accurate longitudes, creating the oversize longitude intervals of the GD. Rawlins 1985G eq.15 showed that the expansion factors were either 7/5 or 4/3 in the region under consideration. Assuming that the earlier accurate map correctly put Arbela 21°/4 east of Carthage, then expansion by 4/3 would produce 3°, the very gap — the very wrong gap — Ptolemy reports. I.e., typically for him — and his defenders — the conclusion was established ere the evidence was generated.

G5 He simply added this 3° to 20°, thus arriving at his fantastic 23° time for Arbela. NB: This solution adds powerful new evidence favoring the theory (still-foolishly-doubted: §§C4; §13B: Rawlins 2008G §1 & Rawlins 2008S fn 13&45) that GD falsely corrupted an accurate prior map by expanding its longitudes by a factor of 30%–40%. Collecting §§C&E with the present case, we now have 3 separate Ptolemy double-false fakes on display here.

25 Was Marinos cited partly because (unlike Ancient Star Cataloger Hipparchos) he was still alive to complain if uncredited? This question casts fresh light on the intended issue of whether the GD was out-of-date when completed. See Rawlins 2008S §K for further evidence that it wasn’t.

26 Due to modern communal non-recognition of occultist Ptolemy’s isolation from actual scientists, we find Neugebauer 1975 (pp.367, 667, 938) cornered into interpreting Ptolemy’s non-use of contemporaneous eclipses as having to mean that (www.dioi.org/cot.htm#c0h) there then existed no empirical scientific community to be isolated from!

27 The Battle of Arbela was fought at nearby Gaugamela and 11° after the eclipse. We find actual Local Apparent Times of the —330/9/20 eclipse’s umbral start: Carthage 17:43, Lilybaeum 17:52, Gaugamela 19:56, Arbela 19:58. So the Gaugamela-Lilybaeum difference in geographical longitude E is ΔE = 2°04’; Gaugamela-Carthage, 2°13’.

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2 Prime smear against dissident is Insanity (as with media on pols’ heresy) by megafunded establishment-polishers & darlings Gingerich (fn 16) & MacGenius Swerdlov (fn 4&18, § fn 35), scientifically-challenged (§2 fn 8) MacG even mirrorlessly calling JHU-physicist & JHAD-satan Robert Newton a Velikovskian “crank and a con-man”: www.dioi.org/j113.pdf, DOI 1.1 §3 §§D2-D3.

3 Nobody disputes the 50-to-1 indictment. But no Reputable Forum (including even popmags, newsmags & torch) clamors broadcast such heresy-supportive truth, either. The rigid decade-ante-date policy: hide it from the public. (Given the power-secretarial state of the nation’s Free Media Press, that’s not even a challenge. Consider: would archons behave as described here if they thought there was even a 1% chance the press would expose it?) E.g., in 1983, the Journal for the History of Astronomy so insisted on (at-the-last-minute, without-warning) deleting the 50-to-1 evidential crusher from a projected DR article, that the paper was suppressed by JHA, being finally published by DR 1½ later: original unexpurgated spread www.dioi.org/f913.pdf, Rawlins 1999 §E. Understand the attitude: the you the public just can’t be trusted with certain central facts, because you might “misinterpret” them and start believing something Unapproved. (Similarly at www.dioi.org/vols/wi0.pdf, DOI 18 §G13T3-T16.)

4 DR has asked AAS to monitor HAD’s “unprofessional” (Schaefer 2002 p.40) behavior: 2002/10, 2015/12/29, & www.dioi.org/jcx6q6.pdf, email 2017/6/26 (no reply) transmitting photographic proof of besmirching archon smear: www.dioi.org/pm1.htm, vs www.dioi.org/pm2.htm, tactics log onto known Ptolemaic recipients, a class which includes the AAS, whose chief in 2017 joined the deaf&dumbers.

Some authors may be able, but this breed of apology never quite is. Evidently countering such hopeless and comically self-contradictory (Rawlins 1992V §§C31-C32) effusions is not a serious challenge (“like shooting fishestories in a barrel of monkeys”: www.dioi.org/j31a.pdf, DOI 1.3 §10). And that is exactly why Ptolemists eschew (§2 fn 52) risking rational debate with DR, written (www.dioi.org/deb.htm) or spoken (13 fn 5), preferring character-assassination-stealth’s bravery: fn 4.

4 Ptolemists’ integrity-level (e.g., §§B6 below) generally shows up less in the (perhaps-unintentionally) deficient original paper than in subsequent failure to acknowledge its thesis-gutting flaws.
Ptolemy-Defense Cult Lays Yet ANOTHER Egg On Own Already-Unwipably-Eegregious Faces ArchonBishop of TruthBury’s Trowel & Slander

We now analyse the latest installment, this time from the Journal of Astronomical History & Heritage 2014, in a half-century serial display of unfailingly invalid archonal apology-defenses of Claudius Ptolemy, sacerdot sanctum-astrologer of the American Astronomical Society and its HAD: author of astrology’s bible, the Tetrabiblos, science-fabricator; & Almajest-perpetrator. The spectacular result has been one of history’s grandest compilations of establishment pseudoscience literature, all accomplished in the service of attempting to save the reputation of a “scientist” icon who was not a scientist at all, but (§A below) just a mathematician who faked science. Badly. Among Ptolemy’s numerous clumsy Almajest’s [the silliest on scintillating display here in [2] were, e.g., [a] 2 different dates (37 days apart!) for the same Venus maximum-elongation, [b] plagiarizing Hipparchos’ star catalog, [c] impossible-for-regular-observer ignorance of his own city’s latitude, [d] 4 alleged solar “observations” which were (as no historian-of-science denies; or admits) many times nearer Hipparchos’ old indoor tables than to the outdoor sky’s actual Sun.

The JAHH paper in question, J.Brandt, P.Zimmer, & P.Jones (below known as Brandt et al 2014B), attempted mathematical analyses of the Almajest’s stellar declinations, observed by four successive ancient Greek astronomers over nearly half a millennium. The journal and authors contend that the dozen Almajest-contemporary declinations could be Ptolemy’s observations, never warning the reader that zero evidence is provided to establish that claim, while simple, definitive, long-published, referee-urged proof to the contrary is below shown (§C5) to have been deliberately omitted. The paper’s “bivariate least-squares” statistical analyses were not bivariate and thus didn’t exactly find any least-sums $S_o$ of residual-squares, as is also demonstrated below (§C23). While observers’ epochs $E$ are nearly right (but not, attempts to find their geographical latitude-errors $x$ are revealed as grossly misguided, at a primitive level (§§C9&C12), though referee DR provided, ahead of publication, accurate $x$ & standard deviations) for all four of the ancient astronomers being analysed, solutions which could’ve been (but weren’t) crudely verified by elementary arithmetic, as will be shown here (fn 34 or §C23). Our discussion’s bluntness derives from the fact that, though Brandt et al 2014B is politely written, its knowing evidential omissions cooperate in trying to grant eternal life to an establishment myth — Ptolemy as Great Outdoor Astronomer — that rolls on, decade after decade, persisting only because the American Astronomical Society doesn’t care that its Historical Astronomy Division is deeply invested in a pathetically obvious historical lie, viciously (fn 4) defended by those HAD archons who long ago mistakenly decreed Ptolemy “The Greatest Astronomer of Antiquity” and thus have faces so at risk of megga-eggitual disgrace that they must forever encourage pseudo-science-for-The-Cause of forever-pseudo-controversy, cult-obediently incapable of admitting that any skeptic has ever made an indubitable contribution to knowledge. Below, at §B, the most recent misfire (Brandt et al 2014B) is put into the context of decades of like uniformly baseless mobaganda (though those interested only in 2014’s mismath may skip straight to §C3), which has by now brain-dirtied the mass of non-specialist historians that writing in opposition may be little more than preaching to the perverted.

1See Gingerich 1976 for 2 prominent examples of Believers (O.Neugebauer & himself) who got too deep into worshipping Ptolemy as “the greatest astronomer of antiquity” ever to reverse and escape their own self-created trap of constitutional inability to admit error, and who consider their image of Authoritative Wisdom to be a more important consideration than [1] the field’s sanity or [2] ever doing justice to pioneer genius Ptolemy-exposer R.Newton, upon whom they are proud to have done their own pioneering, in mum-becoming Newton as the field’s cohering hate-object (Gingerich 1999 p.364; Schaefer 2002 p.40) — before, since his death, honoring DR by elevating him onto the same pedestal.

rotates about the equatorial not elliptic pole: as we can see from, e.g., the educational paper model Evans has helpfully disseminated), so (Rawlins 1982C p.361 & Fig.2) we’d find error waves of amplitude 1°72 in the Catalog’s latitudes $\beta$ (cosine waves: $\text{ibid eq.4}$) and northern longitudes $\lambda$ (sine waves: $\text{ibid eq.3}$). We don’t. (Amusing details at Rawlins 1992V §§ C13-C15 & fn 31). See also the inspired findings of Graßhoff 1990 — which instantly quite dispense in the A1m’s, including a few ultra-giveaway cases where a star with an error of several degrees is found to have the same sized error, with the same sign, for both Hipparchos and Ptolemy.

H2 It thus became obvious e.1990, even to the most religious, that many Ptolemy stars were Hipparchos’. However, no archon was ever going to admit in print the plain truth: the establishment had been blindsided by proof that its challengers had been right all along — that Graßhoff’s test had now unexpectedly surprise-vindicated the long-loathed Tycho-Newton-Rawlins position that the Catalog was stolen. (The post-disaster spin of some was that the three pioneer skeptics had just regrettably not proven their case clearly enough to be understood by the judicious archons atop the American Astronomical Society’s Historical Astronomy Division [H.A.D.] — surely a truth-determination criterion to live by — while Graßhoff had. Which translates as: [a] ashamed refusal to acknowledge that — until the truth hit them in all their faces — believers had been too limited and predisposed to see anything significant in the same evidence from which skeptics had drawn the right conclusion years ahead of final proof; [b] denial of credit to unapproved first perceivers, served by four successive ancient Greek astronomers over nearly half a millennium. The error of several degrees is found to have the same sized error, with the same sign, for both Hipparchos and Ptolemy.

The post-Graßhoff era has been especially fertile for indiscriminate fallback fullbacks by Ptolemy’s (selectively) malleable modern choir, as the politically ambitious realized that the JHA would ever so gratefully publish anything that muddied the clear evidential situation, in order to save archons from facing apt appreciation for decades of falsely denigrating now-vindicated scholars: simply pretend vindication either never happened or isn’t 100.00000000%. Question: is anyone empathizing with the cornered defenders’ needless pain here, caused by artificial extension of the Controversy? (Puts one in mind of equally needless ongoing misery from other kinds of poverty than intellectual — massively less significant than its later emergence, possibly in other hands, supported by persuasive arguments.)

28 Rewards handed out to those who attacked the R.Newton satan include JHA boardship (R.Newton 1991 fn 2) and a MacArthur for miss-man Swerdlow. (It’s hard to find good help anymore.) Among other examples: maid-men Evans and Schaefer were elevated at JHA not long after their massive bunglings (1998 and 2002 attacks on Rawlins. (The unscrupulous here may actually be deliberate.) Selecting boardmembers by such criteria will damage mean-IQ atop JHA for decades to come.

29 D.Duke’s statistical studies indicate that very nearly all stars were appropriated. If Evans and Schaefer were right that Ptolemy probably outdoor-observed the whole catalog, Evans now just hopes that Graßhoff 1990 hasn’t proven that all29 stars were copied from Hipparchos. [2] Schaefer (2002) says the Yale Bright Star Catalog also grabs previous catalogs’ stars, so what’s the concern? However, both these defenses of Ptolemy’s integrity plainly founder upon his claim of 1st-hand observation of all1025 stars. And argument [2] is on the logical and ethical level of defending a bank-robber by pleading
that, well: doesn’t everybody withdraw money from banks? The BSC does not claim 1st hand observation, while The Greatest Astronomer of Antiquity explicitly 30 does so claim, at Alm 7.4, in lengthy detail, falsely saying he observed every visible star (§1K1). Bottom lines: [i] The JHA competed itself repeatedly to the proposition that the Catalog was all or mostly Ptolemy’s. [ii] It isn’t. [iii] But, simply from shame-factors detailed elsewhere here, our “premier” JHA-H.A.D. (JHAD) solipsistically hallucinates — like Dr. Frederick Cook or Alger His — that if we just never confess, then no one will ever know the truth: that we Experts lost what has correctly been advertised nationally by Schaefer 2002 as the hottest controversy in the field. [iv] But neutral observers increasingly and snickeringly do know — which is marking certain JHADists as losers to scholars they themselves have long been assuring the world are crazy dishonest paranoid incompetent cranks. And we’re not supposed to giggle? (You begin to see why the seething losers can never admit it?)

H3 Pickering 2002A §§1 & Fig.1 points out a history-of-science-ignored ultra-simple disproof of Ptolemy’s Catalog authorship (Rawlins 2000A fn 177): the 5° gap which should exist between the antarctic circles 31 of Hipparchos’ stars and Ptolemy’s stars (due to their differing latitudes) does not exist. The 2 circles are virtually identical. Controversy over.

H4 Several Sagittarius (Sgr) non-dim stars were missed by Tycho, so Evans 1987 p.168 (like Evans 1998 p.272) tries to create anticircle-anticircle ambiguity by stressing that these stars — well over 4° high, in what JHA Editor Evans calls “Sag” — are not in Tycho’s catalog, an argument put forth in innocence of the fact that Summer Solstitial non-darkness at the Dane’s northerly latitude L = 55°.9, impeded these difficult Sgr stars’ availability. (By recording Fomalhaut, Tycho actually went down to within 2°.6 of the horizon: Rawlins 1993D Table 17.)

I CATALOG FRACTIONS: JEKYLL’S SLYDE-&-HYDE COVERUP — KNOWING DESTRUCTION OF DATA

I1 The Ancient Star Catalog (Alm 7.5-8.1) has an obvious excess of 0° endings and 30° endings in the latitudes β, due to ancient Egyptian and Greek proclivity for expressing non-integers by using inverse integers: “unit fractions”. (Cause of both excesses detailed at Rawlins 1994L §§4.) But the most common ending for the longitudes λ is 40°.

I2 R.Newton 1977 (pp.245-254) showed statistically that this odd circumstance was simple to explain, once he’d discovered the key and unlocked the longstanding mystery: when Ptolemy stole Hipparchos’ stars, he naturally left the Catalog latitudes β unchanged, while updating all Catalog longitudes λ: 1°/century-precessing them by adding 2°.23, the false figure which Alm 7.2-3 claims stars precessed during the 2 2/3 centuries between the catalog epochs of Hipparchos and Ptolemy, —126.278 (Rawlins 1994L fn 45) and +137.547 (§D3), respectively. From slyding each longitude λ by 2°40′, 00′ endings became 40′; 10′ became 50′; 15′ became 55′ and were rounded to 00′; 20′ became 00′; 30′ became 10′; 40′ became 20′; 45′ became 25′ and were rounded to 20′; 50′ became 30′. (Note how the odd endings 25′ and 55′ got eliminated.) The frequencies of endings in λ and β are displayed by Rawlins 1994L Tables 1 and 2, and the whole slyde&hyde process is verified via χ² test (ibid §§B-C).

30 Before the Catalog’s theft became plain, no historian-of-science was insisting that Ptolemy wasn’t claiming observership (Rawlins 1982C n.3). Schaefer’s dodge (§H2 item [2]) was just the latest in the subsequent tradition of evading facing skeptics’ vindication. One even accent Ptolemy’s use of the word “we” when describing purported 1st-hand observations — a tack which wishes to refute the accusation that Ptolemy faked the Catalog by instead proposing that he plagiaristically stole credit for another’s work, theft without, as Pedersen puts it (above, §B2), “the slightest acknowledgement” of his actual source.

31 A northern hemisphere observer’s “antarctic circle” is the boundary of the segment of the celestial sphere which is ever-invisible to him. Ignoring refraction and extinction, said segment’s angular radius equals his geographical latitude L.

DIO-J.HA 22 12 The Greatest Faker of Antiquity 2018 D.Rawlins

R.Newton 1991. DIO 1.1 §§.
D.Rawlins 1982N. ArchiveHistExactSci 26:211.
D.Rawlins 1991H. DIO 1.1 §§.
D.Rawlins 1991P. Journal for Hysterical Astronomy 1.1 §§.
D.Rawlins 1991W. DIO&Journal for Hysterical Astronomy 1.2-3 §§.
D.Rawlins 1992T. DIO 2.1 §§.
D.Rawlins 1992V. DIO 2.3 §§.
D.Rawlins 1994M. DIO 4.2-55.
D.Rawlins 1994S. DIO 4.3 §§.
D.Rawlins 1996C. DIO&Journal for Hysterical Astronomy 6 §§.
D.Rawlins 2000A. DIO 10 [co-published with the University of Cambridge].
D.Rawlins 2003P. DIO 13.1 §§.
D.Rawlins 2003X. Isis 93:3-500.
D.Rawlins 2008Q. DIO 14 §§.
D.Rawlins 2008R. DIO 14 §§.
D.Rawlins 2009E. DIO&Journal for Hysterical Astronomy 16 §§.
D.Rawlins 2009F. DIO 16 §§.
D.Rawlins 2009S. DIO&Journal for Hysterical Astronomy 16 §§.
D.Rawlins 2012T. DIO 20 §§.
D.Rawlins 2012V. DIO 20 §§.
D.Rawlins 2017B. DIO&Journal for Hysterical Astronomy 21 §§.
D.Rawlins 2017E. DIO 21 §§.
D.Rawlins 2018U. DIO 20 §§.
D.Rawlins 2018W. DIO 21 §§.
Hugh Thurston 1998D. DIO 8 §§.
Gerald Toomer 1984. Ed. Ptolemy’s Almajest, NYC.

Thanks to all who’ve launched an explorer into a 9th decade of a life of unfenced curiosity. Blessed by felicitously often-confirmed inductions of the distant future of the distant past.
13 Most critiques of Ptolemy’s chicanery point primarily to the excess of 40 endings (vs 00 endings) in the Catalog longitudes $\lambda$, but (thanks to the 00’ ballot-box being [deliberately?] stuffed with the entire sample of rounded 55’s) the most shocking frequency-contrast is elsewhere (Rawlins 1992V §C22 item [e]; Rawlins 1994L fn 5): the spectacularly greater number of 10 endings than 30 endings. (Before Ptolemy added 2’40’ to Hipparchos’ $\lambda$, these were 30’ and 50’ endings, respectively.) Looking naïvely at the tabular distribution, the fact that 30’s are the least frequent endings is bizarre, since 30’ is nearly the most frequent latitude ending, as it should be. Indeed, for the latitudes $\beta$, 30’s are roughly twice as common as 10’s, but this is reversed for the longitudes $\lambda$, by far the strongest confirmation of R.Newton’s hypothesis for explaining the longitudes’ odd fractional-endings distribution.

14 Maintaining the Jekyllian pretense to being a genuine, respectable, outdoor astronomer, required the sneakiness of hyding the otherwise-glaringly-odd 55’ and 25’ endings (which Ptolemy’s addition-thievery had produced from formerly 15’ and 45’ endings), by secretly rounding them to 00 endings, where we cannot know whether it was 40’ or 20’ for Hipparchos.

15 So R.Newton ($\S$2) explained why longitudes $\lambda$ are near-befit of 15’s and 45’s (only32 5 in all: five 15’s, no 45’s), though appearing with roughly expected frequency for latitudes $\beta$: twenty-eight times more often than for $\lambda$.

16 Two other little-known extras regarding the Catalog: [A] Rawlins 1994L §§E4-E7 found statistically (at high odds) that the compiler of the Catalog’s southern stars observed from a place where he had estimated his geographical latitude $L$ at a value ending in 5/6 of a degree, consistent with the southern tip of Rhodes Island, Cape Prassonesi (latitude $L = 35°53’N$), but not with Alexander’s $L = 31°12’N$. [B] Shevchenko 1990 p.194 discovered for a specified half of the zodiac, stars’ $\lambda$ exhibited no particular excess of 40’s. Later, DIO 10 (2000) fn 177 tested Gem-Sgr (roughly Shevchenko’s range) and found that — excepting Sco, whose prime stars’ $\beta$ reach atypically far south for the zodiac — these stars’ original Hipparchos’ 00’-excesses were not in elliptical longitudes but in polar longitudes. The novel and insuperable impediment thus created for Ptolemy’s defenders is found in the footnote cited.

17 The root, of the persistence of the embarrassingly-long (given the evidence’s imbalance) “debate” over the Ancient Star Catalog, is that sneakily (fn 9); [b] Greek data-ineptitude’s unattested-but-alleged consistency with allegedly-normal science; i.e., forcing all Greek science it was adduced for, in ever-orthodox support of the 2 prime inter-related elder-dominant clique-myths regarding Greek astronomy that we’ve been discussing hereabouts: [a] Ptolemy’s honesty (fn 9); [b] Greek data-iniquity’s untested-but-alleged consistency with allegedly-normal science (fn 8; also Evans 1998 p.209) that allegedly-retained only theory-fitting data: i.e., forcing all Greek astronomers to fit a faker-astrologer: then, pre-post-erously, with Ptolemaic evidential circularity, using that very model to prove his normalcy, a truth-inversion warp that’s ruled the eld for most of a century.
J CONCLUSIONS

J1 The Alm is an invaluable resource, our only connexion to much of high ancient mathematics. Given what Dio has induced from it (passim), we are (as distinguished from R. Newton) especially grateful for its survival. But it must be used with extreme caution.

J2 The most educational observation we may end with, regarding the timorous state of the modern history of ancient astronomy community, is this: its fiscal rulership can read all that you have just read, and, though finding not a digit out of place in the analyses, conclude or emphasize that [A] Dio has done absolutely nothing, or [B] Dio has done absolutely nothing, and continue to be non-cited for [1] his witchcraft (fn 35) in co-hypnotizing scholars (fn 1) into realizing Ptolemy cheated, & [2] exposing the vile tactics of archons who'll (fn 1) admit they are wrong to slander. R. Newton before even understanding his evidence.

R. Newton 1977 (pp.365 & 379) thought Ptolemy's sham-universal "work displaced almost all of the earlier and valid Greek astronomy. If the Syntax had not been written . . . much valid Greek astronomy now lost would have been preserved directly. . . . we do not owe Ptolemy our thanks for the small amount of earlier astronomy that he has preserved. Instead, we owe him our condemnation for the large amount of genuine astronomy that he has caused to lose." (Note Toomer 1984 p.1’s naïve guess: "the work of Ptolemy’s scientific [1] predecessors . . . being obsolete, . . . ceased to be copied.”) See Neugebauer 1957 p.145 & Rawlins 2008Q fn 232 agree with RNR’s estimate (idem) that Dio Ptolemy reports from others are faked though (as most loyalists don’t deny) routinely led to produce impossible consistent “confirmations” of his models. Basic confusions of those who see Dio’s derivative astronomy as primary: [M1 [b].

It is little understood or anticipated that any decision to shun (usually made in anger or fear, initially) needlessly and precipitously puts the instigator’s integrity at risk. For, the decision is a bet: gambling that the shunned is forever worthless — oblivious to the possibility that his output may prove valid (or later start to be), at which point, how does the bully-invested, no-turning-back shunner then justify continued non-citation? For saving faces (and what else matters to archons?), he has no choice but: (a) not knowing (as he gets progressively more off the scent) that even the deceit can stop. As we get to the point where evidences CENTRAL TO THE FIELD (e.g., §§N1-N17 and N18; fnn 1, 7, & 47 item [1]) cannot be openly discussed without fear of archons — or editorial submission to such (fn 1) — we’ve descended into knowledge-destructive sociopathy.

Among JHA-circle herd-talk compliments toward R. Newton and his solid mathematical analyses had always been the theme of openness & honesty in the history-of-science world: [a] In 1992, complaint from sometime boardperson R. Kargon caused temporary cancellation of Johns Hopkins Univ’s Library subscription: of Dio 1.1.1 [C7 & §3 D2-D3]. And see Scientific American at fn 52. If taking academy’s pretensions at faces-value, one might’ve thought that the JHA or the American Astronomical Society’s H.A.D. would insist upon its officers in rational academic discourse. Instead, we’ve had 4 decades of their publicly inexcusable and/or equivalently inexcusable non-citation of work in a field with a limited number of puzzles, where endeavors inevitably overlap. Difficult with [ii]: among the medium 2 fn 34): will bring much valid Greek astronomy will ever rank uniquely as man’s 1st great scientific achievement, it may seem difficult to distinguish between [i] those meant to impress archons with their achievements, it may seem difficult to distinguish between [i] those meant to impress archons with their

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125 Jhshunning (longstanding: above, fn 109, & Rawlins 1991W fn 171 & 173 of Diller, Newton, & Dio is known to all in the JHAD-circle, resorted-to from careerists’ fear that honest critics are simply bad-for-business.

