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 84°
History of Science Society Theft
BreakfastLunch JHA Refereeing
$100,000 Prize EclipsePair Hunt
Alltime Clumsiest Ptolemy Fakes
Unseen Aristarchos&Archimedes
This DIO is formed of papers (1-3) on new discoveries and academic crimes, sent to centrist journals backed by major universities’ scholars (& 4, on exchanges with one), invited to edit-out any parts they disliked. None found errors of math, science, fact, or other, instead just cutting contact. Boss-Tweedledee sneering in-effect: whaddayagonnadoaboutit? Has acadeae become home to those who ignore colleagues’ sins so long as their own funds flow? Papers 1&2[3-4] detail credit-theft from creators Gosselin, Delambre, Diller, Newton, Thurston by: mis-attack, lockstep-credit-nonsitation, data-tampering, grab, shun, fake forever-controversy ([2 §H2-H3, §3 §C10, §4 §B4], endangering knowledge-advances.

Leaving each paper nearly 1 as submitted entailed overlaps, but reflected how many forums ignore corruption & fle demois of poor stats ([3 §4, §F8, fn 100; §4 §C1 & §C19], abysmal science ([2 §N7, §J7]), esp. scientists’ attitude. (Ponder Ragep’s inversion at §3 fn 9!) No hist.sci forum faces its field’s hijinks&kuonk, nor suggests remedy-path, despite request for such, www.dioi.org/issw9o.pdf, so degeneracy persists, disgracing even CalTech, Princetitute, NYU ([3 fn 9&96, Table 1, §§C5-C11, resp). We value (e.g., p.44) History-of-science’s mining of new ore but regret when sacreadow minds warpe its use. DIO Publisher DR’s modest qualifications for these critiques include researches (unexpectedly later-vindicaded scores of times: www.dioi.org/vin.htm) appearing in such forums as Nature, Amer J. Physics, Astronomical Journal (AmerAstrSoc), M.N.Royal Astronomical Society, PA.S.P., Isis (Hist.sci.Soc), Vistas in Astronomy, Geophysical J. RoyAstrSoc, Norsk Geografisk Tidsskrift, US Naval Institute Proc., Archive for Hist. of Exact Sci, Astronomy, Queen’s Quarterly, Sky&Tel, CBS-News & page-one NYTtimes fraud-exposures (p.44 here).

This DIO’s & others’ bluntness has little to do with why historians-of-science fle us. [1] Calm scholars Johns Hopkins physicist R.Newton (Applied Physics Lab Space Sciences Supervisor) and Indiana University philosopher Aubrey Diller (long world’s leading expert on ancient geographical ms) were gentle but (SEE www.dioi.org/ns.htm) shunned&meared anyway for heresy. (Among others: p.44). Each’s final paper appeared in DIO: 1991&2009. [2] In 2017, DIO told (e.g., §1) toppe history-of-science journal Isis of [a] its 2015 theft, www.dioi.org/isa.pdf, of an original DIO 2008 discovery (residing for 7? in ordmag 100 libraries worldwide), & [b] its 2016 laughably math-bungled (§1 §D) attacks on DR’s 1984 Greenwich Centenary conference paper; pseudo-refereeing of Isis’ assault is Hist.sci.-typical, as inside-witless confirms (JHA breakfast—lunch refereeing: §3 fn 4’s finale). Angered by submission to Isis of an ultimately mild version (www.dioi.org/jqo.doc) of unnimd pp.46-85 within, whose history&science “history-of-science”—Isis somehow never commented on in five emails, Isis Editor H.F.Cohen wouldn’t acknowledge receipt of our protests against Isis’ 2015 echo of & 2016 attack upon DIO’s researches, instead emailing (pp.9&45) he wouldn’t answer or read future DIO communications. Our objections were then sent Isis’ Board as Letter-to-the-Editor: article §1 here (pp.3-8). Read it for yourself & see if it is an unreasonable or angry document, esp. under such outrageous circumstances.

That makes 3 history-of-science journals that have severed contact with DIO. Are our shirt-unstuffings and numerous puncturings of mis-science ([2 fn 50) really this scary to those regularly-blundering, www.dioi.org/jhpb.pdf, & pretend-refereeing ([3 fn 66) journals? Are they exaggerating our import by cowering so transparently? The reader may judge.

Main changes: DIO’s letter to Isis, www.dioi.org/jslg.pdf, has minigrown to §1; SN8 added to §2; & §3 is a plainly blunter version of www.dioi.org/qjo.doc (sent Isis), but has virtually the same content.

[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, §4 §1 fn 6, www.dioi.org/jel01.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 astronomers 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 Way’s researches, instead emailing “further studies.” Whose results can never work as efficiently as plain, raw multiplication.
B Shcheglov (p.705) calls early geography “a quaint illusion” — & his Abstract [cattipped Isis by promising] “Ptolemy’s reputation is rehabilitated in part, and the delusion of high-accuracy ancient cartography is dispelled.” The dispelling 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 defies the broad context of their high physical science as revealed by us [e.g., www.dioi.org/jl09.pdf, for decades]. Our ordnag-estimates of Greek accuracy (check out each for yourself):

1° for big cities’ geogr. latitudes L (Strabo 2.5.7; Isis 73.2 p.264; DIO 16 §3 §§C1-C2)
1° for scientists’ L (Isis 73.2 p.263; Centaurus 27 p.280; DIO 41 §3 §§E; JAHN 17 p.326)
0.1° for star declinations (ditto)
1% for Earth-circmfn. 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 11 §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 difference (DIO 16 §1 fn 24)

ditto or even 1° for star-vs-eclipsed-Moon gap (DIO 13 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.Foad 267A)
for — 145/3 equnox on Alexandria Palaestra polar-set ring (Isis 73.2 p.263 n.17)
10° for sidereal year (DIO 6 §1 fn 38&39; DIO 9 §1 Table 2; DIO 11 §1 fn 14-15)
1/4 century for motion of Mars and arguably Venus (DIO 11.3 §16 fn 26)
0.1° for synodic month (DIO 6 §1 eq.2 & fn 12&18; DIO 11 §1 eqs.1-8)
for 1 anomaly-month (DIO 6 §1 eq.13 & fn 12; DIO 11 §1 §§A3 & eq.2)
0.1° for draconic month (DIO 6 §1 eqs.20&9 & fn 12; DIO 11 §1 eqs.1&3)

Most historians-of-astrometer are, like Shcheglov, unaware of these symptoms of high Greek geographical genius, and Shcheglov is absolving himself from faking the eclipse data that scientists kept only theory-accordant data [Flatly contradicted by Hipparchos’ record] (§3 in 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 ordm-gradually 1° [anciently somewhat vitiated by sundial graduation limitations, ordm-gradually] agreeing (at 4/11°) with the well-under-1° accuracy of pre-stretch Geography longitudes, D.Rawlins 1985, “Ancient Geodesy: Achievements and Corruptions”, 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 accuracy, 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 3309/20 “Arbela eclipse”, Ptolemy’s reported time (longitude) gap is 4/3 too big, so Shcheglov’s n.8 tries allibing 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 Arabla).

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 for his Carthage-Arbela gaffe-gap). As for Pliny 2.72.180, Shcheglov knows Campania & Armenia are c.2° apart. [Longitude gaps between Naples & the Geography of Armenia cities (Diller DIO 5 Table 17; 1984) Dioskourias, Artaxata, Gaggara are 1°47’; 2°01’; 2°23’; respectively, all indeed about 2°]. So Shcheglov concluded that Pliny’s 3° is too high. Yet Pliny just can’t dispel the Campania-Arbela longitude gap is 3° but that the eclipse was seen 3° of local time differently. [Ancients recorded the time of an eclipse’s start: Neugebauer op cit p.844 n.12.] For a solar eclipse, one can’t just equate time-difference and longitude-difference. Local Apparent Time for the eclipse differed in Naples from that at the 3 Armenian cities, by 2°29’; 2°48’; 3°14’ respectively, mean 2°50’.' So Pliny’s 3° was not “badly over-estimated”. (Neugebauer, op cit, p.668, had verified Pliny, ed. correctly adding, “Solar eclipses are, of course, without value for longitudinal determinations.”) So, ironically, both of Shcheglov’s eclipse-examples for ancient inaccuracy have backfired.

E Shcheglov’s other Pliny record is the same Arbela lunar eclipse Ptolemy mis-report 8 P.M. at Carthage, 11 P.M. at Arbela. But Pliny has the same event 6 P.M. at Sicily (west Sicily was under Carthage then), 8 P.M. at Arbela, resp, both times correct within minutes. Shcheglov n.8 doesn’t connect the two Arbela-eclipse-reports; & neither he nor any other historian-of-science has noted that “authoritative” (G) scientist Ptolemy has accidentally miss-assigned Arbela’s 8 P.M. to Carthage! A check of his probable source, Pliny loc.cit., reveals how: by grammatical accident, Pliny’s Latin sentence places 8 P.M. nearer Sicily than Carthage (real 8 P.M. at Arbela thusly: 8 P.M. + 3° was nearer Carthage). Since his 4/3-stretched map already had 1°1/4 for mean motion of Mars and arguably Venus (p.583; 2 eqs.21&25), he faked Arbela thusly: 8 P.M. + 3°1/4), he faked Arbela thusly: 8 P.M. + 3°1/4), he faked Arbela thusly: 8 P.M. + 3°1/4), he faked Arbela thusly: 8 P.M. + 3°1/4), he faked Arbela thusly: 8 P.M. + 3°1/4), he faked Arbela thusly: 8 P.M. + 3°1/4), he faked Arbela thusly: 8 P.M. + 3°1/4), he faked Arbela thusly: 8 P.M. + 3°1/4)… !

For p.705, Shcheglov’s varied attempts at “rehabilitation” include his pure guess that Ptolemy’s sources were bad (as if The Greatest couldn’t better discriminate): “it would be just reality but surely of errors.” Yet we’ve just-above seen how a reliable source, non-astonomer Pliny, was revealingly, the sole eclipse-pair Ptolemy provides (Geography 1.4.2), to illustrate this central method, is half a millennium old, the 3309/20 “Arbela eclipse”, Ptolemy’s reported time (longitude) gap is 4/3 too big, so Shcheglov’s n.8 tries allibing 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 Arabla).

So Shcheglov’s n.16 calling Poseidonios a dilettante but Ptolemy an “astronomer” (p.694) a geographical authority is Quaint at best. Unmentioned in Shcheglov’s attempt to convince historians-of-science that Ptolemy should be somewhat “rehabilitated” (p.687): [1] He “usurped” Hipparchos’ 1025-star catalog (Tycho Brahe, Omnia Opera 3, p.337).
[2] “Astronomer” Ptolemy’s four allegedly outdoor solar observations are fifty times closer to his 280°-old indoor Hipparchan tables than to the sky. (Hipparchos’ ratio is less than 3.)

G So Shcheglov’s n.16 calling Poseidonios a dilettante but Ptolemy an “astronomer” and (p.694) a geographical authority is Quaint at best. Unmentioned in Shcheglov’s attempt to convince historians-of-science that Ptolemy should be somewhat “rehabilitated” (p.687):
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[3] His adopted latitude 30°58’ was -14° off reality, vs just 0°, 3°, 0’ errors for adopted
latitudes of real observers Timocharis, Aristyllos, Hipparchos, & Ptolemy’s Anonymous. 


H Such disasters warn of peril in history-of-science’s long-persistent glorification of Ptolemy as a scientist, while viewing his authorship of astronomy’s bible, the Tetrabiblos, as a factor that only culturally and historically narrow scientists would be bendighted enough to raise. Analyses to follow here reveal that astrology is intimately involved in destroying, probably forever, most of the latitudes in ancient’s now-lost competent maps of the Earth.

I Shcheglov admires Geography latitude-accuracy (p.689, emphasis added): “Methods for determining latitude, being rather simple, had [long] been known in Greece . . . . By Ptolemy’s time, latitudes of a number of the most important cities had been determined (e.g., Alexandria, Rhodes, Athens, Rome, Massalia) . . . . Ptolemy calls such cities . . . ‘foundations’ that should be used as reference points for developing the rest of his map.” No mention that all five “foundations” cities’ Geography latitudes are seriously wrong (rms 26° = ordmag 1°): errors −14°, −30°, −43°, −14°, −14° (mostly quarter-degree negative, from astronomers’ amateurish use of asymmetric gnomon). Meanwhile, statistical stellar analyses by Rawlins (Iis 1982; & DIO 1994, thrice cited in “Secrets”, which Shcheglov read), Y.Maeyama (Centaurus 1984), & J.Brandt (JAHH 2014) show that all 4 real, non-amateur Greek scientists cited above at [§G 3] knew their latitude to ordmag 1°. This twice-confirmed Rawlins discovery undoes Shcheglov’s entire inaccurate-geography thesis. He doesn’t mention it. Nor does he mention the contradiction it obviously creates versus the Geography’s mean latitude error of ordmag 1°. In response to the disjunct, one JHA Editorial Boredperson has offered that geographers must have ignored astronomers! (So, did astronomer-geographer Hipparchos ignore himself?) DR mathematically contends (“Achievement” pp.260-264) these litotheo-unexplained errors were instead from forced latitude-uniformization-herdings, for astrologers’ convenient access to tables at each key latitude or “klima” (for horoscopes’ Ascendant and other “house” boundaries: “Secrets”, eqs.2-3), corruption inconsistent with the astronomer-scientist Shcheglov sees Ptolemy as.


K Selling or owning klimata tables for every latitude-degree was impractically voluminous. [Thus, if Almajest’s 1°/4 klimata-interval was adopted, then each city whose longest-day was closer was 1°/8 to a klima was grouped under it, its latitude made equal to exactly that klima’s latitude.] Such groupings of cities under ONE latitude is explicitly attested at Geography 1.4.2 (even while justly criticized at ibid 8.1; 1 different authors, in all likelihood). Effects of such data-tampering are obvious from errors found in [§J’s sample], while rms error 2°. Some (as expected if due to real astronomers, like those of [§G 3]) 64°, −43°, −251°, −30°, −26°, 14°, −40°, −59°, −84°, −108°, 38°, 204°, 10°, 124°. Dropping −251° (confused Carthage mis-latitude: DIO 16 §3 fn 43; 2009), rms error is 93°; but the (more reliable) median is 59°, hinting both are skewed high by a few 50’s.

L Given this mess, one might ask: who says there ever were accurate ancient maps? We reply by turning to the same 1.4 latitude-accurate cities’ longitudes, and receive a shock. (Sample originally compiled in 1984 for another purpose so not prebiased for longitudes.)

M Shcheglov ignores that, besides 7/5, “Achievement” tests longitude-stretching by 4/3. Poseidonios is connected to 240000 stades by Kleomedes 1.10; 180000, by Strabo 2.2.2. Was the pre-stretch globe Poseidonios’? How fruitful is the 4/3-stretch theory?

N Dividing 4/3 into §J’s 14 Geography degree-longitudes vs Alexandria, to unstretch them: those 6 cities within 30° of Alexandria show rms longitude-error c.2°, or about half a degree. The other 8 cities, several of them ordmag 1000 miles from Alexandria, likewise show rms error 2°. Some (as expected if due to real astronomers, like those of [§G 3]) 8 4-hour-longitudes (some overlap with above sample), already published at ibid p.265, though neither the informatively small errors nor their implication is remarked by Shcheglov.

O Errors’ small size is apt (ibid p.258) to longitudes based on accurate eclipse timings. As is their remoteness-independence (§N), since the error in local-time difference for eclipse observers longitudinally 1° apart is no more or less accurate than for 100° apart. Which is why the unstratched 42° from Cartaghe to Persepolis is correct to ordmag 1°.

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 dodgolating longitudes by lumping them into discrete klima-cubbyholes where all cities in a cell are force-assigned the same latitude (§K; Geography 1.4.2); “Achievement” p.261; “Secrets” §D for handy astrologer-access to inevitably-too-widely-spaced 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 [II.1])

S Given those Almajest tables’ Mediterranean 1°/4 klima-spacing: we can compute that the forced longitude-shifts would, for flawless cubbyholing, theoretically produce 0°3/4 rms error, ordmag-consistent with the 59° median already found above (§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 rejects any implication by astronomers that Ptolemy’s occult profession lessens him. Ironically, this well-intended discipline has long blinded the eld 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-incipiently-cosmopolitan Serapic religion, in whose 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 determining latitude or “klima” (for horoscopes’ Ascendant and other “house” boundaries: “Secrets”, eqs.2-3), corruption inconsistent with the astronomer-scientist Shcheglov sees Ptolemy as.

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 astrologers 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 unmissably 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 = 256000 stades (later nudged to 252000, perhaps for 700 stades/degree-convenience).

[ii] Eusebius’ Sun-distance, 4080000 stades, is thus 100+ (Earth-radii), in the Aristarchos-
Archimedes-Hipparchos-Poseidonios tradition that too-big-for-precision Sun-distance is a power of 10: their 1000r or 10000r, likely origin of the very idea of order-of-magnitude.

By the correct (now generally-accepted, but still sniped-at) 185 meter stade, 256000; Poseidonios' & Geography's 180000 stades, given Editor H.F.Cohen's haughty rebuff (p.45), a Letter-to-the-Editor (pp.3-8 here), 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] ploed 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, unvary 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 coverupping 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 . . . about mere plagiarism, and miscomputed demeaning of accurate and scientifically referred [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 p.73 below, here] to a referee until he got an negative report safely in hand [a report again not evaluating any scholarship]. Does he imagine such transparent 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 appropriate credit for a (needlessly) rival journal’s discovery. If Isis does not acknowledge receipt of this letter . . . it will be reasonable for previously unenlightened observers to conclude that your society is unprincipled,3 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-Bablitch-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’m afraid I cannot help you out here. Sincerely yours, Lynn Nyhart

So: what exactly does HsS’ 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 doesn’t even care 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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3Cohen 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 chaunvist battery of destructive, data-disrespecting — even data-fudging — papers’.” (See [3 p.46 below].)

The Greatest Faker of Antiquity: Still Foolin’ ’Em

[On 2014/8/26&12/22, a somewhat restrained&amp;spare version, www.dioi.org/pf.pdf, of the following paper was submitted to the Journal of Astronomical History&amp;Heritage. Its referee, while admitting our obvious expertise, included personal remarks (fn 1) echoing religious Ptolemist O.Gingerich’s various past slanderous referee reports upon our work, naturally requesting removal of any imagery embarrassing to his clique, even offering to take another 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, revolving 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.dioi.org/oww31.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 cuts, 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 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 educative 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 Almagest is the central document (§31 below) of our valued heritage from classical antiquity’s mathematical astronomy. Though Princettine’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 Serapite state religion at Canopus (near Alexandria) where stood its major temple, which specialized in curing illnesses through astrology&amp;dreams. Ptolemy also authored astronomy’s bible, the Tetraëdros. His employers no doubt appreciated his consistent demonstrations that reality was in perfect accord (§2M) with divine celestial theories. However, for centuries, historically savvy astronomers have known that this famous 2nd century AD astrolger-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 deprecations even included stealing and mis-­precessing Hipparchos’ immoral 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 first time in history to predict star-­positions — to ordmag 1 accuracy! — 100° in advance (ibid Table 23: 100 select stars for 1701.03). But a few invincibly innocent & deeply committed (§B2; fn 11) archonal historians-of-astronomy keep intermittently trying to breathe life back into their longstanding tradition — e.g., Neugebauer 1975 p.284 & Pedersen...
date; A.Jones 2010A xi xii) that Ptolemy was actually an honest outdoor astronomer (even below, at [N3] rating Ptolemy a better observer than Hipparchos!); or at least (Schaefer 2013 p.47) that there is still a serious question about whether he observed outdoors: classic the-controversy-continues resort (latest example: fn 1), ever dear to those fearing faces-loss acts, such as shunning, has been nationally published (Schaefer 1999 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 moralistic and craniolithic insistence upon defensively maintaining — by character-assassination (fn 5) & the threat of exile (idem and fn 1) for dissenters — and unprofessional, deluding, perception-inverting (\$N), logic-doubling (\$S), holey-body honest-Ptolemy-myth as its ultimate herd-sacred tenet, can only weaken the field’s cred. Same for launching fantastic, irrelevant, and transparently projective descriptions of bemused skeptics as “angry” (Pedersen 1974 p.23) and “hitter” (Evans 1998 p.268). For which side is provably angry, see DIO 11.1 p.2; also the case of an eminent referee and Jesus-devotee (fn 5) who routinely (fn 1) calls today’s numerous (idem) Ptolemy-skeptics a tiny bunch of paranoia’s for believing what he himself privately knows5 to be true. Healthy restoration refeereeing (fn 3) but is alert to the need for being 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 §§7-8). That the controversy has been wise 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 moralistic and craniolithic insistence upon defensively maintaining — by character-assassination (fn 5) & the threat of exile (idem and fn 1) for dissenters — and unprofessional, deluding, perception-inverting (\$N), logic-doubling (\$S), holey-body honest-Ptolemy-myth as its ultimate herd-sacred tenet, can only weaken the field’s cred. Same for launching fantastic, irrelevant, and transparently projective descriptions of bemused skeptics as “angry” (Pedersen 1974 p.23) and “hitter” (Evans 1998 p.268). For which side is provably angry, see DIO 11.1 p.2; also the case of an eminent referee and Jesus-devotee (fn 5) who routinely (fn 1) calls today’s numerous (idem) Ptolemy-skeptics a tiny bunch of paranoia’s for believing what he himself privately knows5 to be true. Healthy restoration
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 despised 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 avoidance, is, ethically speaking . . . original. And, then, it’s always stimulating to be diagnosed as insane by one who has for 50+ led a crusade to convince the world [i] that an ancient who was off his latitude by 1°34′ and could not find the Sun within a degree, was a regularly observing astronomer, and [ii] that anyone who disagreed was the insane party (§B1). Equally risible: to be shrunk-analysed as paranoid by one who thinks the whole universe is run by an invisible mind and who publicly calls on “our Lord Jesus Christ.” A world where sanity is insanity and vice-versa. With respect to its obvious duty to return the Ptolemy controversy to a scientific basis — above politics, smearing, shunning religious devotion — the American Astronomical Society for a half-century hasn’t lifted a finger. But (fn 35) has generously given it to the skeptics. Soon after Gingrich had brought P.A.S.P. up to snuff on who is and isn’t reliable, Skeptical Inquirer Editor K.Frazier asked him to debate Rawlins on Ptolemy in Skmng; Gingrich (1978/2/2) explained his refusal by calling Rawlins “exceedingly paranoid” for “suggesting that a cabal has been suppressing the consideration of [R.N.]Newton’s work” on Ptolemy. When finding that Frazier had sent a copy of this helpful character-profile to Rawlins, Gingrich got miffed at Frazier! (1978/6/2 letter) — so exalted is his permanent mental state, assured of ontological immunity from the ethical standards of ordinary mortals, sinless by very definition. Like his image of his inherently inexcusable ancient astronomer-hero. E.g. only such an elevated being could — without any sense of hypocrisy, of damage to knowledge, or of harm to the slanderer — accuse someone of insubstantiality even when the insubstantiality is in his own (or his editor’s) every word. At the head of this note). At Gingrich’s insistence, U. Notre Dame’s 1999/75 debates on whether Ptolemy observed or stole the Ancient Star Catalog could not be held unless Rawlins was kept from the platform. Evidently inspired, by decades of toadily awesome H.A.D. worship, to standards of equity and consistency incomprehensible to the less godly (and strengthened by the surety with which he can count on fellow poles’ reverent silence on his secret actions, some even going so far as to speculate that the world has been airbrushed from being aired in the same breath as Retrospective), Gingrich in a 2000 referee report to Isis on an article (Thurston 2002S) appreciative of Rawlins’ inductive successes, typically promoted freepitch-in-theory while inserting an element aimed at ensuring that heretics’ Irresponsible abuse of freedom would be properly characterized as what could only issue from a disordered mind: “To say that the paper should not be published will only fuel the paranoic claims [fn 1] that group-thinking Ptolemy, who believe that a cabal of [Neugebauer] ‘nutra’ [sic] are preventing from being aired in the name of, I am sure, would have received another paranoid’s letter regarding said zany’s own recent book-review (emphasis added): “So far the Neugebauer camp has not been heard from. Perhaps my merely mentioning [R.N.]Newton in a review of Neugebauer has placed me beyond speaking terms.” (See www.dioi.org/pmi1.htm.) And who was this fellow-paranoid, who imagined a closed Neugebauer “cabal”? No other than O.Gingerich! In the midst of decades of documented — ah — “inconsistent” character-assassination fantasy, Gingerich adds one more fantasy, soberly describing herself as “a practicing Christian”: 1978/2/2 to Frazier. Obviously, being religious doesn’t mend vendettas. We recall gullible or cynical saint-mythologizer Cardinal John Henry Newman’s politically deft holy war on straightforwardly ethical independent idealist Chas. Kingsley; at Newman’s 1890 death, after the standard public eulogy, Cardinal Henry Edward Manning privately mourned his colleague thusly: “Poor Newman. Poor Newman. He was a great man.” (Studium, 1918, Manning chapter, end of part 9.)