126 DIO is common knowledge throughout the JHAD circle. (Can’t blame on Rawlins’ acknowledged shortcomings, for sedate gentlemen Diller, Newton, & Thurston [www.dioi.org/pm3.htm] were shunned from 1934, 1968, & 2003, resp, most of them years before Rawlins barged into The Ptolemy Controversy in 1976. Even highly expert British felt he’d been somewhat exiled, as he imparted to Duke, for honestly owning that Ptolemy’s solar errors were not innocent: fn 122.) But, given the above consistently weird incidents, one needs no inside dope to smell heresy-banishment, along with the temptation that attacking the most archon-resented heretics (no matter how shoddily they do it) will bring decades of knowledge-subtraction. The shunning of the magnum that R. Newton & Dio represented?

For the ancient astronomy field, has Curtis Wilson’s cleansing hope (fn 59) been snuffed? [3] Greek astronomy will ever rank uniquely as man’s 1st great scientific achievement. Its lofty place in human history need not be desecrated by archons’ mundane limitations.

For weighing of competing theories by such criteria as simplicity (% above), probability’s relation to confirmation (% [2] N15), minimal hypotheses (% & fn 122), fruitfulness (% C3 & B, E, F), predictivity (% F & G). Instead of by herd-grandmanship.

J2 What simple, Occamly-coherent theory explains the serpentine communal behavior detailed above? — targeted non-citation, desperately indiscriminate “alternate” solutions to (aimed at anyone upsetting archons’ tenets or pretensions) — the single spare solution that you have just read and, though finding not a digit out of place in the analyses, conclude or pretend that: [A] Ptolemy has done absolutely nothing, or [B] Dio has done absolutely nothing. The mathematics. Given what R. Newton has done absolutely nothing, or [B] Dio has done absolutely nothing, and continue to be non-cited (fn 1) — we’ve descended into knowledge-destructive sociopathy.

125 Jhshunning (longstanding: above, fn 109, & Rawlins 1991W fn 171 & 173 of Diller, Newton, & Dio is known to all in the JHAD-circle, resorted-to from careerists’ fear that honest critics are simply bad-for-business.
among almost-as-accurate proximate ratios (www.dioi.org/thr.htm#epc), or [d] explain why each solution emerges as a ratio, a glaring feature of ALL pre-Ptolemy adopted lunar motions, which by contrast to orthodoxy is accounted-for perfectly inevitably by the eclipse-period-ratio solution. Jones’ blinded private rejection (by subsequently-undercut reasoning) of the DIO draconitic solution, flew all of the overkill-numerous, solid, unambiguous evidences consistent with said theory, particularly its PRECISE match to Hipparchos’ draconitic 5458° = 5923° arc (Almajest 4.2) by pairing an early apogee eclipse, −1244/11/13, with his Rhodos-observed −140/12/7 eclipse, the very same perigee eclipse which he uniquely had also previously (Almajest 6.9) paired with a less early apogee eclipse (−719/3/8) for exactly the same draconitic purpose, with inferior result — inevitably, due to shorter timebase. Comments: In history, no astronomer but Hipparchos ever used an apogee-perigee eclipse-pair. Scoffings at the theory’s outrageousness-vs-orthodox-preconception inadvertently compliment it by reflecting its potential advance’s enormity.

I38 §136 presents three perfectly fitting 2002-2003 eclipse-solutions to Greek adopted monthlengths. Why shynners’ 1st-impotence in finding DR-errors? Or alternate solutions? Answer (§134): there IS no other umbral lunar eclipse-pair whose integral-months ratio precisely, proportionately, directly yields (by Almajest 4.2&6.9 method) any of §136’s three attested, never-before-solved Greek integer-ratio motions, with: both eclipses visible in Greek-Babylonia region, latter eclipse within 50° of discovery-date (c. −262, −140, +136, resp), earlier eclipse not ere Almajest 4.6’s 720/3/19 (oldest eclipse-data historians-of-science accept that Greeks possessed: Toomer 1984 p.166 n.59). To pioneers who undo the above negative assertion by finding, before 2020/1/1, real umbral lunar eclipse-pairs directly solving the ratio in question (under above specs, incl.hist.sci’s own 721 BC bound), DIO will gratefully grant: $10000 for 3277/15121, $20000 for 6247/66951, $30000 for 5458°/5923°, $40000 extra for all 3 relations. [To certify the bet, DIO will deposit $50000 with the National Academy of Sciences, if it will hold same for winners until time’s up.]

J How to Hide from Reckoning: Get Thero to a Shunnery

J1 The foregoing suggests shortcomings in the modern ancient astronomy subfield re: [a] Scrupulous & neutral refereeing. [b] Turfless generosity & citational integrity. [c] Open-minded curiosity and tolerance (Hoskin, Evans, Jones, and Toomer have fled contact with Rawlins for years, e.g., Thurstor 1998D at 200 without a professional-survival priority — nay, necessity — of treating archons with an attitude of nondissent, even supplication, to allay (non-imaginary) fear of being unpublished or outright blackballed, as if such etiquette-issues outrank (§135) academic skills & integrity. (And o-yes accurate history.) [d] Scientific skills (or regular consultation with able scientists), & especially the purest scientists’ attitude of humble subservience to evidence (acquired by careers of frequent empirical contradiction). [e] Celerity of incorporation of knowledge-advancements (vs fn 20 here: 84 years!!) that will vastly outlive historians-of-science’s grasp of the enigma — the sheer brilliance of Hellenistic science. [f] Essential, genuine neutrality and curiosity (hardly compatible with a cult’s insistence on aggressively protecting sacred viewpoints for decades on end), enhanced by willingness to hypothesize-explore — ever subject to evidence-congruity (e.g., fn 16&83 vs fn 20&25&40). [g] Philosophy-of-science&common-sense Oscamite theory of fabrication explains both and to the Almajest’s 1st precision. Therefore, in each case (lunar or solar, which approach would Occam prefer?)

122 Half-dozen evidences for Hipparchos as author of 5458° = 5923°: Rawlins 2002H §C.

123 Neugebauerians long taught that 6 cuneiform-tablets’ lunar calculations for c. −200 proved chronologically Hipparchos (c. −130) took his draconitic equation from Babylon, until Rawlins 2002H §D1 pointed out: [a] the only 3 early tablets using his equation were the only 3 not dated on the clay, and [b] there’s a 7th tablet that is clay-dated, using his equation, but the date is after Hipparchos. As willfully as in fn 28, Jones ignores (private communications) not only this & fn 123’s flock of coherent evidences, but also rejects an unexpected key eyeopening spinoff-benefit: recommending resoundingly increased caution when time-placing non-clay-dated astronomical cuneiform tablets: ibid §D1.

K APPENDIX 1: GRUSOME TESTABILITY WARS

K1 Delambre 1817 2:284 was 1st to notice that, in Alexandria’s 2nd century AD sky, some stars which transited a few degrees above the southern horizon, and were bright enough to have been recorded by hypothetical-observer Ptolemy, were nonetheless not in “his” Catalog — and, by-an-unfunny-coincidence (explored statistically in Rawlins 1982C, all these uncataloged stars were invisible to Hipparchos, who observed 5° north of Alexandria, so that his antarctic-circle (fn 31) of invisibility was radially 5° bigger than Ptolemy’s (swallowing about 4/3 more sky). Automatically fighting the probable implication while unable to deny any facts, Evans 1998 p.272 resorts to the improbable (§B3 [a]), in order to set aside such simple antarctic-circle testing, speculating that because no previous mass-star-cataloger known to us had resided so far south as Ptolemy, there were no constellations to which he could attach stars in the 5°-wide strip of sky he could see but Hipparchos could not (and which no hypothetical early southern constellation had filled), so we must excuse Ptolemy — excuse him, that is, for not doing what The Greatest Astronomer of Antiquity himself actually says he did, namely, record all visible stars (Alm 7.4, Toomer 1984 p.339): “we observed as many stars as we could sight down to the sixth magnitude.” Evans’ dodge typifies modern Ptolemy’s death-agonies: allying one’s self with a crime too often requires possessing another. (Clue: try simple test. C. final sample, fn 30, 37, & 42.) I.e., even if one accepts Evans’ argument, it simply exchanges a charge of plagiarism against Ptolemy for a charge of lying. But Evans’ theory fails anyway since this paper’s revelations. (Ref-report to JAHH: “If DR revises the manuscript, I would be happy to look it over.”) Just as Gingerich did with R.Newton forty-seven years ago [now 50!—] details at Rawlins 1994S §B13. After all: must protect even ‘til-now-undeled Thailand and the antipodean Land-of-Oztrollya [§4 §A2] from the full truth about the integrity & ability of Ptolemy and his never-too-far-away Muffin public-relations organ.

36 Not the 1st time cultists trying to save Ptolemy (or pan-Babylonianism): §N13 must resort to spurning Ptolemy’s own claims (Rawlins 2002H §C7; DIO 11.1 p.26; Rawlins 2002V fn 12).

37 Similarly, a recent paper (Brandt et al 2014B: see also fn 20) tries to deny the certainty of Ptolemy’s fabrication (from Hipparchan data) of any star declinations δ found in Alm 7.3. Various modern scientists have noticed that Ptolemy “proves” his false 1°/cy precession from his 18-star sample using Hipparchos’ data, and ignores the “Siclivos” (Orch) he validated the (Close) distances have accurately yielded 1°/cy. From the consistent 6-fold persistence of the SickSix stars’ neatness, Rawlins 1977 pp.220-225 realized that Hipparchos had typically (above, §B2) just indoor-computed the SickSix declinations from his 7°/cy theory and then turned around to “prove” 1°/cy precession from said facts. The 2014 paper instead conjunctes that Ptolemy quietly stole stars from a Lone-Mystery-Observer of 57 AD, though the proposed LMO is uncit by Ptolemy or any other ancient, so his existence lacks the slightest independent evidential indication. And the proposal inadvertently trades a charge of fabrication (Newton, loc cit) against Ptolemy for a charge of plagiarism: yet another example (above, §K1) of Ptolemist fumable-lawfying to refute one of the hero’s crimes while not remarking or even noticing that they’re simultaneously stipulating to another! And, again-typically (above, §B3 [a]), choosing improbable theory over probable. Unconsidered question, quite aside from the issue of theft: from where and with what degree of inexcusable enhanced by willingness to hypothesize-explore — ever subject to evidence-congruity just-so-happened to pop up at the very time that agrees with precession-decident fabrication from Hipparchos? — an epoch that’s nearly the same for Ptolemy’s declinations-list AND his Catalog.

NB: This is proposed as a central new theory in a DEFENSE of Ptolemy’s honesty? Innocently unarticiated by the paper’s authors: the 2014 paper’s proposed 57 AD date for the former is near the date which Peters & Knobel 1915 p.15 found for when the Star Catalog’s mean longitude-error is null: 58 AD. But we know from the Catalog evidences (§J) that the Sidereal Catalog’s longitude error can deviate to all λ (in Hipparchos’ −126.278 catalog) causing an 8 decade shortfall from the intended 137 AD epoch, thus the Catalog’s naïvely-indicated 58 AD date is just as much an illusion as defenders’ proposed proximate 57 AD date for the Sick δ. If not, then are we to suppose that our secret LMO just-so-hastfully-to-pop-up at the very time that agrees with precession-deficient fabrication from Hipparchos? — an epoch that’s nearly the same for Ptolemy’s declinations-list AND his Catalog. NB: Ptolemy’s SickSix fraud-mimicry in stellar declinations is especially obvious since his outlier Arcturus’ δ conspicuously is correct for just over 50° later than the date consistent with the remaining stars —
various of the non-cataloged Alexandria-visible stars were conveniently attachable to nearby constellations. And we know that Ptolemy was (or copied) a star-attacher: Alm 7.5-8.1 lists dozens of “informate” stars which are in the vicinity of traditional constellations though still outside them, but which he nonetheless appends to them. This even includes Arcturus. (Which we designate as PK110 — meaning star #110 in Peters & Knobel 1915.) Further, the vast constellation Argo (today broken into pieces: Car, Vel, Pup, etc) had already been recognized for centuries, and the Catalog includes 45 of its stars (PK849-893); yet several Argo stars aren’t in the Catalog (bright but unCataloged PK295-300), as suggested by Ptolemy’s including (into PsA) a star only 5⁰ from ζ Car (PK1022), a star only 5⁰ from ζ Car (PK1021) versus 10⁰ from α and δ Gru, and 7° higher than δ Gru in declination, which connects to why Gru was visible to Hipparchus while α and δ Gru were below his horizon, and δ Gru’s μ = 7.1. So, why was Gru Cataloged while the other 3 stars weren’t: [1] the three’s distance from Ptolemy’s PsA? or [2] Hipparchus invisibility? Answers: [1] Gaps exceeding 10° between constellations’ prime stars aren’t rare. Cep: γδκ (PK767&777) 11°. Aqr: βκε (PK632&636) 12°. Peg: γκο (PK316&318) 17°. Hya: βκγ (PK916&917) 22°. So the no-available-constellations argument is slaughtered by [2] the obvious explanation for γ being the Catalog’s sole Gru star, namely: of α, β, γ, and δ, only γ was visible to Hipparchus.

the SickFive, whose weighted mean error vanishes c.59 AD — since he faked it to co-prove false 1°/cy from Ptolemy’s ι PsA by very-incorrect Arcturus δ which was accidentally correct for a time later than his actual epoch by (you guessed?): c.50°. To emphasize this glaring giveaway by summation: if the SickSix sample were actually observed in 59 AD (& its SickFive do indeed have near-null declination-error for that date), it is remarkable that the lone Ptolemy star (Arcturus), whose δ-error also goes null c.50° later than Hipparchus. (To be exact, 52° after Hipparchus; 56° after LMO.) Hipparchus’ μGru was a bit dimmer than 5°: §3. For his deep south sky, virtually every identifiable star brighter than μ = 5 appears in the Catalog. Note: this is about the μ (Rawlins 1993D §4) at which Tycho’s normally high star-cataloging accuracy begins to fade. (Tycho had 14% less sky visible to him than Hipparchus, so he had to go a bit deeper in α to approach his goal of netting 1000 stars.)

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Sourcing Ptolemy’s lunisolar ratio,\textsuperscript{133} 105416\textsuperscript{a} = 8523\textsuperscript{2}, occurred 2 decades ago (all 10![1] digits exactly elicited) by test-exploring Greek awareness of the 800\textsuperscript{0} sidereal eclipse-cycle nest (1/5 of 800\textsuperscript{0} cycle attested: Geminus 8.40:41): solution, awareness, & nest not suspected ere Rawlins 1996C eq.31. (Sidereal year accuracy: \textit{ibid fn 110.}) Royal Muftia Cavilliers have produced no math error or alternate solution since. Predictable result is (see Rawlins 1996C's title and boxed 2013 statement atop its p.2): permanent silence.

More muteness greeted D\textit{IO}'s 2002-2003 discovery that all 3 previously unsolved, anciently adopted mean motions of the Moon (1. System A; 2. draconitic; 3. Ptolemy’s last lunar equation)\textsuperscript{134} were exactly consistent with discovery by ancient scientists who merely divided an eclipse cycle ratio by whatever integer or half-integer was common to both of the ratios. Ptolemy at just the way Ptolemy at \textit{Almagest} 4.2&6.9 explains: outlasting months synodic, anomalistic, & draconitic. Notably, no matter where, over a 400\textsuperscript{0} span (3\textsuperscript{rd} century BC to 2\textsuperscript{nd} century AD), the pairs’ latter eclipses are located in time, all the prior ratio-solving eclipses turn out to be from the \textit{very same century}, the \textit{thirteenth BC} (§136). One might suppose the center’s largely old-guard pan-Babylonianists, would welcome the prospect that such remarkable Greek triple-accuracy could have a fundamental & irreplaceable debt to Babylon and would delight in the potential new vistas opened by these astonishingly exact matches. Instead, the entire history-of-ancient-astronomy shunniness, frustrated by inability (like §133) to find math error or \textit{alternate eclipses to show non-uniqueness}, has been forced to just datalessly scoff (chief sneerleaders: A.Jones and D.Duke) at the very idea of such remote eclipse records as ridiculous a priori. But perhaps neither snickerer has heard about non-cult\textsuperscript{135} scholarship by Johannes Koh who had already (year-earlier) estimated Babylonian observational tolerance as about \(1/350\). Surprise realization that Hipparchos’ famous 600\textsuperscript{0} lunisolar tables effectively went back just that far only occurred\textsuperscript{135} in 2015.

So we should gauge the proposed eclipse-ratio method by comparing it to what may be verrry loosely referred to as “the competition” (e.g., fn 119). Facts: No other method is attested. (Twice: \textit{ident}. No other method is so simple & immediately-direct-to-the-result. No other method could ensure such high accuracy, 1-part-in-10\textsuperscript{3}, 3 times out of 3, eliminating false nearby period-ratios (§137). No other method than eclipse-period \textit{integral ratios} so naturally accounts for why all said motions were expressed as \textit{integral ratios}. No other method explains the 4-digit size of each ratio’s 2 components: \textit{as in Almagest 4.2&6.9}. No other credible (fn 119) method, attested (or unattested) has math-reproduced ANY of the numbers sought, while D\textit{IO}'s proposal has done for ALL 24 digits precisely — that is, all six 4-digit components — on-the-nose in each case: see www.dioi.org/thr#ctpp.

\textsuperscript{133} PianHyp 1.1.6 (Heiberg 1907 pp.78-79 or Neugebauer 1975 p.901 eq.3); 105416\textsuperscript{a} = 8523\textsuperscript{2}. Solved: Rawlins 1996C eq.20-31. Thanks to K.Moesgaard for a perceptive correction.

\textsuperscript{134} JHA 116. The admirable exception to Hist.sci ignoring ancient monthlength accuracy: Pedersen 1974 pp.164&424. But he does not realize how such accuracy was achieved, nor does he go on to challenge the anti-empirical orthodoxy we saw at fn 8.

\textsuperscript{135} M. Moesgaard 1992 p.474. Initial Muftia tactic vs R.Newton & \textit{DIO} was non-citation. But Isis Editor Margaret Rossiter’s publishing \textit{DIO}-respecting Thurstoon 2002S defied the 30\textsuperscript{0} shun, inspiring (\textit{what else from pathologic unregenerates}?) \textit{DOUBLEshun}: [a] Thurston’s swift ever-exile from JHA (www.dioi.org/pm3.htm); [b] \textit{DIO}-citations’ end in AAS-HAD’s Newsletter & [c] Isis’ Cumulative Bibliography; [d] during Thurston 2002S’s refereeing (2000), the usual indiscriminate (fn 66) unreviewed anti-book critiques for launch. Schaefer 2001 (Pb), Schaefer 2002, Jones 2002E (2\textsuperscript{nd} to Pb), Duke 2000T; Duke 2005W (Pb), Jones 2010B (2\textsuperscript{nd} to Pb), as pos outsid each other (to squash #1 blackballei \textit{no matter how}; §§B-G), all now on JHA’s certified-Premier (fn 42) board. Re JHA’s prior villain, we quote from fn 22: “Rewards handed out to those who attacked the R.Newton satan include JHA’s boardship (R.Newton 1991 fn 2) & a MacArthur for miss-man [fn 96 here] Swerdlow. (It’s hard to find good help anymore.) . . . maid-men Evans & Schaefer were elevated at JHA not long after their massive bungled 1996C-2002 attacks on Rawlins. (The usability here may actually be deliberate.) Selecting boardmembers \textit{hastily} will damage mean-IQ atop JHA for decades to come.”

\textsuperscript{137} For this recent shock, see www.dioi.org/thr#bkbv. Re Hipparchos’ 600\textsuperscript{0} tables, see Pliny 2.9.53.

\textsuperscript{133} Likewise, Schaefer 2001 proposes that atmospheric aerosols instead of rocks blocked Ptolemy block just-enough southern sky as to make his putative observations’ \textit{discernment}—range-deceptively look\textsuperscript{2} as if the observer were at Hipparchos’ latitude \(L\) instead of where Ptolemy’s

L \textbf{APPENDIX 2: THE MAGNITUDE SPLIT (AND WHY WASN’T PLUTO KNOWN TO ARCHIMEDES?)}

L1 The bottom line here is a circumstance which Evans 1998 p.272 has convinced himself is “entirely normal,” though it is unique among historical complete star catalogs: \textit{every star in Ptolemy’s catalog is higher than six degrees above his southern horizon} — which is of course just what one would expect of a catalog stolen from an astronomer who worked about that far north of the thief. Note: no other original naked-eye 1000-star catalog’s lowest star was as high as \(3^2\): Hipparchos, Ulugh Beg, Tycho, Hevelius. That is, Ptolemy’s ‘entirely normal’ lowest star’s \(6^2\) altitude is more than double the altitude of anyone else’s lowest star.

L2 A passing allibi by Evans 1987 p.166 even imaginatively hint that perhaps there were, say, rocks just-south of Ptolemy’s putative observatory that just-so-happened to block just-enough southern sky as to make his putative observations’ ‘discernment’—range-deceptively look\textsuperscript{2} as if the observer were at Hipparchos’ latitude \(L\) instead of where Ptolemy’s
The Greatest Faker of Antiquity

2018 D. Rawlins

M APPENDIX 3: CIRCULARITY, PREMATURITY, DERIVATIVITY AND FIVE MISSING SECONDS

M1 How did too much of the academic establishment get sucked into promoting astronomical history’s ultimate pretender as the “Greatest Astronomer of Antiquity”? [a] Were public attacks on a famous scientist resented by science’s politicians as endangering science funding? — but as astrologer-mathematician Ptolemy was not a scientist. Not empirical.

any other altitude than $h = 0^\circ$; see the lucid and irrefutable discussion at ibid [F11]. Further, thanks to an argument marshaled by B. Goldstein, we now have the fact (Rawlins 1993D [L8]) that Ptolemy said in so many words that 1st magnitude stars (pre-extinction $m = 1$) were visible on the horizon in antiquity. (In the exchanging-frauds tradition noted at [K1], some loyalists disbelieve this, thereby [ibid fn 93] assuming Ptolemy’s “horizon-stars-dishonesty [in order to argue] his Catalog-stars-honesty”.) But, if we assume Evans’ preferred (0.20 mags/atm) modern-model opacity, then 1st magnitude stars’ visibility on-horizon entails (ibid [L8]) ancients’ eyesight perceiving 12th magnitude stars ($\mu = 12$); and an atmosphere’s 0.20 mags/atm similarity entails seeing to 14th magnitude ($\mu = 14$). Why? So, why didn’t Archimedes beat Clyde Tombaugh to the discovery of Pluto?

43 Define split-Vagueness V in §3.3’s list [a]: dimmest Cataloged star’s $\mu$ minus brightest non-Cataloged star’s $\mu$. Testing Hipparcos’ $\mu$ (γ Ara vs κ Cru) at Cape Prassonesi (height $z = c.200$ m above sealevel) for five assumed sealevel opacities: 0.14 mags/atm (negligible aerosols), 0.15 (Rawlins 1992B, 0.17 (Evans 1987, #2), 0.20 (Evans 1987, #1), 0.23 (Schafer 2001): $V = 1/5, 1/4, 1/3, 1/2, 3/5$, respectively.

Compare sealevel-Alexandria Ptolemy’s §3.3 list [a]: $V$ at 0.15 mags/atm, $V = 5/4$ (β Sgr vs α Cru). And for mags-atm = 0.23, 0.3: $V = 4/5, 3/4, 3/5, resp$ (γ Ara vs α Phe in both cases).

44 Among those tested by Rawlins 1982B Table II, the only major star ($\mu < 3$) that seems unambiguously to be missing from the Catalog is $\mu$ Vel ($\mu = 2.7; \mu = 3/4$ for Hipparcos, 3 1/5 for Ptolemy). A speculation at DIO 4.3 §14 showed how star PK064 could be a mangled version of a position originally based upon an hypothetical observation of $\mu$ Vel, high by 1/5h in RA (3° great-circle), but in declination fully accurate to ancient precision.