6 DIO has long had a standing offer to publish debates, with quite novel rules, www.dioi.org/deb.htm, scrupulously designed to ensure fairness. [Except for inspiring a contemptuous joke from A.Jones, these rules — and DIO’s generosity in offering debate-space — have proven of no interest to Mufosi.] Such openness to airing the findings of what the referee calls “those with different views” (fn 1) contrasts revealingly with the degree of dissent-tolerated exhibition by the referee’s own circle.

7 Rawlins 1982C p.362 proves that Pedersen positively knew of Delambre’s crucial discovery (§K1), but chose not to impart it to his readers. [See §3 fn 121.]

8 Demonstrating how one earns a MacArthur in certain sub-fields, the principled Dr.Swerdlow has not only called R.N. Newton dishonest and kook (§B2), he has additionally published a Joe-McCarthiesque incompetency-accusation against him (quoted verbatim at R.Newton 1991 §E2), claiming lots of thesis-gutting mathematical errors by Newton here-in-hand without giving a single valid example. (Check the revealing cavil-count context data carefully detailed at ibid fn 6.) And the Journal for the Advancement of Science Editor — before publishing his personal polemic, for personal prevarication, Gingerich (1978/2/2) to Frazier, gingerly phrased: “If this paper is published in the midst of decades of documented — ah — “inconsistent” character-assassination fantasy, Gingerich adds one more fantasy, soberly describing herself as “a practicing Christian”: 1978/2/2 to Frazier. Obviously, being religious doesn’t mend vendettas. We recall gullible or cynical saint-mythologizer Cardinal John Henry Newman’s politically deft holy war on straightforwardly ethical independent idealist Chas. Kingsley; at Newman’s 1890 death, after the standard public eulogy, Cardinal Henry Edward Manning privately mourned his colleague thusly: “Poor Newman. Poor Newman. He was a great man.” (Studium, 1918, Manning chapter, end of part 9.)

9 Ptolemy’s ‘con-man’ (see DIO 1.1 §3) was Gingerich 1978/2/2 to Frazier. And the Journal for the Advancement of Science Editor — before publishing his personal polemic, for personal prevarication, Gingerich (1978/2/2) to Frazier, gingerly phrased: “If this paper is published...”

10 DIO has long had a standing offer to publish debates, with quite novel rules, www.dioi.org/deb.htm, scrupulously designed to ensure fairness. [Except for inspiring a contemptuous joke from A.Jones, these rules — and DIO’s generosity in offering debate-space — have proven of no interest to Mufosi.] Such openness to airing the findings of what the referee calls “those with different views” (fn 1) contrasts revealingly with the degree of dissent-tolerated exhibition by the referee’s own circle.

11 Rawlins 1982C p.362 proves that Pedersen positively knew of Delambre’s crucial discovery (§K1), but chose not to impart it to his readers. [See §3 fn 121.]
bunker — to admit error in the slightest degree.\textsuperscript{10}

\textbf{B3} Notable features of modern Ptolemites are: \textit{a} Consistent preference (fn 18&50; [N15] for the inherently unlikely\textsuperscript{11} over the inherently likely. \textit{b} Failure to notice that Ptolemy's fraudulence is a single simple theory which does so. \textit{B3} Notable features of modern Ptolemists are: \textit{a} Consistent preference (fn 18&50; [N15] for the inherently likely. \textit{b} Failure to notice that Ptolemy's fraudulence is that single simple theory which does so.

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.doi.org/10.1016/0012-3832, DIO 11.2 (2003) [and p.30, p.33 items 5&6]. We await the century when Schwab or Gingerich can display in print the same humility as the "pompous" (fn 1) skeptic side.

11 For examples of downright embarrassingly improbable apology 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 Schwab 1992 p.177, attempting evasion of the fractional-endings argument (R.Newton 1977 pp.245f). 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 astrolabe'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 [§H1] from 1°2/3 to 3°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 §§4. As one encounters no less than 64 pages of such desperate resorates, 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 §F.) For further imaginative excursions, see (Gingerich 1976 in "Science" "On Ptolemy as the Greatest Astronomer of Antiquity" and (§M2) Scientific American, 1979, "The Fraudulent Ptolemy," since these no-workshop-singly-titled Ptolemy-advocates, 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-2002 Feb. The vaunted Free Press at work.) Extra community-embarrassments: massive double-Pb-paper Evans 1987 (below, fn 47); top Newton-exilerer (below, fn 35) Schwab 1989, on whose invincible math-innocence (repeated 1979, 1981, 1989, 2010 while reacting for Ptolemy-eculations), see above, at fn 8 of Rawlins 2018W §F34b. Two more cases of scientself-righteously-vindicated solstice-denier Schwab's delusional argument (§N7 below): [1] The instrumental (and historical) misconception of Jones 2002E p.16, that ancients found geographical latitude \textit{L} from observations at equinox. (Contra chapter-one Ptolemy, Alm. 1:12; see Rawlins 2005§F2-F3 & eq.[3] ) [2] Evans 1998 p.206 explicitly echoes Schwab's highschool incomprehension, claiming a transit instrument "could not determine the time of solstice very precisely, owing to the [gradual] nature of the solstice itself. More reliable for this purpose were the times of the equinoxes." Aside from his clique's instrumental blind-spot ([N7]: is Evans even aware (as is, e.g., Toomer) that all outdoor astronomers' extant ancient yearlengths were solstice-based? — Meton (Alm 3:1), Euktemon (ident), Kalloppos (Toomer 1984 pp.1L24, fn 72; Rawlins 1991W §K[4], Aristarchos (§N18 below), Hipparchos (§N19 below). (Recoverable Greek astronomers' rms errors range from \(\pm 30\) to \(\pm 45\)”) The same ratio for Hipparchos' solar observations (Alm 3.1) are on average about 50 times nearer\textsuperscript{15} to his indoor tables than to the real outdoor sky create Reasonable Doubt, Rawlins 1992V §C20 responded: "Besides a range of specific evidence of plagiarism, there is the simplicity of that hypothesis' fit to the larger evidential situation: if we merely assume that Ptolemy swiped the Catalog, virtually all of [defenders'] central purported 'Equivocal' (ibid. §C22) of the case immediately evaporate." [Ptolemites are hereby asked to specify any that don't.]\textsuperscript{14}

13 To the attempts of Schwab 1992 ("The Enigma of Ptolemy's Catalogue of Stars") to throw the usual flock of disconnected alibis at skeptics, in order to — like any other criminal lawyer — that explains multiple features of the available evidence. Ptolemy's fraudulence is that single simple theory which does so.

\textbf{B4 } NB: In case a response to this paper appears later, look carefully to see if it deals with all nine ([§C-I-K-L] of our proofs of Ptolemy's dishonesty hereabouts (a display which would hopefully alert defendants' alibi-incoherence: §B3 and most especially Rawlins 1992V §§C31-C32) or more likely instead just tries either [i] to claim that faking data and stealing stars isn't dishonest (§§B2 and H2), or [ii] to extrapolate-concoct blanket-rejection of doubt by attempting improbable maybe-coulda-happened theories (fn 11) for one or two proofs, before changing the subject, to divert from the Occamite power of the single obvious solution to all nine.

\textbf{B5 } Ptolemit cultism requires not only invincible innocence of the plain implications of R.R.Newton's sophisticated, epochal opus, The Crime of Claudius Ptolemy (R.Newton 1977; valuably precised by Thurston 1998A) — but also of various lesser-known yet shockingly clear points that leave no reasonable doubt. These follow.

\textbf{C VENUS versus VENUS (AND THE VENUS-MADE-ME-DO-IT DEFENSE)

\textbf{C1} The Greatest Astronomer of Antiquity's four solar "observations" (Alm 3.1 and 7) are on average about 50 times nearer\textsuperscript{15} to his indoor tables than to the real outdoor sky
(errors displayed at Thurston 1998A Table 1): the mean of the error-sinosoid that best fits the tables, and thus very closely fits the “observations,” exceeds a full degree: −65°. (See formula at ibid \(\odot 1\).) This, though naked-eye solar measures can be made to ordm 1’ (fn 47; Rawlins 2018U §B4). Either Ptolemy’s genius was so preternaturally refined that he could look up into the sky and (fn 18) see objects’ theoretical positions instead of their actual positions\(^\text{16}\) or (§B2) he has broken the law of empirical science by pretending to base theory on outdoor celestial “observations” actually computed, indoors on the sky, from or to fit the very same theory (Thurston 1994P [D & Rawlins 2002V §B3-B5], such fakes then used to “prove” said theory, a (literally) preposterous sham-process as Delambre saw: §B2.

D2 To appreciate the grossness of the illegality here, consider its sheer enormity (as emplung on sun, and planetary contexts throughout R.Newton 1977”, for his 3 equinoxes, Ptolemy is alleging 1°-hand visual sightings of the Sun’s center on the celestial Equator at times agreeing to ordm 1’ with indoor theory — when in truth NO PART of the real solar disk was on the outdoor-sky Equator at any of these three times. For his two Autumn “equinox” times, the real Equator was 34’ north of the solar center, i.e., over twice as far from it as was the Sun’s own limb! (The angular radius of the Sun is 16’.) Understand that, while these errors’ ridiculous grossness alone proves fraud, we additionally know EXACTLY (§D6 below) the method of all four fabrications, though Ptolemy presented each as an outdoor observation of the real sky, not a mere indoor-calculation.

NB: No cultist admits that Ptolemy did anything dishonest here. D3 Further, the tabular Sun’s −65° mean longitudinal error (§D1) at the epoch of Ptolemy’s tables, 137.547 (Antoninus Pius Year 1 Thoth 1 [137/7/20] Alexandria Apparent Noon), isn’t a constant in time: it varies by −23°/cy. (A rate consistently bi-miscomputed at CalTech’s Swendlow 2010 p.152, item 3.) So: when is Ptolemy’s Sun correct? Obviously that epoch must be 137.547 + (100°/cy)65’/−23’/cy) = −145, which (as seen at Rawlins 1991W §M6; similarly below at §D5) is Hipparchos’ era, and is indeed the reginal epoch (Ptolemy VII Physkon Year 1) of — and the time of creation (Rawlins 2018U §O) and launch of — his Prime solar orbit (dubbed “PH” at idem). How can Ptolemy’s defenders expect to credibly deny that he plagiarized from Hipparchos, when Ptolemy’s Sun and thus his entire longitudinally contingent celestial system (not just the Sun but the Moon, planets, and stars) is correct only for the time of Hipparchos?

D4 The Hipparchos-Ptolemy tables’ −65° mean solar longitudinal error at 137 AD is well known, but the following revealing point isn’t: the errors of Ptolemy’s “observations” also mimic the Hipparchos PH solar model’s big 0°-4-amplitude annual period! error. (Again: see error formula for Ptolemy’s Sun at Thurston 1998A \(\odot 1\).) For Ptolemy’s mimicry of not just systematic but even random Hipparchos error, see the glaring case of Arcturus: fn 37.

D5 This unsubtle echo connects to the irony that Ptolemy didn’t fake the solar data via tables but by even cruder means. (He fabricated similarly elsewhere as well: Venus \(\sqsubset C1;\) Rawlins 1991W fn 166), the stars [fn 37], and the Arbelia eclipse \(\sqsubset G3\).) As early as 1639 — the year the Ptolemy controversy should have ended (or been doomed to never begin?) — Christian Severin said Ptolemy had merely computed the alleged solar observations from Hipparchan data, and Delambre 1819 (pp.lxiv-lxix) explained in detail how Ptolemy had fabricated his solstice and equinoxes by merely adding integral numbers of Hipparchan years to Hipparchos’ observations of solstice and equinoxes. (Discussion: Thurston 1994P p.58; and Thurston 1998A §§A.KS.) For example (by method of ibid \(\odot 2\), reconstructing individually Ptolemy’s 139 AD Autumn Equinox): to recover Ptolemy’s 132 AD Autumn Equinox (Alm 3.7), just add 278 Hipparchan years (365°1/4 + 1/300 each, 6° longer than has been claimed, e.g., in \(\text{The Accuqiall}(\$M). And in Hipparchos’ case, a ratio something above unity is to be expected since the tables were, after all, based upon his slightly flawed observations, of which he \(\text{contra same delusion Alcuqiall}\) reports several discordant with theory & each other: \(\text{[fn 8}.\)

15 Ptolemy didn’t have to look skyward to see theoretical data instead of real: his Optics takes perfectly false-theory-accordant refraction angles that are erroneous by up to 2°/12 or 150 arcmin. (See, e.g., Neugebauer 1975 pp.895-896.)

16 Go to New York Times’ Science’s 2009/9/8 exarn of a century of establishment promotion of another scientifically unverified myth, a study in celestial immunity to oncoming evidence, analysed in the context of other DIO-shunning bad-loser cults. Previous day’s online edition: http://tierneylab.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 Grabholf’s analyses.

17 From chats with Ptolemist historians for 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 solar “observations” (pretty dumb, since the system depended on the Sun: §D3), so he was stuck with the error and decided [see Ragep at \(\text{[fn 9}\)] not to re-do his whole scheme. (How is this a defense — or been doomed to never begin?) — the year the Ptolemy controversy should have ended (or been doomed to never begin?) — Christian Severin said Ptolemy had merely computed the alleged solar observations from Hipparchan data, and Delambre 1819 (pp.lxiv-lxix) explained in detail how Ptolemy had fabricated his solstice and equinoxes by merely adding integral numbers of Hipparchan years to Hipparchos’ observations of solstice and equinoxes. (Discussion: Thurston 1994P p.58; and Thurston 1998A §§A.KS.) For example (by method of ibid \(\odot 2\), reconstructing individually Ptolemy’s 139 AD Autumn Equinox): to recover Ptolemy’s 132 AD Autumn Equinox (Alm 3.7), just add 278 Hipparchan years (365°1/4 + 1/300 each, 6° longer than has been claimed, e.g., in \(\text{The Accuqiall}(\$M). And in Hipparchos’ case, a ratio something above unity is to be expected since the tables were, after all, based upon his slightly flawed observations, of which he \(\text{contra same delusion Alcuqiall}\) reports several discordant with theory & each other: \(\text{[fn 8}.\)

18 In Ptolemy’s case, a ratio something above unity is to be expected since the tables were, after all, based upon his slightly flawed observations, of which he \(\text{contra same delusion Alcuqiall}\) reports several discordant with theory & each other: \(\text{[fn 8}.\)
Moreover, such an error (see math of inconsistent with each other, and [2] even more seriously false in both instances (by −14′ and −8′, respectively). In other words, an emphatic reversal 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 (§E1) — 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 that (in 1 AD) public miles (≠ nautical miles) distant Pharos flame was slightly over thirty degrees south of due west, so the two sites’ L could not possibly be the same. (Real L difference: 12°-sin30° = 6°.) Further evidence that Ptolemy “doesn’t seem to have allowed his eyeballs out at night” (Rawlins 1985G p.266).

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, written some years before the Alm (as proven in the brilliant paper, Hamilton, Swerdlow, & Toomer 1987), listed precisely the same Mercury mean motion, but most of the other elements differed. So: how could the same mean motion have been empirically and mathematically based for both works, if the respective derivations involved disreputable elements? E.g., deriving the 139 AD position for the Canobic Inscription elements versus doing so using the Alm elements, produces results disagreeing by over 5°. Yet The Greatest Astronomer of Antiquity gives the identical mean motion in both works, to six sexagesimal places, and this discrepancy: Rawlins 1987 p.236-237.

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 light of such sloppy-copy, one can only admire Dennis Duke’s witty new translation of the Almajest’s Greek title, Synaxis (§3 fn 15), as: Cut&Paste. Not in Liddell-Scott-Jones. Yet.

23 In light of such sloppy-copy, one can only admire Dennis Duke’s witty new translation of the Almajest’s Greek title, Synaxis (§3 fn 15), as: Cut&Paste. Not in Liddell-Scott-Jones. Yet.
foreign eclipse times couldn’t have supported the longitudinally-stretched geography (§G4) he borrowed (with credit)\textsuperscript{25} from Marinus of Tyre. Instead, Ptolemy’s vast opus provides (GD 1.4.2) but one\textsuperscript{26} 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 Lilybaenum) 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 calculations\textsuperscript{27} show that non-astronomer Pliny was quite accurate, while The Greatest Astraronomer of Antiquity was amazingly wrong, over 2° off for Carthage, 3° off for Arbela. The former error nearly equals the entire actual 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 disastrious 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 (”exorienis”). By contrast, the Arbela time is given as the “2°23” 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, “seunda hora” appears nearer in the sentence to “Sicilia” than to “Arbelam”. This obviously suggests Ptolemy used Pliny or his source but (evidently unable to read Latin well) took Pliny’s 20°23 as time for Carthage’s.

G4 But how did Ptolemy arrive at 23° for Arbela? Since Gossellin 1790, it has been obvious that multiplication by an expansion factor (Diller 1984 §5) has been applied by Marinus 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 241/4° 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 engaged.

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: §§4; §1 §3; Rawlins 2008Q §1 & Rawlins 2008S fnn 13&45) that GD fatefully corrupted an accurate prior map by expanding its longitudes by a factor of 30%–40%. Collecting §G&C&E with the present case, we now have 3 separate Ptolemy double-false frames on display here.

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

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

\textsuperscript{26} Due to modern communal non-recognition of occultist Ptolemy’s isolation from actual scientists, we find Neugebauer 1975 (pp.363, 367, 522) cornered into interpreting Ptolemy’s non-use of contemporaneous eclipses as having to mean that (www.dioi.org/ct.html#cknh) there then existed no empirical scientific community to be isolated from!

\textsuperscript{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, Lilybaenum 17:52, Gaugamela 19:56, Arbela 19:58. So the Gaugamela-Lilybaenum difference in geographical longitude \( E = \Delta E = 2^{h}04^m \); Gaugamela-Carthage, 2^{h}13^m.\n
rotates about the equatorial not ecliptical pole: as we can see from, e.g., the educational paper model Evans has helpfully disseminated, so (Rawlins 1982C §361 & Fig.2) we’d find error waves of amplitude 1°22 in the Catalog’s latitudes \( \beta \) (cosine waves: \( \beta ibid \) & northern longitudes \( \lambda \) (sine waves: \( \beta ibid \) eq.3). We don’t. (Amusing details at Rawlins 1992V §§ C13-C15 & fn 31). See also the inspired findings of Graßhoff 1990 — which instantly converted dedicated and scholarly Ptolemist G.Toomer — as well as the perceptions of Duke 2002C, all of which combine to show that, e.g., errors in Hipparchos’s stars are statistically quite discernable in the Alm’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 If this became obvious c.1990, even to the most religious, that many Ptolemy stars were Hipparchian. However, no archon was ever going to admit in print the plain truth: the establishment had been blinded 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

\textsuperscript{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 Swardlow. (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 burnings on 1998 and 2001-2005 attacks on Rawlins on his non-deductions on Ptolemy’s isolation from actual scientists. The quietety here may actually be deliberate.) Selecting boardmembers on such criteria will damage mean-IQ atop JHA for decades to come.