45 The half-century Ptolemy Controversy should have been over in half an hour — had all participants amicably cooperated at the outset and sat down together to compare data and enlighten each other. Had defenders resolved early on, before positions hardened in ignorance of, e.g., the significance of Ptolemy’s large error for Alexandria’s geographical latitude $L$ (§E1) and real ancient scientists’ high-accuracy achievements (§M3), participants would (Panglossianly assuming open minds all around) have soon shaken hands, parted in peace — and moved on to more challenging historical mysteries. (As DIO long since has, most of our research on ancients being reconstruction of lost astronomy.) Instead, Ptolemy’s promoters from day-one followed his example by knowing all the answers before consulting either empirical evidence (as against texts) or actual able scientists (as against cult goorooos). Decades of ugly and harmful warfare followed. Again: all needless. But as with many wars, when it becomes obvious to most observers who’s going to lose in the long run, there is a bloody period when
Physics, Scientific American, Archive for History of Exact Sciences, even a well-known physics-textbook, & currently is the cover article of the 2018 Aug Griffith Observer.

Question #1: Is there yet the slightest visible evidence that any — ANY — one of our unanimously deaf&dumb shunners even understand the physics here?

Question #2: Do archon cynosures realize that the 6/5 factor has been standard among navigators&astronomers for over 100? (All scientific navigation manuals have horizon-dip shrunk by $\sqrt{5/6}$ vs straight-line geometry) and horizon-distance expanded by $\sqrt{6/5}$ vs straight-line geometry, both due to atmospheric refraction. See, e.g., the Bowditch.)

Question #3: Would it matter?

Hypothesis-discoverer Rawlins' own 1996 case for re-evaluation (emphases in original) follows.

DIO's new PHYSICAL — not standard kneejerk-metaphorical — theory (assigning both ancient [Earth-C] values’ error to [atmospheric] refraction) simultaneously solves... both the (very discrepant) Eratosthenes & Posidonios domic values ... (through) a single value for the stade: the same... 185m value... found even in most dictionaries.) No other simple, coherent theory does so. [Classic Eratosthenian stade-scruncher J.Dutka... claims that the reason for the 180,000 [stade] 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 — [Eratosthenes’].

Can there ever be rational discussion here when the only theory that fits all 3 data (both C, as well as the standard 185m stade) is not even understood by those who keep proclaiming churning out forced metrological retreats (as recently as late 2016! — fn 97 here), none of which can fit more than one of the 3 desiderata; and even that single fit is often several times worse than 1%. Hint to metropolitans: your century of stade-tweaking has been obsolesced — simply no longer needed to explain disparate C. Note 3 hyper-ironies here regarding Eratosthenes’ Earth-Circumference experiment, often seen as the most enduring astronomical legend of all, and the subject of centuries of failed metrological speculations.

[Since almost all protected ideas are false, protection is itself evidence of falsity.]

M2 Admittedly-non-peer-reviewed Scientific American’s Swerdlow-Gingerich-inspired premature “Acquittal of Ptolemy” (ScAm 1979), published in anti-Thoughtcrime horror at R.Neptune’s scientific 1977 exposure of Ptolemy’s career of fabrication, could hardly have been more ill-timed (fn 12) or more extreme in fundamental-premis misunderstandings of Ptolemy in particular and ancient science in general (most of which survive immutably to this day among his remaining band of believers). It verbatim-echoed the already-echoed premature “Acquittal” promoted Swerdlow-Gingerich’s idea of established-truths regarding Eratosthenes & Ptolemy’s Earth-C values at issue, & with no sign whatever of understanding the paper’s physics. Strabo’s arcs (where Earth-curvature is apt to a meridian circle of circumference 39870000 m): 5000 stades Alexandria-Aswan & Aswan-Meroë, each good to ordmag 1 for 700 stades/degree: $7^2/1$ + $7^2/1$ = $14^2/1$. (Rawlins 2008Q & note ibid [M4[a]) that early-Ptolemaic survey-based Earth-circumference determination was just legend. This can be seen as showing DR’s poor judgement. Or desire to learn. Or both.]

Dutka 1993 p.64 cites Rawlins 1982N — whose App.A explicitly links 6/5 to lighthouse and 5/6 to sunsets — without ([126] citing the paper’s atmospheric refraction theory that explains these felicitous fits to the 2 respective ancient C-values at issue, & with no sign whatever of understanding the paper’s physics. Strabo’s arcs (where Earth-curvature is apt to a meridian circle of circumference 39870000 m): 5000 stades Alexandria-Aswan & Aswan-Meroë, each good to ordmag 1 for 700 stades/degree: $7^2/1$ + $7^2/1$ = $14^2/1$. (Rawlins 20098 3C notes Philo’s solar work at Meroë, presumably for an imperial survey.) Testimony for early-Ptolemaic meridians in 60th: Strabo 2.5.7 (Eratosthenes); also Geminius, etc.: Neugebauer 1975 pp.590 (n.2), 733, & 1364 (Fig.43). Is a Ptolemy I survey’s memory embedded in Kleomedes 1.10’s famous legend? (Rawlins 2008Q [A4[a)). Disnom 1959 pp.230-251, cites 5 ordmags; 105°-disnom mag’s stade-long face-courses. (Sheldon fronts the 5 even more.) The only post-Ptolemy-I course (Athens, rebuilt 110) is also the 185m one. The ancient stade was 1/8 of a Roman mile (1480m): Engels 1985 p.308. Updated compendium of ancient Earth C-values, etc. Aristote 400000, Dikarechos? (0) 300000, Timocharis? (0) 216000?, Sostratos- Eratosthenes 250800, and Poseidonios-GD 180000. Correct circumference C = 216000. (Meter = $\sqrt{4/10}$10/10/10/10/10). The Enium mile = 1625m $\leq$ 756/100 = $\leq$ 1260 + 5/6 of the Roman mile.) Further speculation on the pharaonic stade’s history is found at www.dio.org/orl.htm#chg. Relating attested meridian-60th to 185m is another JHAD-uncited completely original DIO revelation.
Ptolemy as the quintessential or ultimate ancient scientist — knowingly46 rejecting the inconvenient fact that his genuinely empirical predecessor Hipparchos, though also motivated by astrology (at least in the period — 157 to — 145), published theory-discordant data, solar (fn 15), lunar (Alm 4.11), and stellar (fn 47). The attempt to aliibi Ptolemy by overlooking academe’s view of ancient astronomy to fit him has caused as much damage to modern scholarship (§§M1N & N) as Ptolemy visited upon ancient and (above, §A) medieval. M3

“Acutial” adds that ancient astronomers “were mathematicians who concerned themselves with proof, rigor, logic, and consistency rather” than with observational accuracy. Gingerich 1976 p.477 approvingly quotes Neugebauer 1975 p.108, “It makes no sense to praise Hipparchos for the accuracy of his numerical results. What is really admirable in ancient astronomy is its theoretical structure. . . .” See also Neugebauer on Aristarchos’ data as non-empirical (Neugebauer, op cit pp.642-643; Rawlins 2008R §A1). How then did Aristarchos and Archimedes (idem & Rawlins 2012T §E1) find the solar diameter to ordmag 1’ accuracy? How did ancient astronomers find the mean distance to the Moon within c.2% (59 Earth-radii: Alm 5.13)? Or find their observatories’ geographical latitudes to ordmag 1’? (See [4 Table 1] or Rawlins 1987T.

46 A month before publication, Rawlins informed Scientific American of the Hipparchian evidence (SM2) proving their article’s central argument was false. The reaction is provided here at fn 52. (Similar stubbornness at §§N.)

47 To back The Acquittal’s fantastic claim that ancient scientists weren’t accurately empirical, Evans 1987 reports that on 1981/7/16 he observed from Seattle the longitude of star α Sgr by measuring via cross-staff its angular distance from a lunar eclipse and found that his result was off by 2°3 of a degree, nearly triple the angle from lunar center to limb! This error is about equal to that of indoor-Ptolemy’s indicator 135/16 (a solar report (rightly deemed fraud by R.Newton 1977 p.190), but (reasonably more relevantly) also similar to outdoor-Hipparchos’ huge errors twice (Alm 3.1) when also comparing a star (Spica) to the Moon (during eclipses of 145 & — 134). Pointing to all 3 large misses in the eclipse-star observations (by himself & Hipparchos), Evans triumphantly concluded: “No better demonstration could be wished of 1” uncertainty in naked-eye observations, preaching that such Real-Science “might tempered ‘Ptolemy-skeptic’ judgements regarding the precision achievable,” or noteworthy-implicitly asking: So, Msrs. Newton & Rawlins — NOW what’s so criminal about Ptolemy’s one-degreerare erroneous observations?

Evans’ and Hipparchos’ three large outdoor errors clearly vindicated the Acquitters! And JHA experts who had for months vetting the above bio were later titled (Rawlins 1991W fn 288 and Rawlins 2009E §A & fn 7 applied the theory that Evans and Hipparchos had simply committed a sign-error in parallax when reducing each observation — a theory that decades later also proved just as neatly fruitful for Regulus’ hitherto-inexplicably huge ~35’ Hipparchan error (§3 §B6). Recomputation showed (ibid) that all four ordmag-1 errors15 of observational errors of Evans (Seattle 1981) and Hipparchos (Spica twice and Regulus) shrink to ordmag 1’. (i.e., all four errors were primarily those of reduction, not observation.) Warned of his sign error by Rawlins 1991W and in 1997 by Thurston &DR, while Evans 1998’s text was still unfinalized, its pp.257-258 ever-so-subtly Memory-Holed his 1981 no-better-correction data and switched to a different eclipse (no observed data recorded: from 1977 Spokane, an eclipse never mentioned [Rawlins 2009E fn 7 during Evans 1987], continuing, while sticking to only Hipparchos’ slips, the same Sermon-on-the-Muff (§3 §B4), just re-printing his 1987 argument (Evans 1998 pp.256-259) header “HIPPARCOS [sic] & PTOLEMY ON PRECESSION” & kept pretending Hipparchos’ Spica data showed Greek observational unreliability. [Irons. Evans has dehased himself FOR NOTHING: these random goofs can’t aliibi Ptolemy’s systematic fit-to-theory errors.] All while non-citing Hipparchos’ usual accuracy or Rawlins 1991W fn 288! No-better-correction-could-be-wished [1] of the JHA Assoc Ed [now Editor!]’s citational integrity (DIO 8 1998 p.2) & [2] of cultist disconnect between evidence & conclusion (above 6° |2°). The latter collapses to the latter stands entirely. Gondwana collapse also durably evident throughout the revealingly flip-flop history (Rawlins 1992V §§C31-C33) of the Ancient Star Catalog controversy. Today, Evans continues (Rawlins 2009E §A2; DIO 9.1 1999 p.2) decades of evading Thurston’s & Rawlins’ questions on the matter. (In 1997 June, DR asked Evans face-to-faces. Evans, DR then asked Evans for his office phone number so the two could confer. Evans refused. And JHA’s determination to shun permits Evans to face no consequences for such stealth. Other than 2013 appointment to JHA Editorship.) Like Gingerich (§A: fn 5), Evans cannot ever be shown wrong by non-culpable outsiders. [On the of-course-Disappeared “notes from that [eclipse] evening”: [4 §B6.]

The Greatest Faker of Antiquity

2018 D.Rawlins 33

ACCURACY, BEFORE PTOLEMY'S FATED STADES/DEGREE SCALE-SHIFT

I11) stretched the map East-West, inating longitude-differences?

In 1982, it was shown that Eratosthenes' original Earth-circumference C was neither of the long-accepted (variously rounded) stade-values, 25000 or 25200, but instead was 256000. In 2008, 26 later, 3 all were checked106 against Eusebius' long-neglected Eratosthenian Earth-radius r = 40800 stades, and 256000 was the only one of the 3 that fit this r. (How will Carman & Evans 2015's authors explain not mentioning this match, to I part in a thousand, when their own cited sources show they knew of 256000? See fn 10 above.)

I23 It was simultaneously found that Earth-radius r was the empirically primary datum, consistent with based on Sostratos' non-astronomical Pharos method (§22), which directly107 finds r from his Pharos Island lighthouse (in Alexandria harbor), built near Sostratos-Eratosthenes' time and place. Again, our deliberately silent (§§22&126) JHAD-center — which accepts no discovery if not from its own network — must regard the temporal&spatial coincidences as completely without significance. (Like unrecognized confluence in another sphere: www.dioi.org/shg.pdf, “Kit Marlowe's Perfect Crime” §(G3.)

I24 Proposed Sostratos ingenuous idea&achievement: mount Pharos' flame precisely h = 300 feet or half a stade above sealevel, so the apt equation, r = v^2/2h, becomes just r = v^2, thus Earth-radius r in stades can be found by just squaring the flame's coastal oversees visibility-distance v in stades. At first the trick seems suspiciously overseasy & dimensionally impossible. But it works. Note that squaring 202 stades, and rounding conventionally, yields Eusebius' r = 40800 stades (§22): a 3-to-1-unlikely chance-hit (Rawlins 2008Q §11; Rawlins 2018V).

I25 Taking the stade's length to be the generally-accepted value, 185 meters, Sostratos-Eratosthenes' r = 40800 stades is 19% or about 6/5 too high, while Poseidonios' C = 180000 stades, the other anciently standard Earth-size, is exactly 5/6 too low. It is an Occam-DIO dream-come-true to perceive that since horizontal light rays' curvature = 1/6 Earth's, atmospheric refraction would cause observed errors in C of 6/5 and 5/6, respectively, for two simple, clever, low-physical-labor never-leave-home methods108 of measuring the Earth: the Pharos-flame method (6/5) virtually attested by Pliny 2.65.164; and the also-physically-easy (& obvious!) double-sunset method (5/6). So DIO's refraction theory at once satisfies both Sostratos&Eratosthenes&Hipparchos' C (6/5 high), as well as

106 Rawlins 1985G p.265, taking an idea due to (ibid n.22) Gossellini 1790, suggests an ancient, adjusting for the 700 stades/1000 — 500 stades/100 switchover, stretched longitudes by 7/5, mis-assuming they were based on land-surveys (1 §5). Or by 4/3 via Poseidonios' C = 240000 stades—180000 stades. Proposing Greeks organized to compare eclipse observations (fn 13): Rawlins loc cit, vs fn 90 above & with that, the Cape Verde islands, westernmost known land, chosen as Marinus' 0° longitude (www.dioi.org/sof1/pdf, §A5), to kill longitude sign-muffs like those (fn 97) cursing Scheglov 2016. [Speculation 2018/7/15: Marinus = pseudonym, like “Ptolemy”? or Map-tote from maritime Tyre?]

107 C = 256000 stades 1° induced from a Nile map's latitude intervals: Rawlins 1982G pp.212, 214, 216-217; Rawlins 1985G p.259; Thurston 2002S p.66. For which C fits Eusebius' r = 40800 stades: just multiply r by 2π; 1 Rawlins 2008Q eqs.8-11&18, esp. eq. 11’s solar distance = 100.1 AU. Fn 10’s 102 AU is over-exact (fn 42), even while fitting 252000 less well than 100 AU fits 256000 fits.

108 Rawlins 2008Q eqs.2&21&28; eqs.14-15&17-18 for r as Sostratos' direct empirical measure. See also ibid §A4 [a] for the Pharos-flame method and Philet's semi-attestation of it. Double-sunset method: Rawlins 1979 or Scientific American 1979 May. Interval between times of sunsets seen from Pharos' top(bottom exceeded a minute-time, unmissably-enormous alert and gauge of C's size. (Elementary illustration-by-extremes that different results ensue for flame vs sunsets: Rawlins 1992Y §A5.) To insert needing direct attestation: among the many robustshunned monetary clues here, the foregoing utterly original&successful atmospheric-refraction theory — tri-nearly solving the INTERMINABLY-contradicted ancient Earthsize mystery — cannot legitimately be ignored. But it is: fn 111 below. Unbelievably worse: fn 109! One recalls not only JHAD shunning of Diller (fn 25), but the case of L.Boltzmann's kinetic theory of gases, which E.Mach & others spurned because (though theory neatly fit evidence) no one had ever seen a molecule. Did this trigger Boltzmann's 1896 suicide (just ere vindication by science)? We don't know. What we know is: certain pets cited hereabouts wouldn't care. Past perhaps praying for history to repeat. Ever so vainly.

N APPENDIX 4: UNERRING ATTRACTION TO THE ERRING DOUBLESECRET EMPIRICISM & LURKING BLEAKHOLES

Those archons who for generations have controlled journals, conferences, and funding in the history-of-ancient-astronomy field address to themselves the god-like prerogative to classify — with that infallible judiciousness so amply sampled hereabouts — and exclusively publish, those who are equally reliable, trustworthy experts, as distinguished from those crazies who should be blackballed, exiled, unpublished, uncited. However, our question to deities isn't: have they ever had the fairest idea of how an ancient astronomer actually worked, observed, reasoned, and achieved? (Or: have they ever sufficiently acquired a scientist's attitude, for appropriate empathy with the scientists of yore?) No, the germane question is: how have so many of the field's leaders so often concluded—for and tyrannically insisted-upon the very opposite of the truth — and on the most central issues — virtually across the board. A gang of moguls with such a degree and breadth of misperception of their own field's realities may be unprecedented. If they are deliberately trying to acquire a reputation as the sore-dumb retards and fanatics of the history-of-science discipline, their plans could hardly be improved upon. (Which actually would be quite unimpressive in that such men as Neugebauer, Aaboe, Britton, and others are brilliant despite their blindspots.) Think this too strong? Well, if you have the independence to actually read what follows in this section, you'll be taking evidence before making up your mind. (We trust this doesn't break a cult principle.) The specific delusions that have long been orthodoxy in this unreal field:
Assuming that Ptolemy got his astronomical elements from his "observations" — the central JHAD-inversion of this controversy. Contra: [1] A particularly penetrating yet simple demonstration of the truth is due to Thurston 1994P (§D), who noticed back in the 1940s that Ptolemy's iterative proofs of planetary orbital eccentricities start with highly precise estimates, but by the final iteration they're round as can be. Real iterations proceed in the reverse direction. [2] Another instance of JHAD inverse-perception of ancestry-direction is shown below at §N15 item [F]. [3] Mars' Alm mean synodic motion is accurate to ordmag 1°/century, yet the "observations" on which Ptolemy's proof of it is purportedly based are off by ordmag 1° (Rawlins 1987 p.237). [4] The Alm proof of Mercury's mean synodic motion was already shown above (§F2) to be pretense, by one who had started with the answer and (if the Canonical Inscription's elements were also based on alleged observations then be) TWICE — differently — fabricated the "observations" to prove it. [5] The centrally phony aspect of the whole Alm is seldom understood (PHuber the happy exception — Rawlins 1991W fn 224), namely, its near-exclusive use of n observations to solve geometrically for n orbital elements. From the often excellent results sampled here throughout, we realize that ancients' actual historical evolution towards accurate elements obviously involved repeated adjustments from numerous observations. As a mathematician not a scientist, Ptolemy never sensed the value of overdetermination — where the number of equations of condition exceed the number of unknowns sought.

Deeming (Rawlins 2008R §A) the infamous faker Ptolemy an observing astronomer, while antiquity's ultimate fabricator was the immortal empiricist Aristarchos whose universe was rightly at least trillions of times larger than Ptolemy's geocentric misconception.

Even claiming (Rawlins 2008R fn 10) that indoor Ptolemy (errors ordmag 1°) was a better outdoor observer than Hipparchos, whose errors were actually an ordmag smaller.

Accepting that Greek astronomers were not primarily empirical ([§M; DIO 1.1 ff fn 24; Rawlins 2008R §A1 & fn 20). Among objections beyond the a priori: far too many extremely accurate ancient measures survive ([§M3&N1, fn 8 [3]].

Because of mis-signing of parallax-correction (fn 47), teaching in an Oxford University Press textbook that errors of ordmag a degree (exceeding the lunar diameter), as repeatedly found by Newton in Ptolemy's alleged observations, were ho-hum-normal for ancient instruments, as Neugebauerians believe (e.g. [§M3; also: memorable Aaboe conversation, 1976/39]). (Note: Oxford. U: Press was warned by 1997/117 letter of the book’s problems, but [see similarly at fn 46] went to press with all errors intact.)

Resistance to realization that celestial mean motions, lunar (§§N16-N17) and planetary (and even some solar), were based on integral (or half-integral) parallaxes, not by dividing a long angular arc by its corresponding time-interval, as Ptolemy pretends (e.g., §F2).

See DIO's General Theory of Ancients' Cyclicties: Rawlins 2002B §H.

When a ball is tossed upward at 0° and caught downward at the same height 4° later, most of us know it maxed at 2°. Yet, from his own astonishing failure (R.Newton 1977 fn 20) to understand this junior-high maximum-height problem, MacArthur-Genius Sewardlow keeps asserting (with Evans' and Jones' evident assent: fn 11) that solstices could not be determined accurately, and likewise (fn 8) that outdoor maximum Venus elongations must've been so crude that Ptolemy was forced to compute them himself. His argument (perhaps unique in all history-of-science scholarship): the time of maximum cannot be well determined because, near maximum, the object is hardly moving. This sort of spectacular embarrassment is what happens when a history-of-astronomy crusade depends on those with inadequate gifts in positional astronomy, spatial relations, and common sense. (Assuming Sewardlow is not knowingly ladling nonsense to vulnerable archons too predisposed and sub­genuinely to recognize the prank.) Due to just such JHA-published pseudo-science, Ptolemy & Co. even perversely teach — complete with now-ironically Pompous sneers (quoted, R.Newton 1997 loc cit) at sub-JHA untouchables — that ancient equinoxes were more accurate (fn 11) than solstices, from their own unfamiliarity (e.g., JHA Editor-to-be Evans fn 11), with [a] the instrumental and astronomical problems involved (R.Newton 1977 pp.81-82; and

<table>
<thead>
<tr>
<th>Obsvr</th>
<th>$E \pm \sigma_E$</th>
<th>Adop $L$</th>
<th>Its Error $x$</th>
<th>Actual $L \pm \sigma_L$</th>
<th>$x_0$</th>
<th>$\sigma$</th>
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<tbody>
<tr>
<td>Timoch</td>
<td>-294±11°</td>
<td>31° 12'</td>
<td>-1° 8±2'.7</td>
<td>31°13'8±2'.7</td>
<td>±9.0</td>
<td>±8.8</td>
</tr>
<tr>
<td>Aristyll</td>
<td>-258±10°</td>
<td>31°15'</td>
<td>+1°0±2'.1</td>
<td>31°14'0±2'.7</td>
<td>±6.1</td>
<td>±4.2</td>
</tr>
<tr>
<td>Hipp</td>
<td>-131±05°</td>
<td>36°08'</td>
<td>+0°2±1'.2</td>
<td>36°07'8±1'.2</td>
<td>±5.2</td>
<td>±5.0</td>
</tr>
<tr>
<td>Anon</td>
<td>+159±09°</td>
<td>31°15'</td>
<td>+4°4±2'.0</td>
<td>31°10'6±2'.0</td>
<td>±6.0</td>
<td>±5.6</td>
</tr>
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</table>
In 1982, Rawlins sought the epochs $E$, as well as the errors $x$ in the observers’ assigned latitudes $L$, and their standard deviations, for all 4 known ancient stellar-declination observers — Timocharis, Aristyllos, Hipparchos, Ptolemy’s anonymous observer — through bivariate least-squares testing upon these observers’ star-declination data.\(^{100}\) In 1994, ex-