\textsuperscript{29} D.Duke’s statistical studies indicate that very nearly all stars were appropriated. If Evans and Schaefer were right that Ptolemy observed a substantial section of the Catalog, then the error-correlation dot-diagrams of Graßhoff 1990 would exhibit an obviously disjunct mix of superposed shapes: circular (stars observed afresh) and elliptical (stars copied from Hipparchos). But the diagrams are instead just elliptical. (Even if otherwise, this would prove only that someone other than Hipparchos — not necessarily Ptolemy — observed the stars whose dots mapped circularly.
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 claim, at Alm 7.4, in lengthy detail, falsely saying he observed every visible star (§K1). Bottom lines: [i] The JHA committed 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” JH-A-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 (B1 & Fig.1 points out a history-of-science-ignored ultra-simplistic disproof of Ptolemy’s Catalog authorship (Rawlins 2000A fn 177): the 5° gap which should exist between the antarctic circles 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 antarctic-circle 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 = 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: JEKYL’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 §B4.) 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°2/3, 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’, 0° endings became 40°; 10° became 50°; 15° became 55° and were rounded to 00’; 20° became 05’; 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 observability (Rawlins 1982C n.3). Schaefer’s dodge ([H2 item 2]) was just the latest in the superficial tradition of evading facing skeptics’ vindication. One even accent Ptolemy’s use of the word “we” when describing purported 1°-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.

I3 Most critiques of Ptolemy’s chicanery point primarily to the excess of 40° endings (vs 0° endings) in the Catalog longitudes λ, 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’ λ, these were 30° and 50° endings, respectively.) Looking naively at the tabular distribution, the fact that 30° are the least frequent endings is bizarre, since 30° is nearly the most frequent latitude ending, as it should be. Indeed, for the latitudes β, 30° are roughly twice as common as 10°’s, but this is reversed for the longitudes λ, by far the strongest confirmation of R.Newton’s hypothesis for explaining the longitudes’ odd fractional-endings distribution.

I4 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’ and 20’ endings, respectively. That is, the Greatest Astronomer of Antiquity deliberately and permanently destroyed data in a legendary work, just to cover his tracks in a theft. Thus, we cannot now tell whether a Ptolemy stellar longitude with a 00’ ending was 15° or 20’ in Hipparchos’ catalog; likewise for a Ptolemy 20° ending, where we cannot know whether it was 40° or 45° for Hipparchos.

I5 So R.Newton ([J2] explained why longitudes λ are near-befit of 15° and 45°’s (only 32° in all: five 15°’s, no 45°’s), though appearing with roughly expected frequency for latitudes β. twenty-eight times more often than for λ.

I6 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 Rhodos Island, Cape Prassonesi (latitude L = 35°53’N), but not with Alexandria’s L = 31°12’N. [B] Shevchenko 1990 p.194 discovered for a specified half of the zodiac, stars’ λ exhibited no particular excess of 40°’s. Later, DIO 10 (2000) fn 177 tested Gem-to-Sgr (roughly Shevchenko’s range) and found that — excepting Sco, whose prime stars’ β reach atypically far south for the zodiac — these stars’ original Hipparchos 00’-excesses were not in ecliptical longitudes but in polar longitudes. The novel and insuperable impediment thus created for Ptolemy’s defenders is found in the footnote cited.

I7 The root, of the persistence of the embarrassingly-long (given the evidence’s imbalance) “debate” over the Ancient Star Catalog, is that sneakily (ibid [ibid]) 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 Rhodos Island, Cape Prassonesi (latitude L = 35°53’N), but not with Alexandria’s L = 31°12’N. [B] Shevchenko 1990 p.194 discovered for a specified half of the zodiac, stars’ λ exhibited no particular excess of 40°’s. Later, DIO 10 (2000) fn 177 tested Gem-to-Sgr (roughly Shevchenko’s range) and found that — excepting Sco, whose prime stars’ β reach atypically far south for the zodiac — these stars’ original Hipparchos 00’-excesses were not in ecliptical longitudes but in polar longitudes. The novel and insuperable impediment thus created for Ptolemy’s defenders is found in the footnote cited.

I8 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.

I9 A different defense tactic goes the you’re-another-route (earlier variant at [H2]), citing “other” scientists than Ptolemy who fudged data (confiling their occasional over-optimism with Ptolemy’s flagrantly consistent M.O.). But, again, among these, only our Greatest Astronomer of Antiquity ever stole a thousand stars — the factor that (as in §J8) separates the “mercy” Fudge from the naked thief.

32 All 5 stars with 1°7/4 longitude endings are ecliptical and are that rarity (like Tau Boötes) not copied from Hipparchos. DIO found that these 5 oddballs’ conjunctive sources (Rawlins 1992V fn 20 and Rawlins 1994C fn 108) were three lunar eclipses (Babylon, Hipparchos, and perhaps Menelaos) and twice Venus.
**J CONCLUSIONS**

**J1** The *Alm* is an invaluable resource, our only connexion to much of high ancient Greek astronomy. Given that *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 tenuous state of the modern history of ancient astronomy community, is this: its fiscal rulership can read all that you have just been quoted, though finding not a digit out of place in the ancient manuscripts! Or (A) Ptolemy has eons utterly nothing do with it [B] R.D. should continue to be non-cited for (1) his witchcraft (fn 35) in co-hypnotizing scholars (fn 1) to realizing Ptolemy cheated, & [2] exposing the vile tactics of archons' who'll never admit they were wrong to slander R.Newton before even understanding his evidence.

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**K APPENDIX I: GRUSOE 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-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 radically 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 constellator 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 *visible stars*..." We have been in your shoes, too, but returned to 1st principles, discovering that all these unCataloged stars were invisible to Hipparchos! (fn 23).

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**K2** The 1st time cultists trying to save Ptolemy (or pan-Babylonianism): [*§N13*] must resort to spurning Ptolemy's own claims (Rawlins 20020H fn 274). Just as Gingerich did with R.Newton forty-seven years ago [now 50+! — details at Rawlins 1994S §B13. After all: must protect even 'till-now-undeleted Thailand and the antipodean Land-of-Oztrollya ([§D2-§A2] from the full truth about the integrity & ability of Ptolemy and his too-too-far-away-legendary organ.

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**K3** Among *JHA*-circle-hed talk-trolls commenting toward R.Newton and his solid mathematical analyses having become, well, "incompetent" ([A.Aaboe], "up a pole" [J.Field]), "disreputable" (Toomer 1984 p.viii), "silly" (E.H. Grierson), "unbelievable" (J.D. Evans), "illiterate" (Gingerich 1984 p.viii), and (all Swerdlow) "impossible" ([A.A. Allen], "sick" [J.D. Evans]), R.Newton 1977 pp.220-225 realized that Ptolemy had typically (above, [*§C7*]) that data Ptolemy reports from others are faked though (as [*§D2*]) must resort to "unanticipated by..." the Junior Hellenic Astronomers (rawlins 1997 pp.220-225) that Ptolemy had typically (above, [*§B2*]) just-indoors the SickSix declinations from his 1° cycle theory — and then turned around to "prove" 1°/cy precession from said fakes. The 2004 paper instead conjectures that Ptolemy quietly stole stars from a Lone-Mystery-Observed of 57 AD, though the proposed LMO is incited by Ptolemy or any other ancient, so its 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 Ptolemyists fumble-law-fuying to refute one of the hero's crimes while not remarking or even noticing that they're simultaneously stimulating to another! And, again-typically (above, [*§B3*], choosing improbable theory over probable. Unconsidered question, quite aside from the issue of theft: from where (and with what degree of inexusable ignorance) would Ptolemy (uniquely, even for him) acquire — steal — a small set of star-declinations obsolete (fn 20) or overly-untrustful (fn 5) or both, has heretofore seemed just too outré for non-scientists and writers (unfamiliar with the JHAD) to believe, thus popular media (if we consider the 5°-fold persistence of the SickSix declinations from his 1°-cy declinations — and then turned around to "prove" 1°/cy precession from said fakes. The 2004 paper instead conjectures that Ptolemy quietly stole stars from a Lone-Mystery-Observed of 57 AD, though the proposed LMO is incited by Ptolemy or any other ancient, so its 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 Ptolemyists fumble-law-fuying to refute one of the hero's crimes while not remarking or even noticing that they're simultaneously stimulating to another! And, again-typically (above, [*§B3*], choosing improbable theory over probable. Unconsidered question, quite aside from the issue of theft: from where (and with what degree of inexusable ignorance) would Ptolemy (uniquely, even for him) acquire — steal — a small set of star-declinations obsolete by a century (fn 20) while purporting that he observed them himself (Almacest 7.3), and insert them into a set of contemporary declinations? This is proposed as a central new theory in a DEFENSE of Ptolemy's honesty? Innocently unanticipated 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 independent evidence that the real (fn 5) or borderline-red-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-big-picture
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: Algμα 7.5-8.1 lists dozens of “informaetae” stars which are in the vicinity of traditional constellations though still outside them, but which nonetheless appends to them. This includes even 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 (but bright enough to acknowledge): ε Car [m = 1.9] is less than 6º from Cataloged δ Vel: PK886), despite being easily visible from Ptolemy-era Alexandria (pointing-arc 1st-magnitude magnitude-μ ranging between 1.7 and 5; Rawlins 1982C Table 3), though not from Hipparchos’ Rhodes; since all were (see idem) of such dim μ as to be beyond Hipparchos’ in-practice mean magnitude limitμ0 for capture. Even more peculiarly absent from the Alm catalog are α and β Gru. Both of pre-extinction magnitude m ≈ 2 — i.e., of Big Dipper prominence! — and quite visible (§K2) to Ptolemy at μ about 3 and 4, resp (though at all hours below Hippparchos’ horizon), they could have just been set aside as a new9 constellation. After all, [i] There already was a two-star constellation, CMI (PK847-848); [ii] Ptolemy was inventor of the new9 asterism Antinous, which he formed c.130 AD from six9 stars “around” Βql (Toomer 1984 p.357).

The Greatest Faker of Antiquity

L. 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: 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 at that far north of the thief. Note: no other original naked-eye 1000-star catalog’s lowest star was as high as 3º: Hipparchos, Ulug Beg, Tycho, Hevelius. That is, Ptolemy’s “entirely normal” lowest star’s 6º-plus altitude is more than double the altitude of anyone else’s lowest star.