\(^{100}\) Rawlins 1994L §5.3-F8, where nulls show Timocharis (known observations c. 200–297), earliest of Alexandria’s 3 observers of extant star declinations by surveying instrument, alone knew his exact $L$. Later authors (see $E$ for each individual observer, Mayrhofer 2015 [b]), chiefly for the $DIO$’s bivariate least-squares ($Rawlins 1994L$ Table 3 results), he independently finds observers’ epochs $E$ and latitude-errors $x$ by double-monovariate, noncalculus trial-and-error, and graphs — gauging $E$’s standard-deviation by eyeballing, and $x$’s by confusing it with that for a single datum. More recently, in the mathematically-illustrated Journal of Astronomical History & Heritage [JAHH], Brandt et al 2014B, www.dioi.org/bzj.pdf, says, most of Rawlins’ geographical latitudes $L$ are close to our values’. But their $L$ are merely assumed, so $DIO$’s JAHH-requested 2014/8/26 referee report, www.dioi.org/jah8q.pdf, asked that the paper notice $DIO$’s entirely original 1994 nulls-method of non-assumptively determining $L$ (Rawlins 1994L loc cit), a discovery neither available nor even cited anywhere else — a quarter-century later. A few referee-recommended corrections were fortunately applied (www.dioi.org/bzj0.pdf, p.5 to final www.dioi.org/bzj.pdf, Brandt et al 2014B p.331). But the request for showing how $L$ finally could be found (not guessed) was, among others, not met by $JAHH$. See §4.116 below for the weird details. Also and more importantly unmet was the $DIO$ referee’s urging the Brandt et al 2014B paper to take note that Ptolemy arithmetically reduced (Almajej 5.12-13) his transit “observations” to declinations using a seriously false Alexandria latitude $L$, from Vitruvius 9.7.1 (plagiarism unmentioned at Swerdlov 2010 p.151), $L = 30°58’$ (arcctan 3.5; above, in fn 25), erroneous by $-14’$, which obviously is much-too-large for a regular outdoor observer, and also is in 17° conflict with latitude $L = 31°15’$ which is indicated (by nulls) to be the virtually-correct value that was adopted by the stars’ actual observer. Brandt et al 2014B’s standard deviations $\sigma$ were allegedly calculated bivariately; but, actually, after each $E$ was found monovariantly, $L$’s “accuracy” was found by averaging the remaining residuals, original but invalid procedure, making $L$’s uncertainty too small by an ordmag, as referee-specified. NB: Had Brandt et al 2014B found the reverse, finding $E$’s $\sigma$ first, and then their $L$’s, they would have gotten the other variable’s ($E$’s) “accuracy” similarly, the result would have been informationless zero! (See §4.111.) There are further oddities. Ibid’s eq.1 sets $O = C$ equal to $C$. The paper’s $O - C$ graphs are rendered $C-O$. Two strange $JAHH$ attempts to undercut skepticism by splitting Ptolemy’s 18 stars differently from R.Newton (and, by-the-way: Ptolemy and Pannekoek and DR — with $E$’s) arbitrarily omitting those low-star declinations-specifically high-declination ones, which need elimination in order to finally satisfy “editors” too scared (or lazy) to edit, §4 fn 13.) The intent of

Rawlins 2018U §F1, whose eq.10 quantifies for the 1st time the ordmag $4 \text{\sigma}$ effect of deviation from quadracity: [$N$19 below], not to mention [$b$] ancients’ historically uniform choice of solstices not equinoxes for yearlength-determination. Have those who’ve been disbelieving Greeks’ ability to measure solstices accurately (Swerdlov, Evans, Duke) noticed that the newly available papyrus $PFouad$ 267A ($[$N$99] has tried to enlighten them by directly surprise-testifying to an ancient solstice which was accurate to ordmag $1^{+}$? Just-luck? (Like another trio of just-lucks at $ident$? Meanwhile, note that Duke not only wrongly doubts that Greek observations were sufficiently accurate for trustworthy solstices [$iden$], but agreeably if mistakenly proposes that Hipparchos’ $-134$ solstice was $5°$ off, when in truth it’s error was only $1°$ (see also the $DIO$’s Rawlins 1991H in fn 4; Rawlins 2018U eq. 10 & Table 3.) We can test the point: the mean systematic error of Hipparchos’ equinoxes was $7°$ (consistent calculational conclusion of Britton, Newton, and Rawlins: summarized at ibid §B4), while in spite of $6°$ rounding, the errors in recoverable ancient solstices (one in Kallippos; one by Aristarchos, two by Hipparchos: ibid Table 3 & eqs 1.2 & 7.24) are $+3°, 0°, +2°$, & $-1°$, resp, indicating that Hipparchos’ rms solstitial systematic error ($1°$) was more than four times smaller than his mean equinoctial systematic error. Unrecorded ancient equinoxes doubtless had less scatter than solstices, but the latter obviously suffered smaller systematic problems (remember, too, that the ancients [needlessly] worried about [and corrected-for; $\pm 9°$] the effect of several arccmin of solar parallaxes, which would degrade equinoxes not solstices), precisely one of the cult-unperceived reasons why — when ancients sought reliable cardinal points for gauging yearlength — they chose solstices.\(^{38}\)

\(^{38}\) It will be convenient to here list in one place prominent instances, of the pile-on passion of the most elite historians-of-science, for trashing ancient empirical science: Neugebauer 1975 pp.108, 284, 642-643. Gingerich 1976 p.477. ScAm 1979. Van Helden 1980. Evans 1994 p.274. Evans 1997 p.87. Evans 1992 p.32. Duke 2008W p.287. Shcheglov 2016 pp.687x693. (Further discussion at §3M & §3N fn 8.) Another interlude: Ptolemy’s $Tetr$ 1.1 astrology-promo suggests he’s fighting more resistance than historians-of-science know, “of a general nature their causes from the enveloping heavens. But . . . everything that is hard to attain is easily asailed by the generality of men . . . there are specious” criticisms of astrology, but doubts of astronomy “could be made only by the blind”. JHAD’s best anti-fraud defense of its hero might be to plead him “Illegally Blind” $\exists$. See also §E4; & §E6 on his night-shy eyebrows.

We recall how “Mr. History of Astronomy” (AAS-HAD Newsletter #51 Page One) dream-creates his idea of ancient realities. Gingerich 1976 p.477 on a temple-bound fakery-dreammatematician geocentrict-astrologer out in kookburg Canopus ($[\perp ]U$), who was just as skills-inevitable by his real-science world ($[\perp ]E3$) as the JHAD cult is by its: “we can easily imagine Ptolemy surrounded by assistants and graduate students at the famed Alexandrian library.” But on the most original genuine ancient scientist’s coherent heliocentrism: trivial by $\exists$EAA criteria ($\exists$H [b]), just a passing “splendid speculation tossed out during a vigorous discussion between the Alexandrian mathematicians” (www.dioi.org/st56.html).

\(^{48}\) Irony [due to yearlength-estimators’ dependence on truncated (Rawlins 2018U §2 [$C1$]) prior solstices]: despite 4 known reliable solstices (Rawlins 2018U Table 3), no known ancient got an accurate yearlength. In 1977, Brigham Young Univ astronomer H.Kimball Hansen conceived a simple method ancients could’ve used: $[1]$ Find a stable stone point on a hill which near an equinox casts a North-South shadow at apparent noon on a stable stone surface below, both stone locations being more secularly-immobile than human equipment. $[2]$ On some date around an equinox (no need to be just at one, merely when solar declination-motion is near-maximal, mark where the noon shadow is. $[3]$ Note when it returns there 20 years later, and divide the interval by 20. The result, in just $20°$, will be times more more accurate than any yearlength known to have been adopted in antiquity (even though these were based on intervals an ordmag longer) all of which were seriously erroneous, for reasons (analysed at ibid §3C-DQ) which do not apply to the remote Babylonian data which ultimatelymade possible the hyper-accurate Greek lunar periods of Rawlins 2017E §B2-B4.
Historians-of-science unexpectedly ignore the perfectly Occamite 3-for-39 hit-succes of the spare atmospheric-refraction theory that explains and fits both ancient Earth-

Only 2 ancient Earth-circumference C values were widely adopted: Eratosthenes' (really Sostratos'-Eratosthenes') 180' of stadia or Poseidonios' 180' of stades (in lands where either was adopted each in succession, though they exhibit a previously unexplained gross disparity, the former being over 40% larger than the latter. DIO's revolutionary simultaneous solution of BOTH these C values from the same simple theory (math & sources at ibid eq.28) realizes that each Greek C differs from actual C (216,000 stades) by almost exactly a factor of 6/5 (within c.1% in each case), Eratosthenes' high by 6/5, Poseidonios' low by 5/6. Hmmm. It happens that there are 2 very obvious stay-at-home Earth-measure methods (one even semi-attested at Pline 6.15.164): [1] Pharo-flame-visibility & [2] double-sunset (Rawlins 2008Q [A4]. Resolution arises since atmospheric refraction causes horizontal light rays' curvature to be 1/6 Earth's, thus the lighthouse-flame method's result is expanded by factor 6/5, while the double-sunset method's result is contracted by factor 5/6. However, to see this, one must be able to follow the mathematical physics. There is as yet no evidence that any historians-of-science have ever done so, doing the 3 of a century (summary and citations going back to 1982 provided at Rawlins 1996C fn 47) since DIO published this hyper-neat triple solution. Are navigators' familiar formula for the horizon's "dip" has been based upon identical 6/5-mathematics for over a century?!) These 2 DIO matches exceptionally are achieved without judging the stade at all, but just by adopting the standard 185 meter value. [Thus serving as the final, controversy-ending proof that 185m was indeed the true length of high antiquity's stade.] Our achievement here may be compared to the controversy's ever-wheel-spinning metronological-solution literature: snaffling/edging through ancient lore in search of hits of oddball stades, which of course existed all over the place before the Ptolemies presumably regularized the measure by defining their empire's royal stade at 185 meters, commonly miscalled "Attic", which we may instead (below) dub the "sexagesimal" stade. Unique in the centuries-long history of the debate, DIO's solution is physical not metrological. Again (§110), modern archaeologists--this resolution matches all 3 targeted data within about 1% of the known 'true' Eratosthenes' [b] Poseidonios' C, & [c] the 185 meter stade that is now (Rawlins 2008Q [J1]) accepted by virtually all serious historians. By contrast, ALL the usual solutions for C (invariably just metrological rehashes), that keep filling journals' pages, can only match 1 out of the 3 (and even that match is usually several times looser than 1%). Notice the astronomer-deflating surprise that the Pharo solution of the famous Sostratos-Eratosthenes C is geographical, not astronomical. [Speculation follows.] But refutation is effected when we realize that the unconvincing (but low-refraction) presumably-Kleomedean-astronomical survey-based 185 meter stade implies C = 216000 stades (since the product equals actual C = 40 million meters) but 216000 is the cube of 60, hinting that (before Sostratos cleverly but wrongly found for C = 256000 stades, c.270 BC) scientific surveyors had obtained an accurate C to which Greek science had naturally applied standard Greek sexagesimal division to the Earth's meridians (the process' step 1 is even attested: §3 fn 111) to define the stade so that C = 60' or 216000 stades. I.e., sexagesimalization (triple division by 60) of Earth-C 40 million meters produces a geodetically correct "sexagesimal" stade of 185 meters, fine for 600 stades/degree, but not for Sostratos-Eratosthenes' later-famous (but seriously-too-high) 700 stades (of 185m each) per degree. But: when did this hypothetical achievement occur? Traditional games at the Olympic stadium began before 300 BC, but when were the stade-long-footrace markers now found there (Engels 1985 p.298) established? (If athletes ran in various states' races, there must have been gradual standardization, but when?) Timocharis' accurate theodolite-based stellar declinations (Rawlins 1996C fn 250-251 presents for 5 ancient stadia the disparate lengths of the traditional 1-stade footrace, where the sole 185m one is also the only one (Athens, reconstructed +143: Gibbon Decline . . . Chap.2 [Mod.Libred 1:41]) that's post-Ptolemy I. In the Hellenistic world, where else than Egypt would terrain allow a long flat North-South arc, of ordnance 1000 km. Given that the Alexandria-Meroe arc's curvature is apt to a meridian circle of C = 39870000 m, a Strabo-attested 10000 stades interval (at 700 st/degree) transforms into (1 part in 10) that the cities' recent past 14th c. AD astron. is capable reliably to indicate Earth-C so accurately that hypothetically surveyors' 185m conclusion was trustworthy to within ± 1 m. Was Kleomedes' famous Alexandria-Asswan legend a myth (DR's former opinion), or a remnant of Ptolemy's post-conquest land-survey of his empire? (With similar possessiveness, less scientific William the Conqueror reckoned his own new booty in the Domeday Book.) Thus theodolite methods were the most likely practical technique; positioning theodolite-tr日后 constructions (transit instrument) and then constructing accurate divisions (consistent with the superior technology that effected the conquest of Babylon, which lacked such advances as theodolites and trigonometry tables), given Timocharis' accurate theodolite-based stellar declinations c. 300 (Almagest 7.3; Rawlins 1994L). Was Timocharis chief of the project? Hitherto un-noted credit: Timocharis knew Alexander's 31°12' latitude precisely (ibid [F6]), while neither of the other two later star-observing Alexandrian astronomers quite did so (ibid [F8F9]). From solar observations (e.g.,
be good to ordnag 1°, which invites equinoxs-vs-solstice accuracy-comparison (§7).

16 Rawlin 1991H found the Babylonian yearlength on famous Astronomical Cuneiform Text 210 was (§F2) based on well-known Greek solstices (=431/627 & =134/626), the 1° datable transmission of an orbit-element between Babylonian & Greek astronomy, going Greece→Babylon, thereby getting the Muffia’s holiest tenet (§37&f.120).

17 Aristarchos&Archimedes put minimum solar&stellar distances at 10000 & 10000 AU, resp, because humans can see (as no historian-of-science seems aware) to c.1/10000 rad. Rawlin 2017E §K2: “For advancing history of science, knowing science matters.”

18 Ptolemy’s allegedly-outdoor 4 solar “observations” (132-140 AD: Almagest 3.17) averaged over 1° error (not even 1 shot encroached anywhere on the solar disk) and were fifty times nearer Hipparchos’ 3-century-old indoor tables than to outdoor reality (as known for 1900 BC). Fr.Ragop 2010 p.121: undeniable but also (for over 30%) JHA-unprintable. Despite null ancient attestation of their speculation, Ptolemaists insist (fn &127) this is because ancient scientists kept only observations agreeing with theory. (I.e., they cheated.)

A. How could Ptolemy’s solar “data”, all off by a degree happen in the 1st place, outdoors?

B. How did it happen that Hipparchos’ Sun observations’ average disagreement with reality were merely two or three times their disagreement with his tables, vs Ptolemy’s fifty?

C. How could Greeks just copying predecessors achieve their many accurate discoveries? (E.g., Greek vs real ancient monthlengths, Rawlin 2017E §§52-B4: startling proximity.) However, most scholars (A.Pannekoek, R.Newton, Y.Maeyama, J.Brandt, P.Frizmer, among the welcome exceptions) reject ancient high scientific accuracy (fn &35 here), undeniable 2016 subscriber D.Chesnoff even (fn 13 here) calling it a “delusion.”

19 Aristarchos discovered precession 150 before Hipparchos: §G3.

20 length-of-month estimates, of too-unbelievably quick Greek accuracy (11 §F): 1º for lunar-solar diameter; 1º for month; 1º for solstices; 1º for equinox taken on Alexandria’s ring (fn 70 here: Almagest 3.1); 10º for sidereal year (fn 144 here); 1º for observer latitude (see fn 39&100 here and §4’s Table 1 & fn 38); 1º for 500-mile North-South arcs on Earth (fn 111); 0.1º for star declinations (fn 100); 0.ºº for lunar-limb-vs-Sun gap (fn 12 here); 0.ºº, even 1º for star-vs-eclipsed-Moon gap (§B7); ocular error 1º±1º (fn 97 here) for Hipparchos’ mean equinox, with 2º scatter (fn 12); 1º for Earth-circumference-measure precision (fn 108); 10ºº for lunar eclipse-prediction (fn 97 here); 1º for lunar eclipse observation (Rawlin 1985G pp.258&265); 1º-century for mean motion of Mars (and maybe Venus); 1º for all three adopted months (synchronous, damn near perfect, eclipse), each correct to better than one part in a million.

21 Historians-of-science like Schegolev show no sign of awareness of any of these Greek achievements.

out of the ordinary, for instance, Britton 1967 p.29. More generally, §8’s key Obvious Question C joksifies fn 8’s Swerdlov-Gingerich-Scientific-American’s Ptolemy-exculpation-theory.

97 On 1982/7/17, ever-Ptolemy-worshipping JHA, unable to argue facts or math, nonetheless belatedly excised Ptolemy’s sensational 50-to-1 indoor-vs-outdoor ratio (§8) from a projected Rawlin paper fn 74 here; precensored at Rawlin 1999 (E), along with nonelector outdoor-observer Hipparchos’ parallel ratio of just 2-to-1 or 3-to-1. (Even that tiny ratio reflects just that his tables were founded upon his own slightly imperfect solar data. His UH tables’ eclipse-prediction accuracy was ordnag 10ºº: Rawlin 1991H eq.32.) The 1º mean error of Hipparchan equinoxes is mostly not due to eyeball inaccuracy: when one removes the effects of his presumed solar parallax and (like fn 70 above) the errors from refraction in the Sun’s zenith distance, and his instrument’s setting-tilt from polestar-refraction, there’s only 1º (±1º) unaccounted-for: see Rawlin 2018U §B4. Mars (and Venus?)’s accuracy c.1º–2º: Uncertainty ranges from 1º (for latitude) to the umpteenth fruitless metrological analysis of Erathosthenes’ Earth-circumference, Schegolev 2016, www.dioi.org/shc.pdf, which massively contends that high-accuracy Greek longitudes are mythic, his entire assault upfront-promoted by History of science Society (and post-protected by HisS stonewall-noncorrection), ultimately undone by his cruelly confusing a solar eclipse with linear and falsely putting Spain into the eastern hemisphere (and China’s Xi’an & Luoyang into the western). Yet another history-of-science journal’s all-too-common Pb-paper duffer-science: details of these latest Isis disasters can be found here at §D, DIO’s Letter-to-Isis’ hiding-since-recipient Editor H.F.Cohen.
astronomers did their high-precision celestial work (Alm 7.3) by recording angles in the old pedagogical tradition of clumsy fractions of right angles, etc., a position recently undercut by the high-school-level discovery of the previously-unperceived fact that Archimedes’ solar diameter was measured and bracketed in degree-fractions (Rawlins 2018U fn 4)

N13 Selling crude priestly Babylonian indoor astrology as “impressively accurate” (Jones 1991H p.118), and so brilliant it inspired Greek astronomy, according to Neugebauer [1975 p.622 believes in the “advanced state of astronomical techniques” in Babylon] and others of his persuasion (Rawlins 1991W fn 73; Rawlins 1996C fn 128) — even though the sole empirical datum traced in either temporal direction is Greek—Babylonian (Dicks 1994 fn 37). The greater antiquity of raw records from Babylon may say no more than that clay outlasts papyrus. Unlike for Greek astronomy, no record exists of how Babylon arrived at its naked celestial tables (Dicks op cit [C4]; DIO 13.1.2 [H]); clue: virtually all useful astronomical texts from Babylon post-date its conquest by Greece, and its periodic functions are not trigonometric ones (Greek astronomers had trigonometry from no later than c.130 BC) but approximations thereto, by zigzag or even step functions, suggesting (fn 120 below), though not necessarily proving, mere derivative degeneration from its conquerors’ superior technology. (See also Rawlins 2018U §4.) An obvious & devastating point, never previously emphasized for its implications: not a single trapezoid table survives in any form from Seleukid-era Babylon. Babylon made no solstice or equinox observations (Neugebauer 1975 p.366), or meridian or vertical observations of any kind, because (Rawlins 1991W §E3) Babylon had no transit instruments (vs Greek celestial transit data from c.300 BC onward, at Alexandria and Marseilles), and accordingly didn’t know or care what the city’s geographical latitude L was. Which may explain why the only attested figure for it is 43° [a] is nowhere in Greek records, not a word on any ancient text not material, another crushing blow to Babylonianist pretensions, and [b] is too far north by 2°28’ — 148 nmi. Finally, while Greek planetary order was physical — Mer-Ven-Mar-Jup-Sat — Babylon’s was astrological, Beneficent — Maleficient: Jun-Ven-Mer-Sat-Mar.

N14 Failure to learn anything from the first of DIO’s eclipse-cycle solutions (§3 §33), in which equating 9660 synodic months with 781 sidereal years (the interval between two attested local-midnight lunar eclipse records, Babylon — 719/3/8-9 & Alexandria [Heron] 62/3/13-14) neatly recovers all 10 digits of Ptolemy’s previously mysterious final lunisolar equation (Rawlins 1996C eqs.21-31): 8523 tropical years = 10546 sidereal months. (Had JHADists not shunned this remarkable match, they might well have anticipated the faster discoveries of §16, long before DIO. Similarly at Rawlins 2009F fn 7.) This is also the 1st irrefutable evidence for ancient use of sidereal—tropical transformation of period-relations (Rawlins 1996C eqs.26-27), a process later extensively employed in DIO 11.2, which allows (here, in fn 8 [I]) reconstruction of Venus’ accurate pre-blunder synodic motion.

N15 Among the most egregious of all inversions of ancient astronomical procedure: our uniformly on-the-nose huge-cycle-solutions of no less than 4 long-mysterious periodic lunar mysteries (§§N14 and §§N16-N17) fall upon locked-up minds, disbelieved without a glance by jeering JHADists who (frustrated by inability to find error in heresy’s math but determined [§B3] to reject the obviously probable in favor of the preconception-accordant improbable) resort to whatever dodge will serve. Besides jeerleader Jones’ private carelessly unchecked & glaringly one-sided misfire (§N16), there’s a long-popular theory (item [E]: put into writing privately by P.Huber, likely inspired by Neugebauer [also primarily a mathematician, like Ptolemy]) that is so inverse-contrary to sensible scientific practice as to gain special popularity among gaping cult-minds, thirsty for any refutation of undeserving outsiders’ proposals, thus not just refusing but insisting-upon JHADists’ pure speculation that every long-period relation must have originated from splicing together a few much shorter relations, & no other hypotheses need apply. (The truth was 180°—opposite from this rigid position, of course: ancient’s more-convenient short relations descended from less-handy ultra-long empirical ones [DIO 11.2, for reasons about to be explored, below.] Which demonstrates yet again our JHAD’s unerring attraction to the erring. Considerations:

replace ([C6] elsewhere in the same paper, a paper which qualifies as a pinnacle of JHAD-subtractivity, simultaneously managing to deny Greek accuracy on all available fronts — obliquity & klimata & calculations & trig-tables & curlici (& Diller & DIO) — thereby obediently according with predecessors’ orthodoxy (just-as-reliably as did just-as-careerist Ptolemy’s “observations”: fn 8), ensuring Jones’ rapid elevation to JHA boardship. H5

I Unmet Challenges — Advances in Understanding Ancient Science Endangered by Hate: JHAD Brains Glummed Shunning for Cover

I Though numerous scholars have doubted 3rd century BC Greek scientists’ adoption of Babylon’s degree/division of the circle, it was unexpectedly revealed in 2012 that Archimedes’ original unprocessed measure of the Sun expressed its diameter sexesimally as in the range between 27° and 33°, later conventionally published in his Sandreckoner as rightangle/200 and rightangle/164. Hard to believe (given the ultra-simplicity of the math): no one has noticed this for the last 2000.

I For scholars have accepted On the Sizes and Distances of the Sun & Moon as by Aristarchos of Samos, namely missing the significance of Archimedes’ accurate contradiction — by a factor of four — of Sizes’ farcical foundational empirical data (2° solar&lunar diameters!), & in spite of its ludicrously contra-reality requirements that: [a] total lunar eclipses last 12h (Neugebauer 1975 p.642), & [b] for Mediterranean observers, the Moon (at distance c.20 Earth-radii) visibly moves in-REVERSE among the fixed stars every day around culmination. Item [b] earns a truly special place in the Bizzartty-Hall-of-Inname, by the astonishing fact that, again: no one has noticed this for the last 2000.

I In the 27th since publication (Rawlins 1991P fn 6), no modern scholar has yet cited DIO’s discovery of the elementary cause of §2’s 4-factor-overestimate: pseudo-Aristarchos misinterpreted Aristarchos’ lunisolar division of 1/15th of a “part” or μερος of the zodiac as: 1/15th of a zodiacal sign. But μερος (meros) was just an ancient unit = circle/48 = 7°1/2, of which 1/15th exactly agrees with Archimedes’ correct half-degree testimony.

I Aristotle’s famous 87° half-Moon elongation wasn’t empirically a precise figure, as moderns (and seemingly Hipparchos: §D2 above) assume, but a lower-limit. From tiny solar declination-motion near solstices, unformically sneering amateur-astronomer-Ptolemists (incl. MacGenius Swerdlow: ²² fn 8) doubt ancient solstices could

95 Sizes’ 5 farces & Aristarchos’ 1/2-Moon-elongation 87° lower limit: www.dioi.org/jieo2.pdf, Rawlins 2008R §§C1-C3 & fn 17&29. [By proportions, Aristarchos-Archimedes’ shift (Rawlins 2008R fn 37 vs eq 15) from Sun-distance 1000’to 1000’ altered limit to 89’t. (nearer actual 89.5’.)]
96 For “part” (μερος or meros) = circle/48; see Neugebauer 1975 pp.552&671, or Rawlins 1991P fn 6. Those wondering whether Swerdlow&Gingerich are primarily scientists or careerists, cannot miss the amusingly indiscriminate alibis for Ptolemy’s fakes, at www.dioi.org/jk02.pdf, Rawlins 2018U §§B2&B3 and fn 2&3. (And Duke at fn 69 above, vs Rawlins op cit eqs.5&21 Table 3, §E-J. Alleged solsticial-inferiority: fn 27 here, Evans 1998 p.206, & Swerdlow 1979 [Phi Beta Kappa] p.527. Noel Swerdlow, though occasional valued discoverer (e.g., fn 42), is a prime contributor to DIO’s literary scientific life. [H.] www.dioi.org/#!bib.htm. [Www.dioi.org/jhb.htm. (see §J on Swerdlow’s & Evans’ innocence of equal-altitudes, the obvious ancient solsticial-findings), there are unambiguous historical points they’re equally (and, as purported historians, less excusably) unaware of (unlike non-politicians such as Toomer 1984 p.12 & Britton: all known ancient scientists found yearlengths via solstices not equinoxes: Meton, Euktemon, Callippus, Dionysios, Aristarchos, Hipparchos, BM55555. And these doublers of ancient’s solstice-accuracy (plus fn 27: Jones) are now confronted with recently recovered papyrus P.Fodi 267A, testifying to a Hipparchos solstice accurate to ordm 1° (4 fn 20 [C]). Preferring solstices over equinoxes for year-length-determination becomes

re

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2
Pliny's Circuli: Deft Ancient-Trig Approximation-Inventiveness

Now for a case of ancient data-tampering that actually happened — the very sort which modern-chauvinist tamperers have, with dedicated persistence & Creativity, tried misprojecting\(^89\) onto valid data (above §3C-G) that commit the crime of being inconvenient to prevailing Muffia preconception.

Neugebauer classified the seven "circuli" of Pliny 6.39.211-218 as a primitive "arithmetical\(^90\)" scheme, deeming their superficial inaccuracy to be supportive of his contra-reality (fn 8 above) insistence on "the absence of any scientific organization in antiquity," a conviction which he thought helped (along with his reasoning as covered in fn 1 here) to exonerate accuracy-challenged Ptolemy. But the 1\(^{st}\) — and still only available — coherent solution (fn 91 below) ever devised for the circuli indicated they are no more arithmetical than Hipparchus-Strabo’s klímata (which Neugebauer deprecated similarly: fn 18 above), proposing that Pliny’s circuli are instead trigonometric (as are Hipparc’ho’s klímata) and a case where it is reasonable to test\(^91\) for possible ancient data-alteration, since all seven circles are clearly a version of the traditional Seven Klímata.

Fit-probes upon them initially produce ridiculous, yet trending obliquities. Experimentally shifting all circuli \(M\) by the same simple round constant,\(^92\) 1\(^{st}\) (or 4\(^{th}\)) finds gratifying consistency with the same accurate Hipparchan 23\(^{2/3}\) obsolete discovered by Diller and insubstantially & cementally rejected by Neugebauer&Jones (fnm 18&25, resp).

Rawlins’ 1984 paper (invited for the Greenwich Meridian centenary Longitude Zero Symposium), featuring its 1\(^{st}\)-constant-shift solution (fn 91-93 here) of the circuli, has never been cited by Jones, though his own pale constant-shift ploy for the Hipparchos-Strabo klímata (§§C5-C10 above) is either suspiciously or parapsychologically similar, with the difference that Rawlins’ constant-shift solution for circuli turns chaos into coherence, while Jones’ constant-shift for klímata is subtractively designed to accomplish the very reverse, leaving such a poor fit to the data that he doesn’t dare even tabulate them (fn 34 above). Jones mentions Pliny’s circuli but simply calls them “crude”\(^93\) (in the Neugebauer tradition, fn 18 above), never telling readers that Rawlins’ restorative-correction procedure — which is just as elementary and trivial as his own failed (fn 34) klímata-echo of it — reveals a trigonometric scheme whose cleverness is crudeness’ antithesis. Nor does Jones’ circuli-puttown tell readers that these restored Pliny data are consistent with (fn 92 here) the very same accurate Diller-discovered 23\(^{2/3}\) Hipparchan obliquity Jones is busy trying to

\(^89\) Conversely, our tamper-projectors staunchly restrain production for either of our cases here where its need is screamingly obvious: Trio A’s 3\(^{rd}\) eclipse (fn 54 above) and Pliny’s circuli (present chapter). An awesomely perfect-predicting record for perceiving data-correction is and is not appropriate, and, as ever, implying that DIO’s simple, neat fits are merely déclassé’ sorcery.


\(^91\) Rawlins 2009S Fig.1 & fnn 46-48&50 belately weigh indicia that Hipparchos himself probably designed the circuli (contra this: one would expect Pliny to have attributed), which are more accurate than one might expect from their numbers’ roundness, a familiar ancient circumstance (e.g., §D4[B] above) & see evaluations at Rawlins 2002A §§A6&A11.

\(^92\) See Rawlins 1985G, comparing eq.11 vs eq.12, where an ancient scholar’s well-intended but uncomprehending 1\(^{st}\)-shift-of-\(M\)-data is explained at p.263. All of the circle shadow data are expressed in feet except for the Rhodes shadow, which is listed as 100 inches. Resolution (*ibid* n.15): 105 in Latin was written ‘cv’. The ‘v’ was mis-taken by an ancient scribe as an abbreviation for “vinciae” [100] inches extended to “cv vinciae” (105 inches), as we now find in Pliny. Once this is realized, and other data are checked, it’s obvious that 105 is the common denominator to all seven shadow ratios (but Rome) & is key to the equation (*ibid* eq.11) that originally generated the Pliny circuli (ere ancient alteration to eq.12).

“Crude”: Jones 2002E fn 11. Or clever? See restored circuli’s smoothly and flawlessly spring-tracking curve, graphed in Rawlins 2009S Fig.1, in hollow dots: close proximity to the curve of the data filled in (Diller-Rawlins klímata values), over the restricted Mediterranean range (much smaller than Diller’s) for which the circuli were designed.
N17 It is revealing that the JHAD clique has not only failed for decades to understand the central (§M5) significance of the Almajest’s three lunar periods’ high accuracy — but to then let shunning dictate refusal to cite the 1st solution (§N16) anyone has yet achieved, for how these periods were obtained?! That’s non-citation with an impressively unanimous lockstep. . . . (Is there a prize we don’t know about, for this special brand of perfection?) To summarize, cultists keep secret from JHA readers and the larger public BOTH: [1] the historically revolutionary empirical significance (§M3) of the inescapable fact that all three ancient lunar-speeds are micro-accurate, and [2] the eclipse-cycle source (§N16) of such accuracy. (Let Animal House’s Dean Wormer top THAT Double Secrecy.) And don’t miss the saddest&sadist-masochist part: all disbeliefing archons are religious Babylonianists, but their coherent disdain for target-heretics trumps even worship of Babylon, as their rabbotic shunning requires every single cringing cultist to forgo reveling in the science achievement by any civilization: observation and prediction from a single digit, one nds ratios that are in the previously-unexplained ancient records. During the 1 1/2 decades since publication error, nor [b] any alternate eclipses also producing the data we have solved-for (which would show our solutions’ non-uniqueness), nor [c] any below-horizon eclipses used in the analyses. Nor [d] the courage to cite or debate this theory in print: fn 6.

N18 Rigid, total, as-usual-unanimous, high-odds-oblivious rejection of 5 new data-match-based proposals central to the field: [1] Computing from his saros-based 4688ĕ Great Year, Aristarchos originated the “Babylonian” month, 29’19¾00’50”, decades before Babylon (§3 44; or Rawlins 2002A eqs.4-8). [2] DR’s 2001/6/27 British Museum lecture showed (ibid: eqs.9-11; or §3 55) Aristarchos applied to this the Metonic cycle (235 months ≈ 19), resulting in his Metonic “tropical” year Yₐ = 365½/4 – 15/4868 (fatefully off by 6¾, virtually same as Metonic cycle itself). [3] Years before item [2] was discovered, the matching recovery of exactly this yearlength was discerned in Vatican-held ms data listed under Aristarchos’ name on Var. gr. 191 fol. 170 (data at Neugebauer op cit p.601, §3 55; or Rawlins, op cit fn 14-15), yielding sidereal year Yₛ = 365½/4 – 15/4868 (good within a few time-seconds), again altering no Vatican-ns data; and, again, a hit: the

(Which expert mathematician Jones tends to treat as a kind of evidence. Whenever out of the real kind.) From Islam to Aquinas to the JHA to Isis to the Berkeley vandal-shouters: heretical material is valueless junk, so why not cage or destroy it? G9 No other academic combatant ever previously thought of explicitly defending such manuscript vandalism. (Though see Rawlins 2000A 13.1.) I.e., it’s the kind of originality that explains why pioneer Jones adorns JHA’s elite Board of Advisory Editors.

G10 Jones finds space to rummage through several admittedly shaky (utterly unproductive) data-alterations by a spectrum of previous scholars — but just can’t spare room for alerting readers to the existence of DIO’s fruitful analyses & matches, none of which require the emendation of a single digit. So, while suppressing mention of DIO’s known reconstructions, he is further sterilizing (fn 85) the attested ancient evidence they match.

G11 Though Jones’ paper does not even recognize the existence of DIO’s inductions (based upon the very material he’s Talibombing!), he is fully aware of them: [i] According to a Jones—Rawlins 1999/7/14 letter, he was reading the very DIO that issue 1st disseminated DR’s continued-fraction-analysis (of the now-Jones-de-credited Vatican ms data) resulting explicitly in Aristarchos’ Metonic yearlength, 365½/4 – 15/4868. [ii] Jones was an active listener when Rawlins’ 2001 British Museum talk presented his now-fully-developed series of astronomical-odds, digit-for-digit matches to UNTAMPERED data, e.g., 1778022 & the multiple confirmation of 4868 (i.e., §G5’s Discoveries #1 and #2), the culmination of a century of scientific analysts’ ultimately quadruple-confimation of Aristarchos’ 4868 Great Year. The out-of-the-gate after-lecture commentator was Jones, denying their slightest possible significance. To the immortal discoverer of the Winter Equinox (fn 86 here), it’s all mere Luciferan quadruple (fn 88) coincidence, though his 100% rejection’s plausibility may be gauged from his 2010 paper’s silence on DIO’s Aristarchan numbers (no claim of DIO mis-calculation), even while (fn 85) he defaces the ms data they match.
The Greatest Faker of Antiquity

2018 D.Rawlins 41

K
y.
[5] The dif-

results which yield:

&

IS PRECESSION

pre-Hipparchan discovery of it. His value is near 1

Great Year, as already identified

rounded to exactly 1

century & adopted by Ptolemy. Having adamantly rejected all

5 of the above ideas, from the oor at the 2001 event, Jones 2010B (pp.21-22) won't cite

Discovery #2: Merging [A] Aristarchos' Great Year, [B] his

G4 above), & [C] Meton's

famous lunisolar relation25 235° = 19°, we next arrive at a vindication for the kind of ex-

plosory hypothesizing ([31] f) that can occasionally move knowledge ahead: 4868

years of 235M/19 each equals Aristarchos' Metonic Great Year, 1778022° (15° less than

his Kallippic 4868° Great Year). Dividing this result by 365 yield the cycle's year-length

365°1/4 = 15/4868, perfectly matching the figure (Discovery #1) independently found

[§G2] two decades earlier from Vat. gr. 191 fol. 170v: classic predictive success. Nonetheless, Muffioso typically refuse to cite (e.g., [§G1 below]) the confirming evidence. Jones even goes out of his way to destroy (§G8 below) evidence for Aristarchos' multi-obvious possession (c.280 BC) of a monthlength accurate to a fraction of a time-sec, the better part of a century before Greek-conquered Babylon is known to possess such (c.200 BC) — possibly, just possibly, because pan-Babylonianists have made a living indicating that the purpose of such wisdom and precision is to be found only in the cuneiform tablets of a plainly inferior, scientifically-unsophisticated and tridgets civilization (fn 120; §2 [N13], which their cult has become permanently, underetably in-love with?

G6 We can also merge both cycles found from the Vatican mss (§G2) by noting that 152° is virtually 1/32 of 4868°, which allows us to see [2017/6/6] that Aristarchos' Sidereal Great Year is 32° longer than his Kallippic Great Year, thus 1778069°. (Heath 1913 p.315's reconstructed year, showing Aristarchos toyed with a day-rounded exeligmos, is an admirably clever revelation, but not sidereal.) The sidereal year must therefore be 365°1/4 + 32/4868 = 365°1/4 + 1/(152+1/8), or about 365°1/4 + 1/152 (which of course matches §G2's Vat. gr. 381 fol. 163's yearlength). [Since 152° + 32° = 474°, we see (2017/12/27): by Aristarchos' Great Year scheme, precession is 47°/4868, roughly a degree per century.] Summarizing: the two continued-fraction solutions we found to be embedded in the Vatican mss' data, were derived by him from his Metonic Great Year of 1778022° & his Sidereal Great Year of 1778069°, resp. The latter's obvious parallel to the former just adds to the astonishing multiple-vindication4 of the two solutions drawn from the Vatican mss.

G7 But pre-knowing that such redundant success is just superficial DIO witchcraft, teamplayer Jones volunteered to confront an awesome challenge:35 how to alter evidence, to undercut a paper that doesn’t, and how to trash into chaos coherent&kantampered inductions which twice accurately extract cycles (4868° & 152°) connected to the very astronomer — Aristarchos — explicitly named in each instance right on the Vatican documents.

G8 Though Rawlins' math is ineluctably rigorous, subtractor Jones won't be denied & so nimbly sidestows to a sly Gordian tactic: erase all acccents on the mss (the cripple—triple ploy of fn 88), which automatically, deliberately wrecks the data-basis for Rawlins 1999's refined, precise, precise inductive journey from Vatican mss to [1] ancient science's ingeniously constructed (Rawlins 2002A §5)) & modern induction's 4-way (fn 88) reconstructed 4868° Great Year [itself from superaccurate M: §§G4-G5, www.dioi.org/jb11.pdf, eqs.5-7], & [2] Aristarchos' sidereal year 365°1/4 + 1/152. How does such holy warfare differ from the "query en-passant: would ancient astronomy historians accept NYU-InstStudAncWorldDirector Jones' deleting all acccents from the mss of the Almagest?!"

Like-Talibanish is his authoritarian justification for across-the-board wipers: decreeing acccents on Greek mathematical data are destroyingly worthless in his Expert judgement.

Note advice at Rawlins 2000A §2 & §21 regarding [A] caution before plunging fervently into establishment-cultism's bleak-hole of inescapable devotion (to t transiently dominant cults') theories — an unforgetting singularity which lies in wait to swallow the unwary recruit; and [B] treating contrary evidences not as downers but instead welcoming them as possible helpful warnings of more of same to come. The case of politically ascendant new JHA Editor Evans provides a particularly instructive example. Three decades ago he, anxious to please his JHAD sponsors and publishers, voluminously and naively issued his enormous double-lead-article (Evans 1987 — sixty-four pages of JHA-upfront anti-Newton, anti-Rawlins, anti-Ocamb-anything-apologia), using such outre resorts as already highlighted above (§L2 and fn 11), undeterrably explaining-away, to his (and mythical JHA referees') satisfaction, one-after-another oncoming evidences (fn 12) of Polemy's obvious theft of the star catalog. Indeed, Evans was so anxious to speed his archon-kissing effort into print that he oops-neglected to apply his very own proudly, laboriously-developed-for-pages atmospheric-extinction-formula, to the very Tycho Cen stellar foursome he himself ever-so-cleverly mis-adduces for a strawman ploy. (See Rawlins 1992V fn 25, for the beyond-belief-hilarious face of this incident, as all 15 JHA Advisory Editors and an international collection of six expert advisors, allegedly involved as referees, spent any time that they had, which any of them could have checked out in minutes. Note that exactly 6 referees never checked anything during JHA's notorious Farnese disaster, either: www.dioi.org/fff.htm#sxrg. Likewise, at least 6 alleged readers of Isis' 2016 December disaster missed 7 mostly blatant errors: see POSTSCRIPT of [§1 above].) Also, admire that among the JHA's numerous superlatives is the ultimate nastiness of how it takes to bill itself (e.g., on Wikipedia) as a peer-reviewed journal. Had Evans resisted the careerist impulse to reject probable arguments in favor of desperately improvable ones (§B3), he (as leadlong 1987 publication) could have escaped being sandbagged by the brilliant unexpected independent crucial test of Gradolph 1990. And we would all be happier for it.

83 See Moesgaard 1983 or Neugebauer 1975 pp.354f.
85 Dozens of similar DIO vindications are collected at www.dioi.org/win.htm.
86 DIO's exact confirmatory hits are akin to R.Newton's also-years-later fruitful success: §E3.
References

Almajest. Compiled Ptolemy c.160 AD. Eds: Manitiuss 1912-3; Toomer 1984.31
Gerd Graßhoff 1990. History of Ptolemy’s Star Catalogue, NYC.
O.Neugebauer 1975. History of Ancient Mathematical Astronomy (HAMA), NYC.
Keith Pickering 2002A. DIO 12:3.
Pliny the Elder. Natural History 77 AD. Ed: H.Rackham, LCL 1938-62.

51 Compiled c.160 (fn 20; contra Toomer 1984 p.1). The common, more respectful-sounding title, Almajest, is descended from the Arabic almajāṣ, Toomer 1984 p.2. So Almajest seems less corrupt.

close23 to the known (also seriously false) tropical yearlengths of Hipparchos & Ptolemy.
The 2nd expression suggested 365½/14 = 1/10 (−1/4) = 365 1/4 + 1/152, differing but
ordmag 10 from the actual sidereal year then (fn 114 below).
G3 Both results’ implicit periods, 4868 (Great Year) & 152 (2 Kallippic 76′ cycles between iconic Meton’s and Aristarchos’ S.Solistes, –431 & –279, respectively), are among the EXTREMELY46 few numbers long known to be relatable to Aristarchos, and the difference between the 2 induced yearlengths IS precession, the very discovery traditionally mis-ascribed to Hipparchos. Said difference is close75 to 1′/century, which presumably
later influenced Hipparchos to treat 1′/cy as a lower limit, though Ptolemy eventually
adopted 1′/cy exactly (Almajest 7.2-4). Note that Aristarchos is the only astronomer on the
Vatican manuscript to quote different yearlengths, obviously suggesting precession. As the
1st astronomer we know was a public geometerist, he is an apt candidate for true discoverer
of Earth’s precessional wobble.
G4 To measure the Moon’s mean motion & apogee, ancient scientists wisely chose (Almajest 4.2) the 4267 month eclipse cycle for its 126007th interval’s felicitous near
constancy (due to near-perfectly-integral return in 4573 anomalous months), regardless
of elliptic position. That interval’s tiny inconstancy-amplitude78 of c.1/22 guaranteed
the deduced monthlength’s accuracy to one part in ordmag 10 million. (Divide 4267
[4267 months] by 1/2 to see this; the result is merely an upper bound on the better accuracy
attainable by round-the-zodiac averaging.) DIO’s exploration of the 4267th
cycle vindicated Ptolemy’s oft-doubted contention that it was the historical source of the ancients’ highly ac

77 Metonic “tropical” year: Almajest 3.1; Rawlins 1999; Tihon op cit; origin 1st century by, e.g., Swerdlow; cause of ancient tropical yearlengths’ large common error traced by a stimulating paper, Moesgaard 1983, and by Rawlins 1999 #4 & Rawlins 2018U #7.
76 Censorinus 19 & 18.11 connects Aristarchos to 1623′ & 2434′, which are 1/3 & 1/2 of 4868′, resp; see fn 79 below, & Rawlins 2002A fn 14-15 & eq.?
77 Ibid fn 1-4 shows that, whatever one’s sign-choices for the latter digits of the Vatican ms’s Aristarchos expressions, implied precession will still be near 1′/century, Hipparchos’ lower limit (Rawlins 2018U #5), verified by Tihon op cit.
78 The 2676′ cycle’s crucially & conveniently trivial inconstancy: 1st roughly quantified by Rawlins 1996C fnn 18&56, en route to verifying it’s the empirical source (Almajest 4.2) of ancients’ discovery of the key relation 251′ = 269′. Note revealing Mufotic inversion at Toomer 1984 p.176 n.10.
79 Ibid fn 1-4 shows that, whatever one’s sign-choices for the latter digits of the Vatican ms’s Aristarchos expressions, implied precession will still be near 1°/century, Hipparchos’ lower limit (Rawlins 2018U #5), verified by Tihon op cit.
80 Ibid fn 1-4, with the deft capstone-contribution, pointing out the conventional rounding-point, thanks to the long-experienced advice of John Steele and John Britton (Rawlins 2002A §48), neither concurring with our conclusion.

42 DIO-J.HA 22 12 The Greatest Faker of Antiquity 2018 D.Rawlins
observations by that amount on average (while not affecting his solstices), an ordmarg higher than his actual 2' random solar-declination single-data rms error (or scatter).

F9 For Almajest 3.1, Duke’s Table 1 lists a UH-contradicting Hipparchos –134/6/26 Summer Solst at near-PH-accordant noon. Pure invention. There is no such Almajest entry. (See fn 61&70 here.)

F10 Though fully aware of inconveniently-existing P.Fouad 267A, the same Duke paper nonetheless pretends that DIO’s now-papyrus-confirmed predictive hit-[a] & double-hit-[b] (§F5 above), are ENTIRELY ACCIDENTAL — occultist shades of himself and Jones (fn 45&36, respectively).

F11 He calls the EH orbit “neither conclusive nor satisfying” since (emphasis added) “parameters deduced from trio analyses [fn 46 above], are very sensitive to small changes in the input data.” CHANGES?! It appears that orbit-challenged Duke explored resorting again (as at §DS above, item [c]) to data-alteration, but STILL couldn’t find alternate orbits [i] which fit all the relevant data of Almajest 4.11 and 5.3&5 — which Rawlins 2012V calls successively Trios A, B, and C — as do the EH—PH (“Frankenstein”), EH, and UH orbits, respectively; AND [ii] whose underlying cardinal points (Vernal & Autumnal Equinoxes and Summer Solstice) uniformly hit upon Hipparchos’ standard 1°/4 precision — dawn, noon, evening, midnight — as 9 cardinal points for EH&PH&UH (not to mention P.Fouad 267A: fn 66 here) conspicuously do. Duke has been publicly challenged (fn 70) to produce his alternate orbits. Nothing has come forth.


G1 The mystery of the superficially-nonsense ancient yearlengths, found on Vat. gr. 191 fol. 170v and Vat. gr. 381 fol. 163v lay unsolved through decades of fruitless disagreements (fn 87 here). The name of Aristarchos of Samos is written beside two of these yearlengths: \( \tau \xi + \kappa \xi \xi \beta \) and \( \tau \xi + \kappa \xi \xi \beta \), or 364 4° 20' 60 2' and 364 4° 10' 4'.

G2 Taking the numbers exactly as they stand and allowing signage-flexibility, Rawlins in 1980 treated both Aristarchan expressions as continued fractions, and swiftly sent the results to the Journal for the History of Astronomy. Listening to Neugebauer’s perceptive recognition that 60 could signify 60 days, Rawlins saw that the 1st expression could be viewed as 365 \( \frac{1}{[4 + 1/20 + 2/60]} \) = 365 1/15 – 4865, a classic Metonic “tropical” year, quite

p.199. Invented Almajest –134/6/26 noon solstice: Duke 2008W Table 1. Challenging him to produce 1/4-day-cardinal-point Hipparchan orbits with [§F1 & fn 69] distinctly alternate elements: Rawlins 2018U §N2. Awareness of the P.Fouad 267A papyrus that confirmed DIO’s uncited [§F4] discoveries: Duke op cit n.9. For the –145/3/24 11 AM equinox observation (Almajest 3.1) of the refraacted Sun, measured on the large \( \sigma \pi \kappa \sigma \) (ring, originally set into the equatorial plane via [refracted] polestar light), which stood for centuries in Alexandria’s Palaestra: given that actual V.Equinor was 15° & accounting for refraction, there was a 1°-2’ error of observation, some of even that perhaps from over 100’ of the ring’s settlement (§10 & fn 122 below). See Rawlins 1982G fn 17 & Rawlins 2018U §B4. \n


73 Rawlins 1999, with welcome 1982 assist contributed by Owen Gingerich (cited ibid fn 7), regarding the utility of negative signs in continued fractions.