L2 A passing alibi by Evans 1987 p.166 even imaginatively hints that perhaps there were, say, rocks just-south of Ptolemy’s putative observatory that just-so-happened to block just-enough southern stars so as to make his putative observations’ declination-range deceptively look22 as if the observer were at Hipparchos’ latitude L rather than of where Ptolemy’s...
defenders argue he really, really might’ve been.

L3 Hmm. Why do partisans allow their enthusiasm to proffer already-vulnerable-enough arguments without even testing them? Here, one need only, both for Hipparchos’ and for Ptolemy’s epoch and latitude, list the sky’s stars (bright enough to be clearly identifiable in the Catalog) in order of [a] post-extinction magnitude \( m \) and [b] apparent altitude \( h \) above the horizon. If, in Ptolemy’s list [b], all the stars above \( h = 6^\circ \) are in the Catalog while all below are not, then the rocks aren’t in the apologist’s head but actually existed. Yet, test [b] fails. (For both ancients.) By contrast, adopting an atmospheric opacity appropriate to the best nights (when else would one search for dim stars?) near Rhodes’ southern tip, Cape Prassonesi (see fn 42 for geographical latitude \( L \) and height \( z \) above sea level), Hipparchos’ list [a] exhibits a startlingly clear36 split at a post-extinction magnitude \( m_\alpha \), slightly less bright than 5 (obviously his effective limit for capture): the stars dimmer than \( m_\alpha \) are not in the Catalog, while those brighter than \( m_\alpha \) are.44 Comparing these sensible results, to those gotten from applying the same Magnitude Split Test (DIO 9.1 1999 p.2) to the Greatest Astronomer of Antiquity’s Alexandria, will (fn 43) give any scientist a hearty upcackle.

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 pretender as the “Greatest Astronomer of Antiquity”? [a] Were public attacks on a famous scientist resented by science’s politicians as endangering science funding?45 — but 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 a new and rare misrecognition by B. Goldstein, we now have the fact (Rawlins 1993D §L8) that Ptolemy did so in so many words that 1° 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 1° magnitude stars’ visibility on-horizont (ibid §L8) ancients’ eyesight perceiving 12° magnitude stars (\( m = 12 \)); and so Ptolemy’s pre-extinction stars similarly entail a (at) \( m = 14^\circ \) magnitude (\( m_\mu = 14^\circ \)). So, why didn’t Archimedes beat Clyde Tombaugh to the discovery of Pluto?

36 Define split-Vagueness V in §3.3’s list [a]: dimmest Cataloged star’s \( m \) minus brightest non-Cataloged star’s \( m_\nu \). Testing Hipparchos’ \( V \) (\( \gamma \) Ara vs \( \epsilon \) Cru) at Cape Prassonesi (height \( z = c.200^\circ \text{m above sealevel} \)) for five assessed orbital magnitudes: 0.14 mags/atm (negligible aerosols), 0.15 (Rawlins 1982C), 0.17 (Evans 1987, #2), 0.20 (Evans 1987, #1), 0.23 (Schaefer 2001): \( V = 1/5, 1/4, 1/3, 1/2, 3/5 \), respectively.

37 Compare seal-eval- Alexandria’s Ptolemy’s §3.3 list [a]: \( V = 0.15 \text{ mags/atm, } V = 5/4 (\beta1 \text{ Sgr vs } \alpha \text{ Gru}) \). And for mags/atm = 0.23, 0.3: \( V = 4/5, 3/4, \text{ resp } (\gamma \text{ Ara vs } \alpha \text{ Phe in both cases}.)

38 Among those tested by Rawlins 1982C Table II, the only major star (\( m < 3 \)) that seems unambiguously to be missing from the Catalog is \( \nu \) Vel (\( m = 2.7; \mu = 3.1/4 \) for Hipparchos, 3 1/5 for Ptolemy). A speculation at DIO 4.3 §14 showed how star PK964 could be a mangled version of a position originally based upon an hypothetical observation of \( \nu \) Vel, high by \( 1/4^\circ \) in R.A. (3° great-circle), but in declination fully accurate to ancient precision.

39 The half-century Ptolemy Controversy should have been over in half an hour — had all participants amiably cooperated at the outset and sat down together to compare data and enlighten each other. Had defenders realized 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 astronomers’ 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 goonies). 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

[b] Were Ptolemy’s math proofs so admirable that it seemed incredible for him to have plagiarized data? — but (Rawlins 2003X p.502): what if he plagiarized the math, too? It was long believed that the Almajest’s spherical trigonometry proofs were original — until 1901, when it was found that they were taken from Menelaos (c.100 AD): Pedersen 1974 p.73 n.9. [c] Inevitably-feelable attempts to logically back up prominently published one-sided (fn 11) salesmanship, pushing Ptolemy as The-Greatest, put one in mind of Aquinas’ voluminous Reformation-germinating mistake of trying to defend by reason that which cannot be defended by reason. [d] Did damage to Ptolemy’s sacred-grant-cow value trigger the stunning (§B of R.Newton’s valuable insights? With the Almajest as [i] the central surviving ancient work on mathematical astronomy and [ii] suffused with fraud, grant-raising problem was presumably feared (perhaps needlessly: §J1), leading to attacks on Newton, but (far more tragic and longstanding) promotion of a now-widely-accepted misperception of all of ancient astronomy — just to cover for Ptolemy’s fudges, by deliberately (fn 46), falsely claiming that everybody-did-it (§M2) in antiquity — a distortion that’s gatewayed a 180° inversion of truth for a range of ancient-science issues, as detailed at §N, below.

Above option [d] was the most likely place for the original flame of rage at Ptolemy-skeptics to have started. Newton used to note that the last century has seen numerous charges of historical fraud in the physical sciences, but none produced a fraction of the ferocity of Ptolemists. [Wherever there’s a weak, rationally-indefensible tenet, advocates are left with no other way to protect it than (e.g., Rawlins 2017C fn 1)] by suppression, banishment, indiscriminate argumentation, and circulation of way-overdone baseless or irrelevant personal denigrations against opponents. So when we see such phenomena we should sense said weakness. As a general rule that can save plenty of time and bother:

[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.Newton’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 (§M2) ancient-skeptics seeing to “all of antiquity’s” “greatest stars,” or “greatest astronomer” — the modern clique sells Babylonian astronomy as primary science, too, though it’s just as derivative: (§M3.) In truth, Ptolemy was [a] the occultist author of astrology’s bible, the Tetrabiblos, and chief mathematician for his Serapic religious cult at its Canopic temple; [b] compiler of derivative Almajest (most of which survive immutably to this day among his remaining band of believers). It verbatim-echoed the already-echoed (§M2) ancient-skeptics seeing to “all of antiquity’s” “greatest stars,” or “greatest astronomer” — the modern clique sells Babylonian astronomy as primary science, too, though it’s just as derivative: (§M3.) In truth, Ptolemy was [a] the occultist author of astrology’s bible, the Tetrabiblos, and chief mathematician for his Serapic religious cult at its Canopic temple; [b] compiler of derivative (§N20) Euklideseque “handbooks,” perceptively designated thusly (see References below) by the first able mathematicians of his Almajest (Mantius 1912-3) and GD (Stückelberger & Graßhoff 2006); and [c] used his mathematical talents to hustle occultism by faking the overexact truth of his Serapic sponsors’ belief in celestial predictivity, presumably suggesting a comforting parallel superstition-superadvent for astrology’s predictive efficacy in human affairs. Ptolemy’s “Acquittal” promoted Swerdlow-Gingerich’s idea of established fact: it was the “established ethic of ancient science to report only those observations that best confirmed theory and to disregard [i.e., destroy] the rest” — without letting on that [no ancient witness is cited for this “ethic” (just modern genui)]. [1] So-called “observations” repeatedly in error by ordmag a DEGREE (see, e.g., §D (Illegally Blind), and especially at fn 47) obviously never happened in the 1st place, so there were never any Ptolemy observations to select among. [2] The durable Neugebauerian mantra that effectively-dishonest data-selecting was standard behavior for The-Greatest ancient scientists: [a] cannot survive 5 seconds of critical examination for this (§M3), and [b] is based on circularly taking astrologer

the leader of the losing army begins also to lose track of priorities and will not give up until the enemy is knocking at the bunker door. During this final phase of real wars, millions die. For nothing. But the leader’s vanity. In a hypothetical academic war, the whole sub-field could be made for decades to look foolish. For nothing. But, luckily, academe has no vain leaders. So it never happens.
Ptolemy as the quintessential or ultimate ancient scientist — knowingly 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 alibi Ptolemy by wrenching academia’s view of ancient astronomy to fit him has caused as much damage to modern scholarship (§§M1&N) as Ptolemy visited upon ancient and (above, §A) medieval.

M3 “Acquittal” 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 or to condemn the ancients for the accuracy or for the errors in their 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 §EI) find the solar diameter to ordmag 1’? How did ancients find the mean distance within c.2% (59 Earth-radii: Alm 5.13)? Or find their observatories’ geographical latitudes to ordmag 1’? (See [4 Table 1] or Rawlins 48 fn 34 & Table 3) consistent with each of the 4 observers knowing his latitude to 0M3.

47 To back The Acquittal’s fantastic crock 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 23/s of a degree, nearly triple the angle from lunar center to limb! This error is about equal to that of indoor-Ptolemy’s ‘impossible’ mean lunar period report recently implied by Rawlins 1991W (fn 197) (the erratum fraud by R.Newton 1977 p.190), but (marginally 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 “disprove[s] the ‘Ptolemy-skeptic’ ‘judgements regarding the precision achievable’ by ancients, not-very-unlikely asking: So, Msrs. Newton & Rawlins — NOW who did it so criminal about Ptolemy’s one-degree-erroneous observations?

Evans’ and Hipparchos’ three large outdoor errors clearly vindicated the Acquitters! The DIO J.HA experts who had for months vetted and refereed Evans 1987! Until 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’ Hipparchian error (§3 §B6). Recomputation showed (ibid) that all four ordmag-1’ 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-demonstration 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 ([§B4], just reprinting his 1987 argument (Evans 1998 pp.256-259) header “HIPPARCHUS sic! AND PTOLEMY ON PRECESSION”) & kept pretending Hipparchos’ Spica data showed Greek observational unreliability. [Irony. Evans has debase himself FOR NOTHING: these random goofs can’t alibi Ptolemy’s systematic fit-to-theory errors.] All while non-citing Hipparchos’ usual accuracy or Rawlins 1991W fn 288! No-better-demonstration-could-be-wished [1] of the JHA Assoc Ed [now Editor!]’s citation integrity (DIO 8 1998 p.2) & [2] of cultist disconnect between evidence & conclusions (§3 §B4, §D6). But (and) Evans’ 1997 ‘hectoring’ & ‘incoherence, the latter stands entirely on Evans’ authority 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. Evasion. 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 Editorial & Like Gingerich (§A fn 5), Evans cannot ever be shown wrong by non-club outliers. [On the of-course-Disappeared “notes from that [eclipse] evening”: §4 §B6.]

1994L Table 3). How could Hipparchos measure all of his 3 eclipse-based star-longitudes (Rawlins 2009E) to similar precision? (Note, too, the 3 neatly-interconnected 1% hits hypothesized at §N10.) How were solstices fixed (§N7) to ordmag 1’? Whence arose a Greek stade-length consistent with knowing the Earth’s size to 1% (fn 49 below)? Was it just a series of miracles that all 4 surviving ancient collections of star declinations are (Rawlins 1994L §§F5-F9 & Table 3) consistent with each of the 4 observers knowing his latitude to ordmag 1’? From where (Martian visitors?) did the ancients obtain all 3 lunar months to (Rawlins 2018U §D) one part in ordmag a million or better? For the synodic&draconitic months: nearer ordmag 10 million! (Most of these accuracies were unknown before DIO.) It would be far more miraculous if researchers had merely tailored data to previous values. Back in 1979, did Ptolemites ever take 5 seconds to contemplate such a self-evident & devastating point, ere committing to an obviously uncertain position so deeply that [fn 34] there could never again be a chance of turning back: with the courage of a Lynch-mob, gang-smearing as an incompetent crank (§3 fn 34 & §4 fn 2) prominent Johns Hopkins physicist R.Newton, whose analyses of Ptolemy employed math which historians-of-science couldn’t even understand much less perform. Before such fateful investment, did they even know (fn 12) of the inescapable §F2 Mercury inconsistency, or (§H1) the absent-error-waves test? Can Muffliosi show they ever even looked for an explanation of those remarkable millionth-precision lunar-period accuracies (§3 §H10), BEFORE committing themselves forever (fn 9) decades ago to the above fantastic Neugebauer-klan notion that Greek astronomy was more theoretical than empirical? — and, in this connexion, to such a fragile jest as deal-to-all-evidence promotion of an indoor faker (notorious as such among astronomers for centuries) as The Greatest Astronomer of an antiquity about whose outdoor astronomical empiricism they obviously understood a great deal (§N) less than nothing. Hopefully, these considerations will warn today’s budding archons how a mestastablishment — initially from preconception and carelessness science, then in heedlessly hot outrage at heretical challenge (Swerdlov at §B2, longtime JHA Editor M.Hoskin at fn 3), eventually ever more stubbornly and far-fetchedly as contrary evidence mounted (fn 50) — ultimately got itself regrettably mired down ever deeper into a spectacularly ludicrous position, with which it has still not even seriously started to recover.

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 assume 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, uncredited. 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 retardrums and fanatics of the history-of-science discipline, their plans could hardly be improved upon. (Which actually would be quite unfavourable in that such men as Neugebauer, Aaboe, Britton, and others are brilliant despite their blipspots.) 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:
N1 Asserting 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 ($\S$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 $\S$N15 item [F]. [3] Mars’ $\textit{Alm}$ 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$^{\text{r}}$ (Rawlins 1987 p.237). [4] The $\textit{Alm}$ proof of Mercury’s mean synodic motion was already shown above ($\S$F2) to be pretense, by one who started with the answer and (if the $\textit{Canonic Inscription}$’s elements were also based on alleged observations then he) TWICE — differently — fabricated the “observations” to prove it. [5] The centrally phony aspect of the whole $\textit{Alm}$ is seldom understood (P.Huber 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.

N2 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 $1^{r}$) was a better outdoor observer than Hipparchos, whose errors were actually an ordmag smaller.

N3 Even accepting that Ptolemy astronomers were not primarily empirical ($\S$M3; DIO 1.1 §1 fn 24; Rawlins 2008R §A1 & fn 20). Among objections beyond the a priori: far too many extremely accurate ancient measures survive ($\S$M3&N1, fn 8 [3]).

N5 Because of own 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., $\S$M3; also: memorable Aaboe conversation, 1976/3/9). (Note: Oxford U. Press was warned by 1997/7/16 letter of the book’s problems, but see similarly at fn 46) went to press with all errors intact.)

N6 Resistance to realization that celestial mean motions, lunar ($\S$N16-N17) and planetary (and even some solar), were based on integral (or half-integral) cycles, not by dividing a long angular arc by its corresponding time-interval, as Ptolemy pretends (e.g., $\S$F2).

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

N7 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 high-maximum-height problem, MacArthur-Genius Swerdlow keeps asserting (with Evans’ and Jones’ evident assert: 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 indoors. 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 Swerdlow is not knowingly ladling nonsense to vulnerable archons too predisposed and subconscious to recognize the prank.) Due to just such $\textit{JHA}$-published equinoctial evidence, Ptolemy even perversely teach — complete with now-ironically Pompos sneers (quoted, R.Newton 1977 loc cit) at sub-$\textit{JHA}$ untouchedables — that ancient equinoxes were more accurate (fn 11) than solstices, from their own unfamiliarity (e.g., $\textit{JHA}$ Editor-to-be Evans at fn 11), with [a] the instrumental and astronomical problems involved (R.Newton 1977 pp.81-82; and Rawlins 2018U §F1, whose eq.10 quantifies for the 1st time the ordmag 4th effect of deviation from quadracity: $\S$N19 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 (Swerdlow, Evans, Duke) noticed that the newly available papyrus $\textit{P.Fouad}$ 267A ($\S$N9) has tried to enlighten them by directly surprise-testifying to an ancient solstice which was accurate to ordmag $1^{r}$? Just-luck? (Like another trio of just-lucks at $\textit{idem}$? Meanwhile, note that Duke not only wrongly doubts that Greek observations were sufficiently accurate for trustworthy solstices [$\textit{idem}$], but agreeably if mistakenly proposes that Hipparchos’ – 134 solstice was 5° off, when in truth it was only 1°: see van der Waerden at Rawlins 1991H fn 4; also 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 $\S$B4), while in spite of 6° rounding, the errors in recoverable ancient solstices (one by Kallippos; one by Aristarchos, two by Hipparchos: ibid Table 3 & eqs.1&2&7&8) are $+3^o, 0^o, +2^o$, resp, indicating that Hipparchos’ rms solstitial systematic error ($1^o$) was more than 4 times smaller than his mean equinoctial systematic error. Unrounded 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; $\S$N7) the effect of several arcmin 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.


N9 We recall how “$\textit{Mr. History of Astronomy}” (AAS-HAD Newsletter #51 Page One) dream-creates his idea of ancient realities. Gingerich 1976 p.477 on a temple-bound fakermathematician geocentrist-astrologer out in kookburg Canopus ($\S$I U), who was just as skills-ineducable by his real-science world ($\S$E-E-F) as the JHAD cult is by its: “we can easily imagine Ptolemy surrounded by assistants and graduate students at the famed Alexandria library.” But on the most original genuine ancient scientist’s coherent heliocentrism: trivial by $\textit{JHA}$ criteria ($\S$H2 [b]), just a passing ‘splendid speculation tossed out during a vigorous discussion between the Alexandrian mathematicians” (www.dioi.org/sti56.htm).

48 Irony [due to yearlength-estimators’ dependence on truncated (Rawlins 2018U §2 [C1]) prior solstices]: despite 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 several times 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 $\S$C-DQ0) which do not apply to the remote Babylonian data which ultimately&fortunately made possible the hyper-accurate Greek lunar periods of Rawlins 2017E $\S$B2-B4.
N10 Historians-of-science unexceptionally ignore the perfectly Occamite 3-for-3 hit-success of the spare atmospheric-refraction theory that explains and fits both ancient Earth-measurements, in strictness, with equal success of the spare atmospheric-refraction theory that explains and fits both ancient Earth.

49 Only 2 ancient Earth-circumference C values were widely adopted: Eratosthenes’ (really Sostratos’-Eratosthenes’ value of 256,000 stades, and Poseidonios’ 180,000 stades) were adopted in each successive generation, 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 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 Ptolemy 2.65.146-150 and hence included in the 1st-cm-era data by A.Diller) and 3 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. Indeed, more than 3/4 of a century (summary and citations going back to 1982 provided at Rawlins 1996C fn 47) since Dio published this hyper-neat triple solution that 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 fudging 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 endless wheel-spinning methodological-literature literature: snuffling through divergent ancient lore in search of hints of oddball stades, which of course existed all over the place before the Ptolemaics 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 methodological. Again (§110), many other ancient stade matches are targeted within about 5% of the data web’s \[b\] Poseidonios’ C, & [c] the 185 meter stade that is now (Rawlins 2008Q §J1) accepted by virtually all serious scholars. By contrast, ALL the usual solutions for C (invariably just methodological 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-defating surprise that the Pharo solution of the famous Sostratos-Eratosthenes C is geographical, not astronomical. [Speculation follows.] But refutation is easily achieved when we realize that the unclutter (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’s 40 million meters produces a geometrically 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 un-organized intervals and therefore had 999-999-999-999-9999-9999 1/4 presentations for 5 ancient stadiums 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.Librevc 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 ordmag 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) should of course extend for at least 1/4 part of 10000, that the cities of Alexandria & Meroe’s 143° 1/4, the latter in a position reliably to indicate Earth-c’s so accurately that hypothetical surveyors’ 185m conclusion was trust-worthy to within ±1 m. Was Kleomedes’ famous Alexandria-Asswan legend a myth (DR’s former opinion), or a remnant of Ptolemy I’s post-conquest land-survey of his empire? [With similar possessiveness, less scientific William the Conqueror reckoned his own new booty in the Domesday Book.) The likely story is possible using theodolites (transit instruments possibly constructed 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 determinations c. -300 (Almajest 7.3; Rawlins 1994L). Was Timocharis chief of the project? Hitherto un-noted credit: Timocharis knew Alexandria’s 31°12’ latitude precisely (ibid §F6), while neither of the other two later star-observing Alexandrian astronomers quite did so (ibid §§F7&K9). From solar observations (e.g.,