— From Acquittal of Ptolemy. Written by Swerdlov-dazzled Paul Hoffman, unsigned; instigated by Editor Dennis Flanagan who told Rawlins on 1979/2/7 that he didn’t like pipsqueaks who tear down giants, adding that Ptolemy might not be a giant, but Robert “Newton is a pipsqueak.” Did Flanagan even know that Newton was the scientifically brilliant Space Sciences Supervisor of the Johns Hopkins Applied Physics Lab? Not if he listened to the mathematically-challenged [§N7 and fn 8] trio cited in the piece he published [Swerdlov, Gingerich, & V.Thoren], reflecting the kind of muttered slander (more at fn 35) created and spread behind backs by parties many of whom even today keep believing that if they can just preserve or salvage some sliver of doubt that Ptolemy faked, they are thereby not utterly convicted of the vilest kind of academic misbehavior, in their decades of gang-smearing those who were — the ultimate irony — on the side of truth right along. Which merely adds ethical incomprehension to scientific. And why would a party have ever in the 1st place resorted to slander & shaming & running instead of inviting debate. IF (§M1) is genuinely believed evidence & competence backed its position? Why did peace never break out? DIO 16 p.2 fn 1 (2009), emphasis in original:

“Rational, pacific discourse shows who’s right & numerate, so: why would archons tolerate peace?”

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52 The Acquittal of Ptolemy. Written by Swerdlov-dazzled Paul Hoffman, unsigned; instigated by Editor Dennis Flanagan who told Rawlins on 1979/2/7 that he didn’t like pipsqueaks who tear down giants, adding that Ptolemy might not be a giant, but Robert “Newton is a pipsqueak.” Did Flanagan even know that Newton was the scientifically brilliant Space Sciences Supervisor of the Johns Hopkins Applied Physics Lab? Not if he listened to the mathematically-challenged [§N7 and fn 8] trio cited in the piece he published [Swerdlov, Gingerich, & V.Thoren], reflecting the kind of muttered slander (more at fn 35) created and spread behind backs by parties many of whom even today keep believing that if they can just preserve or salvage some sliver of doubt that Ptolemy faked, they are thereby not utterly convicted of the vilest kind of academic misbehavior, in their decades of gang-smearing those who were — the ultimate irony — on the side of truth right along. Which merely adds ethical incomprehension to scientific. And why would a party have ever in the 1st place resorted to slander & shaming & running instead of inviting debate. IF (§M1) is genuinely believed evidence & competence backed its position? Why did peace never break out? DIO 16 p.2 fn 1 (2009), emphasis in original:

“Rational, pacific discourse shows who’s right & numerate, so: why would archons tolerate peace?”
§3 Astronomer in Wonderland: Historians-of-science


Universities’ science departments deserve to know the kind of mis-math (fn 13), herd-think (fnn &10), data-tampering (§§B-G), & idea-grabs (fn 10, §§C8) too often passing for scholarship in prominent but joke-refereed (§2 fn 3; Rawlins 1991W fn 6) & cover-up/vol (fnn 10, 11, & 97) journals in history-of-science, a field rife with smears (fn 8), shunnings (fnn 11&16; Rawlins 1991W fn 171&173), threats (fn 109), & rejection of normal science (bizarre details: idem & §§26; fn 100) if favoring heterodoxy, with research-advances’ acceptance contingent upon whose clique the discoverer belongs to. (Repellent examples: Rawlins 2017E §§G3). Further, there’s mythy evidence that archons teach, value, or even understand (§§G5 & 11 [f], fnn 42&106) exploratory hypotheses’ use, tempered by Occam (§I25, fn 33, §1 A, §2 fn 49), to expand&define knowledge. The result (p.45 & §§B5&C-G) regarding advances in ancient astronomy, is inevitably more destructive than constructive. However incomplete, the following chronology is a start towards top academic institutions’ enlightenment re contemporary history-of-science’s frailties. (Even while DIO values the field’s finds [e.g., fnn 42&127&§114, §2 §F2&fnn 42], from which scientists have learned. Despite wan reciprocity.) Mathematical scientists’ scrupulous verification encouraged. Volunteer referees welcome (since the perps lack the will&skill): dioi@mail.com.

Continuing the history-of-science cult’s staunch tradition of exiling and/or gang-smearing such math-savvy, even eminent intruders as van der Waerden, R.Newton, H.Thurston: despite physicist D. Rawlins’ half-century of astronomical-history researches (samplings above & p.2), a said velox, www.dio.org/gir.pdf, of the following please-clean-your-house paper (with amiable cover letter), was inflexibly (fn 100) spurned in 2017 by the History-of-science Society’s Isis (ultimo US hist-sci forum), which refused to evaluate its History or its Science, while unable to deny its accuracy, relevance, or multiple demonstrations of the most prominent historians-of-science ALTERING DATA (esp. §§C&D-F-G), uncorrected-unretracted math-bettes (§B4, fnn 27&97), dreadful science (§C5), even weird science (fn 2). (And see fn 4’s conclusion, for the Journal for the History of Astronomy’s DEFINITELY-originial idea of refereeing.) Not to mention shunning of competent heretics’ scrupulously refereed research advances (§I), and systematic non-citation of the scientific-history journal DIO, though for over 25° it’s been easily the most mathematically and astronomically competent journal in the science-history field. Vol. 10 even highly exceptionally co-published (with the University of Cambridge), long supervised by colleagues composed of that rare minority of scientifically able historians (e.g., astronomer-legends E.M.Standish, emeritus CalTech-JPL, & Chas.Koval, late of STSI), so seethingly feared by the democratically-ruled majority, whose mathematical and ethical shortfalls DIO has discoveries [a][k][b]. (Thion has further shown that, c. = 150, Hipparchos experimented with previously unknown versions of solar motion, epoch, & precession.)

F5 Reaction to DIO’s vindications has been less than inspirational — but valuably revealing, nonetheless. While still under the influence (fn 59 above) of the late Curtis Wilson, Jones graciously assented to UH; but nowhere has it been acknowledged that (repeating for emphasis) Rawlins 1991W and (fnn 27&96) Rawlins 1985H revealed, years in advance of P.Foulad 267A’s surfacing in 2005: [a] a Hipparchos’ search for a – 157 stolissae; [b] his tables’ use of Kalippos’ way-out-of-date solar motion; [c] ancient stolissae were accurate to ordmag 1°.

F6 NB: These DIO-induction-predictions aren’t side-issues. They are central to understanding the early years of Hipparchos’ evolution from amateur-observer-astrologer into an immortal empirical scientist. And subtractors have been uniformly oblivious to a central steel connexion, revealing his original resort to calculating not observing his earliest, grossly wrong Summer Solstice in – 157, indoor-computed using the obsolete Kallipic calendar: this is the most conspicuously odd building-block of the lopsided EH orbit, accounting for most of why EH’s e/a were so agrantly awful:

$$e = 3\pi/44$$ (vs 2\pi/1.2 PH, & 2\pi/110 actual), A = 44° (vs 65° PH, & 66°/112 actual).

F7 In 2008, Duke, in yet another unrefereed Pb paper for JHA (whose board he had earlier silently joined while DIO Editor), scales new pinnacles of ancient-empiricism denial, as he tries razing the entire basis of Greeks finding accurate orbits (such as P&H.UH), claiming that their equinoctial solar declinations’ error averaged c.15°, nearly equal to the angle from solar center to limb! This is just a bad jowpaw case of confusing systematic error with random error. The Duke paper’s Table I displays admirably well-computed times of Hipparchos’ equinoxes, whose errors are clearly sprinkled ordmag 0°.1 positively&negatively on either side of zero. Undoing Duke’s historian-usual (§4 fn 43) listing of C – O as “error” (O – C), we see how much more crucially overlooked that the Vernal Equinox O – C errors are all negative, while the Autumnal O – C are all positive, since the Rhodos equinoxes were subject to 7° systematic error (found independently by 4 different scholars) which corrupted all these equinox denigration (equally well-refereed: see challenge here at fn 70) was adduced to head off that hideous eventuality. (And without even citing Rawlins 1991W, the very paper whose thesis is being trashed! – a wise precaution, to prevent anyone from checking anything — also without mentioning that idem’s math has been endorsed by various experts, specified at Rawlins 2018U fn 10.) No surprise. For a shun to keep working for (fnn 116&125), such intermittent commando operations are simply standard maintenance. And, unlike for a military attack, you can completely screw up, but — to your outerland protecting mob (§2) — it’s still a successful kill. Because, besides DIO, no known reader is surely any student of Hipparchos and/or of the paper in question (Duke 2008W) has yet read beyond its bald acceptence (ibid in Rawlins 1991W fn 106). Why take the trouble to question any of the paper’s assertions? After all (fn 4 here) if they’re in the JHA, they must be true.

67 E.g., 1995 Thron added note by Jones (& also creditable) Jones 2005. But here, in the neighborhood of above §C, and fn 85 below, there is double-irony in Jones 2005’s perfectly chosen titular quote from brotherfudger Ptolemy.

68 EH&P elements compared at Rawlins 1991W: §§K9 vs §§K10. Duke 2008W pp.293-294 calls DIO’s “recounting” in the footnote “due not to DIO error in the substantive text but to the word ‘said’ math is too “sensitive”, implying (fn 66) that §4’s double-vindication by papyrus was merely spooky-lucky. These inexcusably (esp. §D6 here) citationless attacks are met in fn 37 above, and another (ibid §§D8&C, and fn 85 below, there is double-irony in Jones 2005’s perfectly chosen titular quote from brotherfudger Ptolemy.


by him from UH, unbeknownst to Ptolemy); however, the 2nd alone agrees (by chance, as it happens), though the underlying mean longitude he lists for it is discrepant by 5° vs PH — even while tellingly agreeing to the arcm in with UH.

[4] PH orbit periodic error has amplitude 0°.4, so it formerly seemed odd that the Sun-based Ancient Star Catalog’s periodic error is 0°.2 — until recovery of UH, whose periodic error’s amplitude is 0°.2.

[5] Hipparchos demonstrably used the young waxing crescent Moon to fix his fundamental stars’ longitudes (as earlier realized by M.Shevchenko 1990); fixes’ average Moon-Sun elongation was roughly +30°.

[6] UH’s 1st (1778020-1780815) was both previously unsuspected. But, 14 years later, papyrus P. Fouad 267A was examined by A.Tihon (paper 1st presented: Peking 2005) & was found to explicitly verify 1991

— even while tellingly agreeing to his outdoor sky, none doubting (fn 97), — even while tellingly agreeing to the outdoor sky, none doubting (fn 97), — even while tellingly agreeing to the outdoor sky, none doubting (fn 97). Have shunning, censoring, data-fudging, & viciously (fn 4 fn 2) defending naked fraud by a cult-glorified pseudoscientific superstition-peddler (long notorious among scientists) devoted from merely-tolerated to insistently-normative? Simultaneously with Isis’ resistance to the below history, it was learned (see §1 here for links to all papers) that: [1] a 7°-old discovery (Rawlins 2008Q fn 6) had been unattributed published, www.dioi.org/ecv.pdf, as Isis’ 2015 LEAD article (report repair requested), and [2] Isis’ pseudo-refereed final 2015 LEAD article had extensively attacked Rawlins 1985G (Greenwich Meridian Centenary paper) on ancient longitude accuracy (refered on its mathematical merits by a panel of prominent scientists), calling such accuracy a “delusion” — due to the critic’s own amazing delusions (fn 97): [a] Treating a solar eclipse as lunar, neither author nor Isis even yet realizing it sorta matters. (See hist.sci icon Neugebauer’s able 1975 analysis at §1 [D]; so Isis’ cascading scientific innocence gauges hist.sci-decline since.) [b] Putting Spain into the wrong hemisphere. Rather than print DIO’s temperate Letter-to-the-Editor (§1), www.dioi.org/islg.doc, Isis Ed. H.F.Cohen fled (“I will not read, let alone respond to, any further messages on your side.”), play unanimously endorsed by his 30 Adv. Editors, www.dioi.org/isb.pdf, as Isis ducked refereeing the history or science of Letter or paper.

If this is the top of history-of-astronomy, one can imagine what’s going on underneath. But, then, actually, one need not imagine, since scores of examples of the field’s too-ordinary amusing scholarship are catalogued at www.dioi.org/jlah.htm, the oddest being “science” as credible as the Earth’s East Pole (Winnie the Pooh Chap.9): [a] the 1976 Dictionary of Scientific Biography 13:321 discovery of the Autumn Solstice and [b] JHA 22:2:119’s 1991 discovery of the Winter Equinox. See §§C-G for data-tampering by top polys, incl. the NYU Institute for the Study of the Ancient World’s Director, fictitiously brilliant Alex Jones.

Isis’ 2017 coverage of its 2016 sham-refereeing disaster (p.8 here: 7 largely-obvious undetected errors, 2 of them crippling) is just the latest example of the level of equity, ability, and openness at history-of-science’s most eminent & incestuous forums, which now exist in a state of such evidence-immunity and no-consequences self-rule that they have for a 1/2 century been tragically & punitively violating (awful details: §2 [N]) on the very opposite of the manyways-obvious ([110] truth of an issue as central as Greek astronomical empiricism, meanwhile becoming ever-more-incapable of self-righting the field’s ship.

After decades of observing science-shy historians-of-science and watching limited mentalities (fnm 8&96) like careerists Noel Swedlow and Owen Gingerich be elevated to an eminence that empowers their dementedly vicious smears (e.g., fn 34 & photos linked at fn 6) of those merely disagreeing with them, one may wonder whether historians’ too-frequent encounters, with scientists appalled at also-too-frequent technical-gooliness by history-of-science archons, have led to a defensive pattern (gameplan?) of curling-up into a self-protectively classicist-think quirk: “You can’t trust historians & scientists” (Johns 1949b §C4).

“We don’t want the history of physics to be written by senile physicists.”

It’s pathetic enough that the history-of-astronomy cult’s overarching vision of ancient astronomy hasn’t advanced for decades. But when we find it’s actually retrograded, aggressively undoing long-accumulated perceptions of wise scholars, e.g., P.Tannery, R.Newton, plus eminent astronomer & pioneer Ptolemy-exposer J.Delambre (2 centuries ago last year), then we might ask: [a] whether universities should keep implicitly endorsing such a field’s leashed research, and [b] if historical investigation in the mathematical sciences would be more openminded & technically able if it were hence to proceed within, or sometimes in supervisory association with, the relevant science dept’s of those universities that value it.
History-of-science — Data-Tampering, Idea-Theft, Seminumeracy, Smearing, Shuns, Club-Preference

Wellspring of a Projective Myth: Greek Science as Fumbling, Fabricating, and Unempirical

Muffia Cult’s 84 War On Greek Astronomers’

Cornucopia of High-Accuracy Achievements

Current Historical Advances Endangered

Summary: Ptolemy’s Apologists as The Greatest Alibiers of Academe

Carefully crafted and refined advances in the history of ancient astronomy and ancient mathematics: [a] have long been exiled by centrist-journal editors who shamelessly flee (fn 100 below) whenever they cannot justify their actions, as observed 34 unprogressive years ago by Robert Newton (Johns Hopkins University Press, Astrophysics Laboratory); and [b] are being smothered by a chauvinist battery of destructive, data-disrespecting — even data-fudging — papers, whose logic ranges from desperate to supernatural, displaying scant evidence of referencing or even specious canons of scientific evaluation as simplicity, minimal-premises, fruitfulness, and predictivity. Auto-rejection has been inspired by the new era of grantmagnet (Diller 1984 fn 26) orthodoxy that the famed ancient data-faking ([18], bumbling ([1] §E) mathematician-astrologer Claudius Ptolemy was “The Greatest Astronomer of Antiquity” (like hype at, e.g., fn 9) whose allegedly-outdoor solar observations’

1] New early spherical trigonometry date, 2nd century BC (fn 16-17&24 below); the same era’s 1º-accurate trig tables & 1º-accurate calculations (fn 37); 3rd century BC Greek scientists’ use (fn 42) of order-of-magnitude (ordmag) and their adoption of degrees (fn 94). [Superscript glossary: [§132.]

JHAD = cartel of JHA ([Journal for the History of Astronomy, O.Gingerich principal editor for 40°) & HAD ([Historical Astronomy Division, Gingerich long-dominant co-founder) of the American Astronomical Society, whose Ethics Statement has just denoted [2017/10/11] research ethics three notches: now behind [1] racegender, [2] sex-triggers, and [ironic in present context] [3] bullying. DIO argues evidentially for high Greek accuracy ([§10; Rawlins 2017E]) & Hist. sci reverence for allaying inaccuracy, to ameliorate Ptolemy's gross fabrications, e.g., inverting relation of theory & evidence (Ragep crudely: fn 9), & focusing on Greek “theoretical structure, erected in spite of the enormous difficulties that beset the attempts to obtain reliable empirical data” prominently quoted by Gingerich 1976 p.477; see fn 8, 62, & 97 here. Neugebauer 1975 p.531 crowned indoor astrologer (Rawlins 2003X) Ptolemy “the greatest astronomer of antiquity”, echoed verbatim by Gingerich 1976 [AAAS!] & Gingerich 2002. Since Ptolemy’s Almagest contains much of what survived from ancient mathematics & math-astronomy, it has become accepted-in-practice that grantmanship requires continuing pretense that this invaluable astronomical handbook (the 1st great modern translations call Ptolemy’s Almagest and Geographical Directory “handbooks”: see each’s title in References below) was primary science (fn 9), not derivative (which it obviously was: [2] §§2&2N20 here, or Rawlins op cit, whatever the cost to plausibility and ethics. Another JHAD promotion of derivative science as primary: fn 120. Consistently invincible auto-rejection of high-odds, perfect-hit solutions, which have the effrontery to contravene current orthodoxy, encourages vulnerability to adopting embarrassingly unlikely alternate theories, and thus (effectively) escaping into the miracle world of the supernatural, palming off — as valid scholarship — notions unworthy of a rational enterprise. For a JHAD-wayhowse of ultra-outré occultisms, see here at: §§C11, D2&D3, E2, G7-G9, G11, H4, I22; fnn 12, 33, 44&45, 55, 68&69, 89. 3 DIO’s principled approaches to knowledge are brought together below, at §§1 [g]. See, too, fn 10.

BM55555 [ACT #210] (c. – 100) bears a yearlength computed from Greek solstice data, an unexpected, shockingly-orthodoxy discovery: 1st definite proof of what must have been substantial Greek influence on Babylonian astronomy (e.g., fn 120 & §2 [N13]. Backed immediately by Britton and long accepted near-universally; but lately unnoted, except opposed by Duke (non-citationally), using a faked Almagest 3.1 report: fn 70 & §F9.

F3 Recovering the US orbit cleared up a half-dozen mostly-long-lasting mysteries simultaneously (resulting DIO papers never cited by JHA):

[1] Why the Aristarchos —279 and Hipparchos —134 Summer Solstices are the only61 two among the twenty-eight solar records52 of Almagest 3.1&7 where Ptolemy (who abhorred discrepant data) suppressed the hour — which we’d never have known, absent Babylonian cuneiform text BM55555.

[2] Hipparchos’ final three calculated positions63 of the Sun at Almagest 5.3&5 generally conflict with PH, but are all consistent with UH. (Jones appears to accept this analysis.)

[3] When Ptolemy recomputes those true longitudes (via the PH orbit he adopted throughout the Almagest), he twice finds disagreement with Hipparchos’ reported values (all computed

61] Below, §§P9; or ibid §§B3&B4. BM55555’s revelation: ibid eqs.6&8. I will ever be grateful to the editor to DR 1980/8/15: ibid § (A5) that scholars (including DR) were ignoring Ptolemy’s hour-omission for two of the Almagest 3.1&7 solar data.

62] See, e.g., the bizarre attempt at Neugebauer 1975 p.284 (followed by Evans 1998 pp.273-274 & n.32, etc., contra ibid p.209, as noted below, at fn 127), to claim that Ptolemy was a BETTER observer than Hipparchos, oblivious to their relative errors, random & systematic (Rawlins 1999 §E — the section of this paper which was suppressed by JHA Editor Hoskin, without showing error of any kind). This joke-inversion is based merely on roundings in Hipparchos’ semi-popular Commentary which are cruder than for his regular longitudes (Almagest 3.1&7.2) or declinations (Almagest 7.3). Neugebauer 1975 pp.642-643, deems Aristarchos’ data nonempirically fake (similarly Evans 1998 p.72 vs Rawlins 2008R §A, sardonically at §A3, condensing the most unexceedable of JHAD fantasies into a few lines).

63] Ptolemy did, xed the monthlength and sidereal year to high precision — was a minor, confused mathematician-astrologer Claudius Ptolemy was “The Greatest Astronomer of Antiquity” (like hype at, e.g., fn 9) whose allegedly-outdoor solar observations'
hugely contra-reality super-adherence to 280°-old indoor tables, is uncriminal since Greek astronomers were theorists not empiricists, who suppressed — i.e., destroyed — data inconsistent with prevailing models (fn 8&9 below). The Princetitude’s iconic O. Neugebauer (Science seconding), “It makes no sense to praise or to condemn the ancients for . . . accuracy or . . . errors in their numerical results. What is really admirable in ancient astronomy is its theoretical structure”, a view defied by physicist R. Newton’s 1977 Johns Hopkins University book, *The Crime of Claudius Ptolemy*, and by D. Rawlins’ scientific-history journal, *DIO* (www.dioi.org/dioind.htm), which has fitted to attested ancient data scores of new heretical reconstructions (many evaluated below, esp. §I, with selected links), meanwhile asking how ancient astronomers coping predecessors could advance to their surprisingly numerous but heretofore remarkably unappreciated high-accuracy Greek measures. (Below, compare §§10&11[D] to craniolithic cult-insistence on Greek inaccuracy: fn 1, 8 [I], 69, 93, & 84.) Also investigated: ahistorical myth of “theoretical” Greek non-empiricism & data-selection; Occamite resolutions of such problems as ancient Earth-measure by Pharos flame & double-sunsets; the method explaining all 3 Greek-adopted temporal Great Year, 1458 Great Year, 1°-accurate monthlength, and pre-Hipparchos discovery of precession. One of R. Newton’s favorite expressions for counter-revolutionary mis-scholarship: A subtraction from the sum of human knowledge.

A Advances in Understanding Greek Science Endangered by Fudge Germinating Out of a Moated, Bloated Network’s Heresy-Phobia

A1 In the contemporary history-of-ancient-astronomy subfield, numerous coherent, mathematically-copper-fastened, expertly refereed, but archon-offending progressive discoveries have appeared for decades, elucidating hitherto-mysterious ancient data. When these offenses cannot be undone frontally, certain careerist serial knowledge-subtractors, — compactly called the Moffia or the JHAD (fn 1) hereabouts — have themselves made a discovery, to wit: that their network of politically centrist captive journals will help wage *DIO* against heterodoxy by publishing effectively unrefered articles that:

1 Wikipedia’s article on the virtually unrefered *Journal for the History of Astronomy* [JHA] actually claims the journal is “peer reviewed”! (See fn 109 below, also re Wikipedia’s 2008-2014 war upon Rawlins’ Wik-biography.) Meanwhile, the best-refereed journal in the field, *DIO*, is repeatedly, aggressively classed by Wikipedia as Unreliable (not deserving an article, with bio-references to Rawlins as “publisher” persistently suppressed), though neither Wikipedia’s CISCOP-soldier administrators and associated threatening cult-vandals (repulsive details also at fn 109) nor JHA have in years of trying and seething, managed to find incompetent scholarship anywhere in *DIO*’s score of volumes. Meanwhile, by a contrast that would be embarrassing to honest forums, *DIO* has (without even having to try) discerned dozens of flagrant examples of prominently-published history-of-astronomy catastrophes that nobody refereed with care (or cared to referee): see fn 50&86 below and at §1’s POSTSCRIPT; also www.dioi.org/jh.htm, www.dioi.org/fh.htm#vec. Historians-of-science seeking ready buddy-publication (to convince their universities and funders that they’re academically contributory) evidently prefer such laxity vs being refereed scrupulously, since far more vitae get padded by indiscriminateness than by care. Given the spectacular muff-fertility of the history-of-astronomy cult and its captive JHA, plus its distaste for wasting time on alien authors or on the bother of serious refereeing (fn 97&100), *DIO* from its inception has dubbed this incestuous, self-perpetuating cartel: “The Moffia”. (Considering the history laid out in the current paper, who could resist such an apellation?) A secret of maintaining eternal muffery is *JHA*’s rigorous refereeing of sufficiently
Deliberately: §§C10, D6, G11, fn 10, twice more at fn 121. (This pattern goes back at least to the prominent Sky&Tel attack on R.Newton by dutiful Mufffios Janice Henderson 1976, without citing his name or papers.) [B] Exhibit daring proclivity for the inherently improbable over the probable (even the most remotely of each: fnn 33&36&45), whenever seeking apparent evidential support for any cult-in-too-deep position threatened by new findings.

[C] Effect sterile destruction by demeaning or even outright altering (e.g., §D5 item [C]; §F10 fn 25&85 & [see fn 30&11&58]) the data upon which an offending discovery is based, decreeing that whatever version of said data was previously accepted (right up until the moment of hoaxing), is therefore found the 1st readily found the 1st unreasonableness only — of a convenient sudden — become suspect of unreliability, often requiring the subtractor’s own Expert revision or doctoring to have any hope of the data ever attaining cult—Acceptability. In-balance is the possibility that one or two or dozens of valid potential advances might languish indefinitely unrecognized, wasted.

A2 Below, we analyse, in §§C-G, a flock of recent instances of such data-fudgery-for-orthodoxy, aimed at submerging computely proposed, compelling, but still-little-known heretical historical advances. We also append, in § below, a score of potentially-heuristic examples of such advances where those, governed by agendas, shunning, & cliques — ever-attended by denigration5 of outlanders — instead (of resorting to data-alteration) just archon-oscillating contributors. One JHA Advisory Editor privately estimates no real refereeing is occurring, while another (who wanted DIO to stop refereeing at all, realizing it was the only way to compete with other Hist.sci journals!) believes papers by JHA favorites aren’t refereed, but rather preferred: straight-to-press unread (flagrantly, laughably clumsy Centaurus example written by JHA boardmember: fn 50 below), a practice actually eaiser by JHA insistence on printer-ready (Rawlins 1991W fn 6) and wordprocessor (www.dioi.org/pm3.htm) submission. (Such conveniences are only exacerbating a trend already underway [as DR warned JHA, 38’ ago: DIO 1.2 fn 6&[B4] whereby “editors” become little more than printers. Note DIO’s typical insensitivity, www.dioi.org/ths.pdf, in asking an editor to edit.) And it shows. Lucky for us, JHA refereeing’s judicious deliberateness is self-extolled by proud 1970-2013 Editor M.A.Hoskin. www.dioi.org/pm3.htm, emph added: “it is quite common for an article received at breakfast to be refereed during the morning . . . and the verdict sent to the author by lunchtime.” For about 60 cases of thus-inevitable JHA-published odd and/or miscalculated scholarship, see: www.dioi.org/s2013.htm, samples here at fn 10-11&86; etc. List of botches debuting in DIO has been or can be compiled. Maybe due to scrupulous DIO refereing?

At least until afternoon tea.


6 O.Gingerich. longtime head of Harvard’s scienceDep’t, defames Ptolemy-skeptics, www.dioi.org/pm2.htm, in private communications. E.g., his 2000 referee-report to Isis innumerately broadbrush-label the now-substantial (if largely silent for professional reasons) Ptolemy-doubting party as just a tiny “paranoic” bunch (§2 fn 5), merely for objecting to communal shunnings he at once everyone in the field knows are real, even attested: fn 5 here; and www.dioi.org/pm1.htm, www.dioi.org/j43f.pdf, “Naked Came the Arrogance”, Rawlins 1994S §§B5-B8; Gingerich. loc cit; and cult echo-slander sampled at www.dioi.org/j411.pdf, DIO 1.11 §§C7; also see fnn 16&20, as well as ibid §§D2-D3, vs §§3 fn 7. Establishment-resent Gingerich’s whackamole campaign to contain heresy is detailed at www.dioi.org/j43f.pdf, 1994. His referer reports on skeptics often pretend (between slanders) that it would ever so good to have the other side heard (§4 §A2). Were this not sham, his JHA would hardly have gone decades printing just Gingerich’s side of the Ptolemy pseudo-controversy, protecting readers from exposure to DIO’s too-dangerous evidence&reasoning, DIO’s reconstructions [A] are consistent in method (for both trios); [B] are rife with ancient-typical round-number elements (Rawlins 1991W eqs.5, 8-9, 11, 21-23); [C] change no Almajest 4.1.1 numbers, these already long-established by Newton’s learned 1977 analyses (§E below), & invent no convenient Hipparchan mechanical miscalculations.

E By contrast, Duke: [A] like Toomer, calculates R first for Trio A, then reverses course49 to satisfy Trio B, which doesn’t work, anyway, unless an extra variable d (hitherto not in evidence, in Ptolemy, India, Toomer, DIO, or elsewhere) is arbitrarily brought in to rescue the situation; [B] finds no round elements; [C] alters extremely precise numbers like 51°30’25’’ and 8°48’24’’ to instead become extremely precisely 51°19’37” and 8°44’08’’, respectively, though there’s no conceivable potential of eredraft, or any independent justification of fudgery so shamelessly explicit, besides rigorous issuance of The Right Answer. To.smooth DIO’s natural-flow-multitift coherent solution under a pillow fluffy with special assumptions&tampering, resurrecting the spirit of co-subtractor Jones, above (in [C] & below (fn 85). Like Jones (§C10), Duke has refused DR’s request to withdraw the paper.

D5 The non-manipulated Rawlins 1991W fourfold-fit reconstruction (above, in §§D2-D3) is never cited throughout Duke’s prank (did hypothetical referees even know of it?), though his paper originated as a challenge to that very reconstruction, vying for a DIO prize, www.dioi.org/prv.htm, but evaluated and rejected by DIO prize-judge Thurston. It was later published by Centaurus. (After refereeing whose superfluity is shockingly obvious for math and even text.) Contra Duke’s attraction to committing fudgery: throughout Rawlins’ researches, it proved unnecessary51 to “correct” any of Hipparchos’ calculations in order to draw coherent results from his data, so a historically new conclusion52 emerged:

Hipparchos’ purely mechanical computations are dependably flawless.

Robert Newton’s Foolishly-ignored Discovery of Hipparchos’ one-Eclipse-Defege Eclipse

E1 DIO shows (fn 56 below) Hipparchos’ – 157 Early solar orbit “EH” was succeeded by his – 145 prime-years’ orbit “PH”, in turn replaced by his – 134 Ultimate orbit “UH”. He added EH&PHip when computing lunar elements from longer-lunar-eclipse Trios A&B (§C1&§E2, fn 55. 55. 1997, consenting to warning: Rawlins 1991W fn 206.桂, was published years earlier at Rawlins 1991W fn 206) and wordprocessor (www.dioi.org/pm3.htm) submission. (Such conveniences are only exacerbating a trend already underway [as DR warned JHA, 1991]. Shunning’s reality acknowledged by Schaefer 2002 p.40 while endorsing Jones & Duke inadvertently double-confir54m Newton’s “incredible” prediction. Like Jones, Duke does not notice Newton’s 1st warning, & deliberately (§D6 acts as if the carefully-referred (§D3) DIO paper that did heed it does not exist, though it was unsuccessfully challenged (§D6 by Duke to arbitrate Thurston and (fn 32 above) precisely in Isis in 2002. Duke’s paper independently computes55 best-fit e&A of the Trio B-accordant EH orbit, & of the inevitably weird-restored-Trio-A-accordant orbit, as if original, though


Rawlins 2012V fn 17, and especially fn 22, where we find that no referee even read the Duke paper’s TEXT. For practices & business enterprises that lead to similar disasters, see fn 4 & 2 fn 47. Below fn 72. The 1st shift, discussed below in [E] & detected not to be a mistake but (worse) a deliberate Hipparchos-school fudge, as shown in Rawlins 2012V §G & fn 11.

Ibid §A3.


55 Ibid p.293. The Ake for Trios A&B, which are independently computed and presented at Duke 2005T fn 5 and Duke 2008W loc cit, were published years earlier at Rawlins 1991W fn 205&162, respectively. Half&half discovery (§E2 below): ibid §M5; and §M6 discerns that the 2 elements
$R = 1000\cot 87^\circ = 52'24''$, or, in $60'\cdot 3144' = 23'47''$ — thus matching $^{34}$ Tri’s $A$’s $R$ (above). Now, a common slip (ancient & modern) is confusion of unit-fract (inverse integer) & arcm, since each is signified by a prime-marker; so if we test the hypothesis that a Hipparchan-school computer later misread $52'24''$ as $52'12''$, we find, in $60'\cdot 3122.1''$ — exactly matching $^{34}$ Tri’s $B$’s $R$ (above). Unable to count the math, centrist pols (faces eternally, irrevocably cut into shun: fn 125) have, since the decades since 1991, had no reaction to this minimal-premises double-match of both $3144' & 3122.1''$, besides implicitly contending $^{35}$ (by pushing incompatible fudge) that this is all PURELY COINCIDENTAL. Any wonder the above Summary was forced to confront the supernatural? — see fn 2 here. D3 DIO also found double-consistency with its theory that Hipparchos’ calculations $^{1}$ had used eclipse-pairs $^{46}$, not trios (the trio-approach has never yielded unmanipulated data-match) & $^{2}$ [2] had sought only one unknown, eccentricity $e$ (or epicycle-radius $r$), not apogee-at-epoch $A_0$ or mean-longitude-at-epoch $e_0$. Thusly computing $e_0$, produced $327.39' & 247.30'$, respectively, each a neat match to the above ($\text{D1}$) corresponding attested Hipparchan data: $327.23' & 247.12'$. (Finding no mathematical error, Duke regards these matches, too, as just ANOTHER spooky double-accident.) H.Thurston & John Britton carefully verified all the 1991 paper’s supporting math & recommended publication. In 2005 soon-after-JHA-boardmember D.Duke defined those recommendations (despite Toomer’s honest acknowledgement of his 1973 speculation’s dubiousness), resurrecting it by altering — explicitly altering — Toomer’s numbers, to ensure its success by inventing technologically convenient calculations. DR, recently expanding $^{48}$ his 1991 orbit-reclamations, compares DIO-vs-Duke simplicities: 

that History-of-astronomy’s present Ultimo Archon mis-signed his 1981 paradox-correction ($\text{B4}$), but won’t admit so since silence helps keep alive the JHAD sacred cow of Greek inaccuracy. So far, his game has worked. Ability-consensus of academic’s honesty has proved Evans’ record here (faithfully carrying-on the exemplary tradition of 1970-2013 Editor Hoskin’s grasp of mathematical astronomy: www.dioi.org/fff.htm#fency): [i] 1981–1987 failure to sign parallax correctly, and [ii] 2015 failure (on solar-distance) to understand that if Greek observing accuracy was on the order of $\pm1''$ as Gingerich insists (Rawlins 2018U fn 3), and as Evans echoes [item 1] or $\text{B4}$ above), then since Eratosthenes’ parallax is given as ordmag $1^{90}$ ($0'56$ at Carman & Evans 2015 p.14), solar parallax’s uncertainty was ordmag $100%$ of its size (like $\pm1.5\%$). (Rawlins 2008R fn 35 argues that Aristarchos suspected an even higher ratio.) So only non-scientist pols like Polymy or JHA-Dists wouldn’t realize that the JHA-Isis 102’ distance’s ordmag-$1%$ (!) precision was way-larify too exact — when uncertainly is 2 ordmags looser. All this reveals an even looser grasp of Greek astronomy (& elementary mathematical astronomy) by two of the world’s most deliberately-emanent-hitory-science-journals. Among famous classical Greek astronomers, only faker Polymy insisted on solar-distance numbers of such naive wayoverprecision, e.g., Almajest 5.14.16, which Hartner 1980 p.26 justly deemed a “faire-tale” (like R.Newton 1977 p.198); yet another case where seeing Polymy as typical of his era, or even its dominant Greatest, has warped history-of-science’s perception of actual science in antiquity. $^{43}$ Rawlins 1991W eq.23. $^{44}$ Rawlins 1991W eq.24. Confusion of arcmin & unit fractions: $\text{ibid}$ fn 251; Neugebauer 1975 p.166 n.3, & p.729 n.15; Thurston 2002S p.60. Only DIO’s theory (or an incantation?) explains the odd but Mufia-unciated circumstance (Rawlins 2012U $\text{[15]}$ that Almajest 4.11.1’s two $R$ differ by less than $1\%$. For Toomer and Duke, that must be yet-another-longshot hypothesis. Pure chance: Duke 2005T, onlineLibrary.wiley.com/doi/10.1111/1600-0498.2005.470204, and see fn 68 below. $^{46}$ Rawlins 1991W fn 77f. This should be obvious since [a] Almajest presents the data in pairs, after all!, & [b] pairs-analysis yields matches to the trios’ attested elements, while other approaches don’t. Inducing that $A_0$ and $e_0$ were pre-assumed, not sought: Rawlins 2012U $\text{[12]}$. Pair-calculations’ matches of $e_0$ to Hipparchos’ attested values: $\text{ibid}$ $\text{[5]}$ & Rawlins 1991W $\text{[14]}$, & Thurston, loc cit. (Doubling double-occlusion: implicit in Duke 2005T. Summaries in Thurston op cit pp.60&66-67. $^{47}$ Toomer 1984 (fn 15 above) p.215 n.75; Dicks 1994 fn 42. Recycling: Duke op cit. $^{48}$ Augmenting 1991 analysis: Rawlins 2012U $\text{[4]}$ & DIO-vs-Duke contrasts: $\text{ibid}$ $\text{[2]}$ (or here at $\text{D4}$). Recognized Rawlins 1991W $\text{[7]}$-$\text{N15}$, & above in fn 41, 43, 44, & 46. Round-element coruncopia: $\text{ibid}$ eqs.$&89$, & $\text{K9-K10}$, M4, N10. 

impunitively ignore mathematically and logically solid but cult-displeasing findings for decades. This transparent behavior continues even despite 2002 national attention to “unprofessional” acts characterizing the Ptolemy Controversy (ancient astronomy’s hottest). Are debate-averse conservatives justified in unprofessional contempt towards unprofessionally uptopy heresy? That question is explored below, along with the field’s dominance by dissent-intimidation & shunning, which has only solidified (fn 125) since 2002, perhaps from scientifically-shy historians of astronomy hanging shy of scientific critics. Which suggests several questions that may lurk behind superficial arrogance: [1] Are historians of mathematical scientists the only historians who fear their own subjects, defensively paralysed, if even, if ironic, fake (fn 34, 12 fn55) higher expertise? Are their dept’s like French dept’s staffed by profs who don’t speak French well? And pretend it doesn’t matter. (It does: $\text{[4]}$; Rawlins 2017E $\text{[5]}$; Rawlins 2018V end.) [3] Is this why too many historians of-science cannot (e.g., $\text{[12]}$) admit mistakes (as scientists routinely do: R.Newton, B.L.van der Waerden, DIO 11.2 [on cover!], S.Goldstein, etc) & can be $100\%$ sure teamplayer fellow historians of-science won’t ever ask them to? [4] Does that relate to the inverse: science-historians’ endemic reluctance to acknowledge non-club-members’ alibi concoction was promoted nationally (through Scientific American: inspired by its Editor, mentor by Harvard’s Gingerich & MacArthur Scholar, who (fn 68) actually slandered률on in order to fudge alleged observations to agree with positions that were “theoretical” (i.e., computed indoors — so how does fudging differ from fabrication?) and throw away any that didn’t agree. 

B Hiding Modern Empirical Data: Boomerang Irony & Lawlessness

B1 When today’s Ptolemyists are determined to justify what they already knew but were never deliberately “investigating” — the unscientific but cult-approved deed of destroying data needn’t be anachronism. Apologists, for Ptolemy’s “observational” ordmag $1''$ errors, claim it was normal for Greek astronomers to compute outdoor “data” indoors. Or to fudge alleged agreements to agree with positions that were “theoretical” (i.e., computed indoors — so how does fudging differ from fabrication?) and throw away any that didn’t agree. 

and (especially scary) competence. 

$^{77}$ Schafer loc cit. But how “hot” is a controversy where virtually all prominent print is on one side? The covering side. 

Historians-of-science remain impenetrably loyal to the idea — which DIO J. 1 p.10 fn 24 deems “mas-simi-try?” — that Greek science is “peculiarly inauthentic” (i.e., computed indoors — so how does fudging differ from fabrication?) and throw away any that didn’t agree.
B2 In 1987, in order to justify the modern Ptolemaic vision of antiquity, JHA Editor-in-progress J.Evans published an exceptionallypolemical, technically pathetic, but politically brilliant Step-One towards becoming a Muñía Maid-Man by assassinating R.Newton's credibility — anticipating full well the boost he’d achieve towards his ultimate Editorship by attacking JHA Editors’ tête-noire-Newton, i.e., telling ‘em what they wanna hear, regardless of the cost to truth and to the reputation of one of ablest scholars ever to grace the field. (And regardless of whether Evans’ paper was valid. All that mattered to JHA was the attack’s teamwork-contribution to the pretense that Newton was as crazy as its cult’s unanimous goosestep-slander was insisting: fn 34. Which is why the parties soon proven right [14 $\delta$4] and the Catalog theft were excused, while those who were impenetrably wrong were elevated — the most-impenetrable lifted into the field’s politically-extra Editorship. No surprises.) Evans’ paper tried alibiing Ptolemy’s ridiculously huge errors to his and his cult’s satisfaction, by adducing three instances of grossly erroneous outdoor placement of a star’s position, from measurement of its angular elongation from the known-position Moon at mid-eclipse: Evans’ own 1981 Seattle observation of the star $\lambda$ Sgr — the record of which has since disappeared without explanation — and two ancient observations of the star Spica vs the eclipsed Moon (Almajest 3.1) by Hipparchos in $\pm$145 and $\pm$134. The errors were all ordmag 1$: respectively, $-40^\circ$, $-33^\circ$, $+33^\circ$.

B3 After in 1991 DIO showed (fn 10 below) these were not observational errors at all, a 2009 Rawlins paper detailed the precise explanation which shows that Evans’ three data ultimately support his conclusion’s opposite.

9 Rawlins 2009E $\delta$h (emphasis in original), critiquing Evans 1987, http://journals.sagepub.com/doi/pdf/10.17770/212886708100401, also Evans 1998 (appreciatively reviewed for its considerable merits by Thurston 1998D in DIO 8). Overview-question-in-passing: is there any reason other than ambition that would lead a scholar to look for a way to defend Ptolemy’s honesty where (as for the Moon-star case at hand) the evidence is too obscure for non-specialists to understand? At least already for at least 1200D $\beta$ Id.4’s $\alpha$-74 parameters & $\alpha$A’s fits are uncomplicatedly, high-schoolishly, arithmetically indoor-computed his four alleged “observations” of the Sun: $\pm$8 below. A textbook case of politics overwhelming reason — as it has, for ordmag a century of history-of-science’s ubiquitous, naked promotion-for-grantof-profit of a known scientific criminal. (See, e.g., §2 or Pedersen 1993 p.559’s justification of Ptolemy’s massive indoor plagiarism of Hipparchos’ coordinates, after initially denying it at 74 and 258; cf. idem §2 shows what Ptolemy had too much “integrity”). Further history-of-science contributions to ethical philosophy are announced from the field’s heights by NYU’s A.Jones (who knows Ptolemy faked science $\delta f$2 fn 2, but ranks true history [vs JHAD status] in canny political role): the Jones-edited 2010 Springer volume Ptolemy in Perspective (CaTech [!] 2007 conference, arranged by CaTech’s Swerdlow), is prefaced by Swerdlowian prose, “Among the SCIENTIFIC authors of the Greek-Roman world, none gives us such a strong impression of writing for posterity as Ptolemy. . . . no reference to himself except as an OBSERVER, scholar, and theoretician . . . . Nor is there anything meretricious in Ptolemy’s efforts to give his SCIENCE a public face. . . . he . . . made astronomical OBSERVATIONS [vs below at $\pm$8] between the mid-120s and the early 140s of our era” (caps added). The same Jones-edited collection calls Ptolemy’s fakes “observations of the Sun” with mere “errors” (Swerdlow 2010 p.151), adding that Tycho deemed Ptolemy a thieffraud and so dumped his fake data, epochally discovering accurate precession thereby: $\pm$2 $\lambda$A; Rawlins 1993D fn 141. In this same CaTech collection, we’re told (Ragep op cit p.126, emph added) that he in a typically ($\pm$2 fn 18) condemnationless history-of-science cliche “reply” to the fatal-for-scientists revelation that Ptolemy indoor-faked allegedly outdoor data: “But let us look at this another way. Ptolemy decided not to tamper with the year[length] he had inherited from Hipparchos” (the very datum used to fake all 4 of his solar data; compare #1 $\alpha$’s tampering & data is the scientific&equivalent of real scientists’ tampering with theory when improving same: just two O.K.-options for resolving theory-vs-data conflicts! Consider the revelation: this CALTECH-SPONSORED expression of inside-outside-science is considered the epitome of DEEP non-judgemental thinking, in the history-of-science commune, where no one in authority seems able to even tell a real ancient scientist from an occultist fraud. And see [1] §§H&T on the field’s robust enthral of Ptolemy’s concoction to astrology, believing in reckless defiance of the awful truth ($\pm$11) that only unenlightened, “paradigm”-insensitive scientists could suppose it reflects negatively on him.
his theory’s doubtless-illusory woes: they are the unreliable party, having committed the offense of disagreeing with the theory of the most authoritative expert (here, in fn 86), adding that Hipparchos’ trigonometry tables are suspect of a parallel disloyalty. Meanwhile, DR contends that the glad & enlightening opposite is recommended — both for Strabo’s klimata data35 and for Hipparchos’ trigonometry tables — by Table 1’s 14-for-14 fit, cited above (fn 24). Jones is doing a convincing imitation of one who imagines those astronomical-odds-defying 14 perfect hits merely36 constitute a paranormal or religious miracle, with no significance or status in his people’s idea of the real world of science, where Occult’s Razor slices an illusion like a 14-hits-out-of-14 table completely out of that special bubble, convincing it to the Orwellian-Greboevian flames it deserves.

C12 Note: it’s been obvious since [the Rawlins 1994M investigation] that 1” accuracy is crucial37 for the 18th klima’s fit (Table 1 above; or Rawlins 2009S Table 2), suggesting that the historical process of refinement of high-accuracy trig tables goes back further than generally believed, as successfully presumed throughout an earlier38 (1991) trigonometric DIO reconstruction of Hipparchan lunar orbits, discussed below, in §D and fn 38&39.

NB: We now have consistent confirmatory double-evidence for Hipparchan 1”-accuracy: [a] Table 1’s 18th klima, as just noted (fn 37). [b] Below fn 46’s neat hits for attested e&r.

C13 Ironically, D.Duke’s rejection (fn 69 below) of that reconstruction unintentionally highlights the Hipparchan trigonometry tables’ accuracy. It just seemed incredible to Duke that DIO’s analyses (precised at [§D] here) of Hipparchos’ Almagest 4.11 eclipse-trios could possibly be reliable, from sensitivity to tiny uncertainties. Yet we have multiple-verification of those analyses’ validity, because Hipparchos’ calculations (fn 24&52 here) & trigonometry tables were more accurate than previously believed by Duke or anyone else (including DR, before 1991 testing). So his/hotness’ now-prominent defects stubbornly just emphasize the shocking newness of the discovery of Hipparchos-era 1”-accuracy computation & trigonometry tables: [i] the two eclipse trios (customarily called “A”&“B”) confirmed each other by both producing Hipparchan orbits based on Kallippic motion (period 365⅔/4) [2] new papyrus testimony vindicated in 2005 ([§F] both of the novel inductions of DIO’s 1991 analyses: [A] Hipparchos’ search for a −157 Summer Solstice, [B] his contemporary passing adoption of Kallippic solar motion.

C14 None of these confirmations could have succeeded unless Hipparchos’ trig tables were indeed (as already indicated: fn 37 below) accurate to 1”, as later were Ptolemy’s (Almagest 1.11). Specifically, if DIO’s 1991 elicitation of lunar eccentricity e from Trio A had computed with just a trig function by 1” (from slightly unreliable trig tables), Hipparchos would have found other than e = 327/23 (the correct value, which is attested ([D1] below) & is found to be agreed (§D3) with calculation via 1”-accurate trigonometry tables. To repeat (§C12): [i] the Diller-klimata table’s 18th entry (here, in Table 1 & fn 37) and [ii] DIO’s eclipse-pair-based orbit reconstructive matches, both consistently establish the 2nd century BC as the earliest date we know 1”-accurate trigonometry tables existed.

Concluding this section’s revelations, of sph trig & 1”-accurate trig tables & calculations, 3 centuries before Ptolemy: we recall the mentality that long ago locked-in establishment-wise here, Gingerich 1976 p.477 in Science (!), blaming his hero Ptolemy’s huge errors on (caps added) “CLUMSY mathematics invented only a generation earlier” (12 §M1 [b]).

35 Strabo’s numbers are being repeatedly vindicated here (universally-accepted restoration noted in Jones 2002E n.9 conclusion), though his interpretations are fertile ground for reconstruction: e.g., above, in fn 25. Trigonometry-table “imprecisions”: Jones op cit p.17.

36 This, in a familiar chauvinist tradition we keep encountering here, e.g., in fn 25&85, and even more astonishingly at www.dioi.org/hihr.htm#csqcv, and below in §J. Computing odds against Jones’ theory (& Neugebauer’s): Rawlins 2009S §§J1, J3, & J6.

37 At the 18th klima in above Table 1, 58° 12’ 31” rounds to matching 58° 1/4; but 58° 12’ 29” wouldn’t.


B4 We quote from this 2009 DIO paper, which so precisely (and ironically) solves JHA Editor J.Evans’ 3 boomeranged eclipse-based star-longitudes that, during the near-decade since, no historian of science has ever acknowledged that the DIO paper even exists:

Among the gymnastic hysterical-astronomy pratfalls enlivening JHA’s hefty (640p!!) James Evans double-lead-paper attack [Evans 1987], upon (then-minority) Ptolemy-doubters, was Evans’ lordly illustration of [skeptics’] dumb overestimation of ancient [observational] accuracy . . . . [Evans op cit n.50 (p.275) presents his own non-telescopic (cross-staff) 1981 July 16 Seattle observational determination of the longitude of a star (λ 5Sr) by using a lunar eclipse (as Hipparchos had) [measuring the star’s angular distance from the Moon when [it was] 180° from the Sun’s already-tabulated position] — which after Evans’ reduction produced a longitude erroneous by −2°3/3, thus according to him (idem) showing that the huge errors in some ancient observations were so ordinary that such were a poor basis for learning anything about ancient science [i.e., condemning Ptolemy’s gross errors]. As further examples, Evans specifically mentions (idem & p.235) Hipparcos’ two hugely disparate Spica data [also eclipse-based] . . . which disagree by over 1°. He then draws for us a [Ptolemaist] lesson (emph added): “No better demonstration could be wished of the uncertainty attached to the method” of fixing stars’ longitudes by eclipses. However, when instructor Evans repeats the very same sermon (on Hipparcos’ eclipse-star errors) 11 later [in many-ways-valuable J.Evans, History and Practice of Ancient Astronomy (Oxford: Oxford Univ., 1998)] p.259 (“This shows the size of the possible errors in ancient measurements of absolute star longitudes”), he slyly deletes mention of his formerly prominent 1981 eclipse-star measures — which shows that (during the 1987–1998 interim) Evans had read [the 1991 revelation] & . . . that DR had discovered
that Evans’ and Hipparchos’ errors. . . . WERE NOT OF MEASUREMENT BUT OF BASIC SPHERICAL-ASTRONOMY MATHEMATICS. . . . [Yet] when ineducator evaluator Evans’ 1st-hand evidence somersaults, he just pretends he was right anyway, unable to admit DIO scored & “premier” JHA bellyflop... and Contra Evans, neither his own nor Hipparchos’ problems were observational. Both simply misconspired . . . valid observational data by using invalid math: the wrong sign for their parallax corrections... the [1981 Seattle] longitudinal lunar parallax $p_a$ was virtually $10/3$. . . . the sign mixup would naturally cause an error of . . . $-40^0$, & the laughably small $A$ (here in fn 12 & [110] &) sufficiently he rejected (Evans 1987 loc cit): “too small by about 40°”. (Typically, Evans had no comment since, despite [Rawlins . . . face-to-faces] & Hugh Thurston [by letter] directly bringing the matter to his attention.) After correcting for this Muff, we [see] the admirable smallness of the 1981 observational error of Evans (a dedicated student of ancient instruments & possessor of a steady hand, since the cross-staff requires it): merely 1 or 2 arcmin, just the sort of accuracy DR has consistently ascribed to the best ancient . . .

field has a word to say in criticism. Parallel Evansiana: despite denial of reading DIO, Evans’ clumsy unannounced sly-try 1998 eclipse switch (unsusuable details: fn 11 below, or & fn 47) shows he’d read Rawlins 1991W fn 288, DIO’s detection-revelation of Evans’ 1987 parallax-misuse, ever-uncited by him during 3 decades of hiding from publicly facing this central demonstration of his cult-engendered fallibility. Also even-Evans’s fn: DIO, www.dioi.org/j301.pdf, the standard critical edition of Tycho’s 1004-Star Catalog, Rawlins 1993D (fruit of DIO’s 7 years of scrupulous investigation & math-reconstruction: 1987-1994): flagrantly deliberate citation-avoidance in Oxford U.Press’ Evans 1998 pp 271-272 & n.28 thereon (p.459), 5 with after DIO 3 appeared. (Will scholars not following suit offer the obviously hyper-tender sensibilities?) In tender sensibilities published because by a journal correcting an Evans mistake he pretended was never made? (But doesn’t say so. [Indeed, doesn’t say anything.] And no historian-of-science asks. A field ruled by fear for decades. But note B.Schafer has admirably broken ranks on JHA-shunning of DIO 3.) More Evansscience appreciated in Rawlins 1992T §[HI-H7 & fn 65, and at Rawlins 1993D §[I8, where Evans is shown to unwittingly require Ptolemy saw 12° magnitude stars. (Ptolemy-Flamekeeper Evans in 2013 succeeded Hoskin as JHA Editor. Utterly apply.) Intermittent listing at www.dioi.org/hs/hh, & not listing other examples of deliberately-ignored (recall 1994/2 letter, above) revelations of undeniably erroneous but never-retracted Evans scholarship. See also DIO 8 §[4 fn 4 on the unsusably-actually & inadverantly non-empirical—empirical argument at Evans 1998 p.72, ultimately adopting (non-citationally) yet another DR original discovery §[I4, DIO 1.1 §[7 &[C1, Rawlins 1991W §[R, fn 99, fn 263&272; Rawlins 2008R fn 17): Aristarchos’ 87° half-Moon elongation as not precise but a lower limit. And don’t use www.dioi.org/jh.htm?&cm, on Evans twice copying J.Dreyer’s prose without quotation-marks. Irony: DIO’s Tycho star catalog & the differently-important Hipparchos parallax-sign discovery, were both triggered by Evans’ own mis-apollo for icon Ptolemy. At Rawlins 1992T §[H8 & Rawlins 2009E §[A6, find our gratitude to Evans&Hoskin for each of these gifts. (Like thanks to Jones&Toomer at ibid fn 207&292; to Duke, §C14 above & Rawlins 2012V fn 22; to the whole Muffa at Rawlins 1991W §[S3.) Evans’ modesty precludes him from citing any of these thank-yous. Compare to DIO’s rule of always correcting errors — for both integrity and refusal to mislead readers: www.dioi.org/err.htm, as well as below at §I14 & fn 98a&110, also DIO 1.1 §[4 §[A2 (1991) & DIO 11.2 (2003) front cover & p.30. JHAD’s perverse reaction to the contrast neon its priorities for all to see. And eyeroll.


12 Rawlins 2009E fn 5: “E.g., Rawlins 1982G p.263 & n.17 (dissed by Swerdloff, rejected by 1st

C9 DIO’s 2002 switch from Neugebauer’s half-fit, to Jones’ even-worse-fit (fn 33): a coronated cut flexibly changing-its-story (& data) to continue inflexibly rejecting Diller.

C10 JHAD-unrealized: Hipparchos’Data-Fudgery for Myth&Turf — Hipparchos’-raw data have unexpectedly SPECTACULAR sensitivity to even the most minuscule33 imperfection in the $\epsilon$ or the constant-shift A assumed for testing fits, which renders it especially astonishing that the Diller-Rawlins theory (even without help from an arbitrary [Jonesian] crutch-resort to a constant-shift) accords with all 14 data. (Superior even to the mathematically best-fit solution found by least-squares, which fails at klima 14$^0$/4.) Jones’ 2 premises, [i] Hipparchos’ $\epsilon$ was Eratosthenes’, and/or [ii] Strabo’s data need alteration by $A = 100$ stades, overkill-wreck any “scientific” “observation” he possibly had for his klimata data. He evidently didn’t know how he could run the required least-squares (a disability seemingly near-universal among historians-of-science: fn 34&100) — much less the multivariate version. We have attempted enlightenment by devising a simplified method of estimating bivariate-probability loci (fn 33 here), comparing the klimata analyses of Neugebauer & DIO: www.dioi.org/sta.htm#xmxw. Even accepting one of Jones’ two premises & using least-squares to adjust the other unknown, several non-fitting klimata ensue, as emphasized to him by phone immediately upon his 2002 publication. The reality is rendered particularly obvious by his failure — unique to this controversy — even to supply a table of data. (Diller, Neugebauer, & Rawlins all tabulate.) This, because neither his $\epsilon$ nor any other can fit his own fiddled version of the data. JHA’s intensive breakfast—lunch refereeing (fn 4 inexplicably never noticed the omission of a Jones table, or his non-citation of the undeniably better-fitting 1994 Diller-DIO table well known to him via his (Thurston 2002S) & direct communications (www.dioi.org/biv.htm#jphn). Question: why persist for 16 more years for nonfitter & actually promote (Duke same at xx)?

11 C12 Since 2009 April (when Diller’s last apparent non-fit unexpectedly was found to accord), Jones has kept contending over his shoulder that the data are to blame for not fitting (Evans, 2013)...
of Hipparchos. Jones does not merely pollute Strabo’s klimata with these alien data from scattered other works of Hipparchos, but uses them as an excuse to shift the entire dozen, suggesting Strabo’s data could have been anciently corrupted by addition of a constant, A = 100 stades (meanwhile Jones consistently miscomputes Syracus’ latitude by 200 stades), which he argues must be corrected-for — thereby justifying-excusing his replacement of already-perfectly-fitted unmanipulated data, with sub-50%-fitted (Table 1) revised data. Seeing Jones extrapolating from a very few exceptional klimata to the whole set, atheist DR is reminded of creationists who generalize from rare, anomalous geological strata to rejecting natural selection entirely. Obvious point against the significance of Jones’ disparate data-injections: he well knows that Hipparchos changed adopted parameters over time.

C7 Further, Jones (ignoring 2°31/3’s confirmations: [4H; Rawlins 2009S fn 23&54]) attempts Pluto’s 112 testimony that Hipparchos’ obliquity was Eratosthenes’ \( \epsilon = 23°51'20'' \), a value which Jones imaginatively attributes to a speculative Hipparchian computation from a conjured-up non-Hipparchian Alexandria \( L = 31° \), without realizing that obliquity \( \epsilon \) would already be known since ancient times found it concurrently with \( L \) — and via solstices, not as Jones curiously supposes.27 equinoxes.

C8 Jones also-traditionally alters the Almajest’s text for Hipparchos’ Marseilles latitude, from \( L = 43°0'4'' \) (consistent with 43°1/2 of the Toleda Geographical Directory [GDI]) to \( L = 43°0'1'' \), in order to reconstruct (using Eratosthenes’ \( \epsilon \)) 2nd century BC Hipparchos’ Jones-speculated indoor calculation-invention (for unstated reasons, and counterchronologically) of Pytheas’ well-known longago (c. –300) sidestool noon gnomon ratio, \( 41 \frac{1}{2} \) (whose precision argument was it an outdoor datum); and, to force the speculation’s success, Jones begs tolerance of an odd-but-convenient Hipparchian miscalculation, yet another ad hocke wrenching of ancients’ data. [While rejecting Table 1’s normal roundings!]

C9 Jones’ promotion28 of such jigsaw juggling seeks at least a half-share of Diller’s discovery. Worse: by fantastically alleging that Diller used invalid data, Jones (Neugebauerian: [§2]) lodges his half-fitted theory — unvetted (§10) and unbabulated-whimsical — as SUPERIOR to Diller’s ultimately-perfectly-fitting one. Though willing to refute once Diller’s 1934 paper (with 2 nonfits of 11 listed klimata, until DIO’s 1994&2009 upgrades: [§C3] & fn 34 end), shunsoldier Jones dutifully, consistently refuses to acknowledge the bare existence of Diller’s 1994 vindication: DIO’s initial near-perfect 12-hits-out-of-13 table29 (though its merit is prominently recognized by H.Thurston [I] & by G. van Brummelen’s meticulous standard history of early trigonometry [Princeton University]) — the final DIO 16 \( \frac{3}{3} \) update distributed in 2009, where the ultimate blemishless-fit perfection (Table 1 here) of Diller’s 14-for-14 victory is too irrefutable to deal with. Except by fleeing.

27 Jones loc cit. Syracuse misconputed: Jones 2002e n.10. He also controversially if traditionally altered a key Alexandrian datum: fn 25 here. His highly Creative obliquity-speculation: op cit p.16. His preference for equinoxes over solstices for \( L \) is Jones loc cit originate (spotted by Thurston); and below fn 96 vs Britton 1992 p.29. Non-Hipparchian Alexandria \( L \): Rawlins 2009S fn 30. Obliquity \( \epsilon \) found concurrently with \( L \) via solstices: ibid §§F2-F3 & eq. 8, and C1.12.8.
28 Original-text Hipparcian Marseilles \( L = 43°0'4'' \) (Almajest 2.6), rounded at GD 2.10.8 to 43°1/2, often altered to 43°0’1’’ (defining GD’s consistency with 43°0'). See Rawlins op cit [J. Achronology: Jones 2002e p.17. Pytheas gnomon ratio: Strabo 1.4.5 & 2.5.4-5. Rawlins 2009S exs.2-3.
29 ibid Summary [1a] and Rawlins 2009S §4.
30 Jones loc cit. requires Hipparchos rounded 41.713 (41;42.47) to 41 4/5 (41;48). though 41 2/3 (41;40) is rounder & almost twice as nearby. F.Rapego 2010 p.128’s Jones-promo sees no problem here.
31 E.g., Jones’ foreword to recent (long overdue) reissue of Pedersen 1974.
32 Initial Diller-Rawlins table of Strabo data compared to Neugebauer’s: Rawlins 1994M p.56 [aptly augmented in later renderings; as noted at fn 19]. JHA-uncited for 24’ now, but noted by Thurston 2002S p.67, and by van Brummelen 2009 p.65. Final 13-for-13 version of Diller-DIO klimata table: www.dioi.org/vals/wt.pdf. Diller 1984 Table 0. The table’s subsequent 14-for-14 expansion: here in Table 1 or www.dioi.org/jg03.pdf, Rawlins 2009S Table 2.

B5 (By decades of not admitting parallax-mismatch by now equal a kind of data-alteration?)
B6 The above-quoted 2009 article added: [i] detailed proof of DIO’s 1991 contention that the 2 superficially awful-looking Hipparchos lunar-eclipse star-placement measures cited above were accurate to ordmag 1° if his parallax-corrections were correctly signed; [ii] further, if Hipparchos’ hitherto-unexamined only other eclipse (~140/11/27) was used to fix nearby Regulus, undoing the very same parallax-sign-error shrinks the same apparent ordmag 1° error (common to all 4 mis-lengths) down to just a few arcmin (7’, more rounding error), yet again.

[Note: All data are subject to trivial error from ordmag 10° uncertainty in that era’s \( \Delta \phi \).]
B7 Exact data (Rawlins 2009E fn 22): JHA’s acceptance of the unchallenged record leaves 1967 errors of: 0.015, 0.007, 0.007, & 0.017. 

B8 The former amounts are obviously less credible than the latter, when set in the context of Hipparchos’ other known observations’ mean single-datum scatter (fn 12): \( 0.1 \) for 3 lunar- limb-vs-Sun measures, Almajest 5.3&5. 2’ for 17 solar equinoxes on Rhodes (mean’s traceable error 7’, mostly non-observational: Rawlins 2018U §4B), Almajest 3.1; 5’ for 17 stellar declinations (mean’s error 0’±1: Table 2 below), Almajest 7.3.
B9 The ore-refinement findings by DIO for the three-star-vs-eclipsed-Moon data raised by Evans have shock-vindicated DIO’s longtime position that Greek scientists were empirical and accurate — and the Regulus case constitutes classic coherent theory-fruitfulness.
B10 Has Evans produced his 1981 written record, to refute DIO’s 1991-2009 stimuli? No. He (fn 27 hand below); and colleagues cooperative averted gaze (fn 11 above) is consistent with the dreary theory that they prefer their own flubs to be granted silence, in return. Mutual consequence-free lawlessness.

C SphTrig’s Debut: A.Diller’s #1 Discovery Mobbed by Half-Fits
C1 Expressing them in stades north of the Equator, Strabo preserved a dozen-plus Hipparchan geographical lattitudes \( L \) corresponding to what ancients called “klimata”.31 narrow referee Toomer, Rawlins 1985G passem. [Rawlins 1985H,] Pro-Greek-competency: DIO 1.1 (1991) fn 24. Hipparchos measures of lunar limb-vs-Sun separation (Almajest 5.3&5), mean error 0°: Thurston 1998A ñ1. For Rhodes equinoxes, Hipparchos’ scatter (mean single-datum error) was 2’7: fn 70 here. With error 0’±1 (Rawlins 1994L, §G3), he found his geographical latitude \( L \) presumably from polestars, knowing stellar parallax was negligible. His \( L \) is inferable from his star-declinations, which show merely 5’ scatter (here in Table 2). Regulus-restoration: Rawlins 2009E eq.8. Correcting the four-stories-placed discussed here, for proposed parallax-sig-slip & for Hipparchos’ \( \Phi \) orbit’s shortcomings (at that era, primarily an error wave of amplitude 0°.4), the above-\$\beta$’s exact before-correction-vs-after data are found here at $\beta$(7 or at Rawlins 2009E fn 22). Evans’ refusal to recognize that DIO’s analyses have ordmag-shrunk all 4 of his and Hipparchos’ longitude errors (each from 0° to 0°.1 or 0°.1’ chance odds ridiculous) implies that he suspects scientifically-irrelevant dark magic, behind treasonous dirty-tricks [34 fn 48], & unprecedentedly insufferable quadruplé-lése-majesté.

13 Familiarity with klimata is vital to understanding the disgraceful ordmag 1° inaccuracy of Polymata’s geographical latitudes. For the purely astrophysical Hipparchian cause, see, e.g., here at §17 or, Rawlins 1985G p.260f. Both sources analyse evidence consistent with the self-evident theory that organized ancient scientists had corresponded for lunar eclipse local-time comparisons (accuracy limited mostly by ruling-ness of sundials&astrolabes used for timing eclipse-start/end), to nd lon-
Table 1: Diller Sph Trig Proof: Hipparchan Longest-Days in Hours ⇒ Latitudes in Stades

<table>
<thead>
<tr>
<th>Klím</th>
<th>Longest Day M</th>
<th>L Calcd Sph Trig fn 14 egn</th>
<th>Round Neart Degr/12</th>
<th>Conv Stads C3</th>
<th>Round Neart 100 St</th>
<th>Strabo Princ Instt Neug</th>
<th>NYU JHA Jones</th>
</tr>
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<tr>
<td>Eqtr</td>
<td>12°3/4</td>
<td>12°36'23&quot;</td>
<td>12°7/12</td>
<td>8808</td>
<td>8600</td>
<td>10200</td>
<td>8800</td>
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<tr>
<td>Cin</td>
<td>15°3</td>
<td>15°30'54&quot;</td>
<td>16°7/12</td>
<td>11600</td>
<td>12600</td>
<td>13000</td>
<td>11600</td>
</tr>
<tr>
<td>Mer</td>
<td>21°3</td>
<td>21°30'54&quot;</td>
<td>24°</td>
<td>16800</td>
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<td>17600</td>
<td>16800</td>
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<tr>
<td>Syc</td>
<td>33°1/2</td>
<td>33°15'25&quot;</td>
<td>36°1/4</td>
<td>25375</td>
<td>25400</td>
<td>25500</td>
<td>25300</td>
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<tr>
<td>EgL</td>
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<td>30°30'54&quot;</td>
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<td>28140</td>
<td>28800</td>
<td>28700</td>
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<tr>
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<td>33°30'54&quot;</td>
<td>43°1/4</td>
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<td>30300</td>
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<tr>
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<td>38000</td>
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<td>55°1/4</td>
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<td>40800</td>
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<tr>
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<tr>
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<td>60°1/4</td>
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### C2: Mistypically Diller as Competitor Not Colleague
Neugebauer attacked these findings by 1934 letter, later publicly branding them "absurd" and not even to be "taken only by his own highly irrational mathematical mis-steps: details in fn 97 below. From H.Buckle History of Civilization in England 1873 (1:318-320). In medieval times “the credulity of men had reached a height which seemed to ensure to the clergy a long and universal dominion. . . A book . . . sanctioned by [the most eminent] judges” recorded that the Carolingian hero Roland fought the Moors’ towering goliath Fenacute to no-decision until he “engaged his adversary in a theological discussion. Here the pagan was easily defeated” and, thus confounded, was quickly slain by the sword. When, despite being repeatedly informed of contrary evidence, our era’s equally eminent whistleblower-resenting SCIENCE journals (12 fn 11) dishonestly trust for decades Memnonian Jesus-hugger Gingerich’s insistence that his fellow occultist &court Almajester was The Greatest Astronomer, of an antiquity on whose ingenious empiricism (§3) Gingerich remains invincibly clueless, can we regard contemporary academy’s forums as any less deliberately mythmaking-for-the-cause than those of accurate history’s prior Dark Ages? 14 L = arctan((cos(7.5A)/tan e)) (L in degrees; M in hours); Almajest 2.3, Neugebauer 1975 p.38; further sample klimata tables, ibid, pp.706-736.


16 Diller 1934.

17 Diller fits’ perfection easily verifiable here Table 1, or at Table 2 of www.dioi.org/jg03.pdf, Rawlins 2009S. No JHADists agree. Yet the truth is plain to all but those controlling discourse, lockmawed even after 5/6 of a century of invincible non-innocence. seriously,”18 proposing his own typically Babylonian-in-the-woodpile arithmetical solution fitting only about half the data, claiming commonality of Hipparchos’ klimata with primitive arithmetical schemes, an idée fixe also mis-applied by him to Pliny’s circuli (fn 90). [Reliable test, by which one may discern a scholar confident in his creativity & ability: reacting to aliens’ successes not with jealousy and-or destructiveness, but with genuine collegiate appreciation. How many JHADsters have? Not zero, but too close to it.] C3 Rawlins tabularly19 and satirically noted obdient shunning of Diller’s theory by every one of the Neugebauer-mob’s altars20 (for 5/6 of a century now), though it fit roughly twice as many data as Neugebauer’s, while DIO introduced into Diller’s analysis the following improvements/improvement (Table 1 here): [a] Ancients’ standard 5° rounding of L and — after conversion to distance north of the Equator, at 700 stades/degree (Strabo 2.25.7) — applying customary 100-stade rounding of said distance, accounting for which converted all Diller’s near-hits to on-the-nose21 hits. [b] Finding several further Hipparchos-Strabo klimata unknown to Diller, which turned out to fit his proposal (not Neugebauer’s) in every case: SEVEN-fold fruitfulness (Diller 1984 [$D_3$], showing22 that while Neugebauer’s theory is indefensible, Diller’s fit Strabo’s data perfectly23 for ekevery klima: 14-for-14. C4 See step-by-step calculations, left—right, in Table 1, where Diller-DIO’s values (col.6) match every attested klima (col.7). (Neugebauer’s [col.8] or Jones’ [col.9] don’t. Both scholars’ thesis-killing misfits italicized in Table 1) are a MAJORITY.

How often does such success occur in this kind of reconstructive work?

### C5 Enter soon-after-JHA-boardmember&NYU-tenured A.Jones’ prominent 2002 Journal for the History of Astronomy [$JHA$] brief,24 in which Strabo’s data — previously wrecked-upon, by Dicks, Neugebauer, and Rawlins — were abruptly decreed, on the authority of Jones himself, to be henceforth considered unreliable, requiring re-do according to his judicious perception of the situation, as he rejects all three men in favor of his own new theory. All klimata data at issue were obviously from a single Strabo table. But, exceptionally well-read in the literature, Jones compared sources, noticing that 2 or 3 Strabo klimata seemed c.100-stades-discrepant vs corresponding (non-klimata) data in other works...