Philo’s at Meroe: Rawlins 2008S §C), the latitude difference between Alexandria (31°12’) & Meroe (16°57’) was knowable angularly as 2 3/8 sixtieths of C. (At the later standard of 700 stades/degree, this is 9975 stades, only a quarter-percent short of the Strabo-attested distance of 10000 stades.) But, given the habitable Nile Valley’s narrow sinusosity and the non-trivial longitudinal difference between Alexandria & Meroe: how would the survey team measure the 1578 km latitudinal N-S difference between the two cities, in order to divide by 2 3/8 60°ths, to find C? If the proposed survey was thorough & scrupulous enough, we may speculate that this would’ve been possible, though quite laborious. (But: more laborious & expensive than Alexandria’s non-speculative achievement of erecting & maintaining the Lighthouse?) Surveying had been a vigorous science in Egypt for over 2000 years before Ptolemy I, as witness the Great Pyramids’ precision (DIO 13.1 pp.2ff1). Indeed, Egypt’s traditional latitudes near Giza-Heliopolis were better than C.Ptolemy’s (Rawlins 1985G p.260). But Kleomedes hints at a direct N-S arc. Pondering this, we note: an arc-path due south of Alexandria, virtually along the 29° 9 E meridian, could be measured without being interrupted by the Nile or tough-grade mountain at any point. [We already mean the latitude (already named by Philo: (3:11) there on that way.] Precisely reconstructing the numerical achievement: presuming measurement c. –300 of the 1578 km terrestrial arc south from Alexandria (31°12’) to Meroe’s Phi-determined latitude (16°57’), 14°1/4, the stade would have been indicated to be 1578000m/(14°1/4)600 = 185m. [Note added 2018/4/24. DR’s researches have ultimately realized a hitherto-unperceived 3rd BC century Greek-sciences dichotomy: adoption of Babylon’s division of the circle into 360° degrees (for sky (Rawlins 2012T fn & §E), but passing use of 60° for Earth. Perhaps because the latter scheme neatly produced a unit nearly equal in a traditional stade, while the former yielded a unit c.10 times larger or c.6 times smaller,
astronomers did their high-pitched historical 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 (§3 fn 120 below), though not necessarily proving, mere derivative degeneration from its conquerors' superior technology. (See also Rawlins 2018U §34.) An obvious & devastating point, never previously emphasized for its implications: not a single trig 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 §§E) 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 that city's geographical latitude L was. Which may explain why the only attempt at figuring it R, 38°, [all is known] in Greek records, not a word on L, was there unequivocal material, another crushing blow to Babylonianist pretensions, and [b] is too far north by 2°28′ = 148 nni. Finally, while Greek planetary order was physical — Mer-Ven-Mar-Jup-Sat — Babylon's was astrological: Beneficent—Maleficent: Jup-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 recover 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 vaster discoveries of §16, long before DIO. Similarly at Rawlins 2009E 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 § 41) 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 high-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 jearing JHADists who (frazzled desperate to find error in heresy's math but determined §3B3) 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 incoherent 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 suggesting but insisting-upon JHADists' pure speculation that very low period relations (from solar eclipses exceeding 1000' all high-sounding), due to integral or half-integral anomalistic records, with common integral factors removed, as at Alm 4.2&6.9 where factors 17&10, resp, are divided out to simplify the ratio. (Our-long-cycle teaser at §N14 lacked such divisibility.) Consult esp. the half-dozen neat evidences and fits (Rawlins 2002H §§C3§9) backing the theory that the draconitic month was determined by Hipparchos, by using the very same back-end — 140/11/27 eclipse he'd used (Alm 6.9) when first applying (almost as accurately) the same eclipse-period method. (Debate-averse Jones privately produced a single-item retort [which insta-melted upon examination: Rawlins 2002H §§D], while ignoring all 6 shocking positive evidences, possibly a non-scientist's receptivity to data contrary to invincible preconception?) These empirical RATIOs at last explain how ancients determined lunar motions (which Ptolemy&cuneiform express as RATIOs, after all), all accurate to 1-part-in-dormag-about a million or better. The proposed method: [i] is bi-attested (Alm 4.2 and 6.9), while no other pre-100 BC method [A] Without even being told, all positional astronomers instinctively know that the secret of ensuring high accuracy for a 2 event-based celestial period P is just to wait for a large enough number N of returns, ensuring a huge time-interval, t1 to t2, so that the error in deduced P = (t2 − t1)/N caused by the errors in t1 & t2 is trivialized by the enormity of N. (How else could the ancients determine [Alm 4.2] the synodic month correctly to within well under 1 time-sect?!) [B] Even Ptolemy knew enough (§3 fn 119) to use very long intervals when faking non-periodic arc/time estimates of solar, lunar, & planetary speeds. [C] The short planetary periods of Alm 9.3 are obviously not directly measured since they are [i] not integral (there is a remainder of a few days, from the cycle's imperfection), & (see item [D]) [ii] not sidereal. [D] Genuine, huge, observed integral period-relations for planetary periods were listed in Ptolemy's Planetary Hypotheses (Neugebauer 1975 p.906 Table 15), mostly on the order of 1000', verifying to all but splice-dreaming JHADists that long cycles were recorded in antiquity. These vast planetary cycles are all listed by Ptolemy in sidereal years, which is JUST the type of cycle one obtains directly (no need for indoor splicing) from centuries-separated pair of raw outdoor observations of stationary points on the same star, as explained by Neugebauer 1975 p.390, producing period relations without remainders, just as in Plan.Hyp. (Neugebauer loc cit supplies centuries-long sidereal planetary periods for each planet: again, no remainders. [E] There are many ancient attestations (§N16) to direct determination of long celestial periods, but no attestation (or purpose!) for stringing-together short ones to fake very long ones. Ibid p.553 produces an atypical ancient text that for Mars splices sidereal cycles of length 32' (5' short of 15 synodic revolutions) and 47' (4' beyond 22 synodic revs), to produce 79' (1' short of 37 synodic revs) with the advantage of reduced (but still non-zero) remainder. (Neugebauer or source mistakenly renders the three day-remainers as degree-remainers.) But there's zero attestation for the hilarious idea that any ancient scientist did (or would expect to) construct a reliable 1000' period-relation from such crude (low N: item [A]) short-time-base cycles. Nor could he expect to indoor-create, from short and remainder-polluted period-relations, a neatly integral (unremaindered) period-relation of the direct outdoor-obtained type which was already empirically available anyway (& more accurate by an ordmag) without such needless fiddling, & upon which (Rawlins 2003J) all Alm 9.3's short periods were ultimately based. [F] The Alm 9.3 Jupiter 71' tropical cycle's superficially inexplicable big remainder (while the well-known 83' tropical cycle was available with a 50-times-smaller remainder) proves its historical descent (Rawlins 2003Q eq.40—eq.44) from an unremaindered outdoor-observed 427-sidereal-yr integral period-relation, not the reverse (a fiction made fact by Neugebauer 1975 p.391). See Rawlins 2003J §44.

N16 Out of typically excessive and (www.dioio.org/chr.htm#bsvx) uninformed certainty that 13th century BC Babylonian observations couldn't have occurred, privately scoffing at & non-citing as utterly, a priori-ridiculous the only solution (§§III-III7) YET discovered for (any, much less) ALL of the last 3 hitherto-unsoled anciently-adopted lunar motions ([A] System A: [B] draconitic; [C] Ptolemy's last lunisolar equation), namely: period-relation ratios from eclipse cycles exceeding 1000' (all 3 stable, due to integral or half-integral anomalistic records), with common integral factors removed, as at Alm 4.2&6.9 where factors 17&10, resp, are divided out to simplify the ratio. (Our-long-cycle teaser at §N14 lacked such divisibility.) Consult esp. the half-dozen neat evidences and fits (Rawlins 2002H §§C3§9) backing the theory that the draconitic month was determined by Hipparchos, by using the very same back-end — 140/11/27 eclipse he'd used (Alm 6.9) when first applying (almost as accurately) the same eclipse-period method. (Debate-averse Jones privately produced a single-item retort [which insta-melted upon examination: Rawlins 2002H §§D], while ignoring all 6 shocking positive evidences, possibly a non-scientist's receptivity to data contrary to invincible preconception?) These empirical RATIOs at last explain how ancients determined lunar motions (which Ptolemy&cuneiform express as RATIOs, after all), all accurate to 1-part-in-dormag-about a million or better. The proposed method: [i] is bi-attested (Alm 4.2 and 6.9), while no other pre-100 BC method
is attested at all; [2] is the sole ancient method even nearly capable of such hyper-accuracy; [3] automatically spits out ratios; and, [4] inducing the three solutions without manipulating a single digit, one finds ratios that are exact in the previously-unexplained ancient records. During the 1 1/2 decades since publication of this unexpected and fruitful discovery, no scoffer has found in the analyses [a] any math 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 DIO analyses. Nor [d] the courage to cite or debate this theory in print: fn 6. NB: Whereas the selected pairs’ back-end eclipses are spread across 4 classical-era centuries, the front-end eclipses are 4 times more tightly grouped, in a single productive century: the 13th BC.

N17 It is revealing that the JHAD clique has not only failed for decades to understand the central (§35) significance of the Almagrost’s three lunar periods’ high accuracy — but to then let shunnishment dictate refusal to cite the 1st solution (§16) 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 (§33) of the inescapable fact that all three ancient lunar-speeds are micro-accurate, and [2] the eclipse-cycle source (§16) of such accuracy. (Let Animal House’s Dean Wormer top THAT Double Secrecy.) And don’t miss the saddest&sadist-masochist part: all disbelieving 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 mathematical recovery of the greatest heritage from Babylonian astronomy, not to mention the earliest major science in our society: observation and accounting for all of precious eclipse-records, now-lost, but invaluable to classical-era scientists, who, without the huge N (§15) which Babylon’s data repeatedly made possible, could never have found those above cited astonishingly accurate lunar speeds, that are among the prize glories and proofs of empirical Greek astronomy. Rejectors of DIO’s 3 bio-cycle solutions have the advantage that all 3 proposed front-end eclipse records are long gone. But: [a] The early front-end eclipse for the parallel 9660° = 781° case does survive (§14 & §3 [§33]). [b] Is it JHAD-banned to use intelligence to induce Greek astronomers’ possession of the §16 front-end eclipse records? — just as DIO induced (§19) Hipparchos’ − 157 solstice & adoption of Kalippic motion, both of which (after same JHADists’ scoffs) turnout (§19) to be anciently attested. In this field, what is intelligence for, if not to revive lost antiquities? Were the earlier data extant, there’d be no instructive mysteries here. Who deserves a field with no challenges, no advances beyond texts?

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 4688° Great Year, Aristarchos originated the “Babylonian” month, 29° 11’ 00” 50’, decades before Babylon (§3 [G4]; or Rawlins 2002A eqs.4-8). [2] DR’s 2001/6/27 British Museum lecture showed (ibid: eqs.9-11; or §3 [G5]) Aristarchos applied to this the Metonic cycle (235 months ≡ 19°), resulting in his Metonic “tropical” year \( \text{Y}_{\text{A}} = 365\frac{1}{4} \text{I} - 15/4868 \) (fatefully off by 6°, virtually same as Metonic cycle itself). [3] Years before item [2] was discovered, the matching discovery of exactly this yearlength was discerned in Vatican-held ms data listed under Aristarchos’ name on vat. gr. 191 fol. 170° (data at Neugebauer op cit p.601, \( \tau \alpha \epsilon \delta \beta \gamma \kappa \iota \delta \) or 365 4’ 20’ 60 2”; if expressed as the continued-fraction 365\( \frac{1}{4} \text{I} + 1/(20 + 2/60) \) (Rawlins, op cit eqs.12-13, this is \( \text{Y}_{\text{A}} = 365\frac{1}{4} \text{I} - 15/4868 \), verifying above item [2]. All without altering any Vatican document number and much aided by Neugebauer 1975 p.602’s perceptive interpretation of \( \epsilon \) as sixtieths. [4] The sidereal-year companion data, Aristarchos’ op cit \( \epsilon \delta \beta \gamma \kappa \iota \delta \) or 365 4’ 10’ 4’ 10’ 4’ from vat. gr. 381 fol. 163° (Neugebauer 1975 p.601), we write as continued-fraction 365\( \frac{1}{4} \text{I} + 1/(4 - 10 - 41) \) (§3 [G2]; Rawlins, op cit fn 14-15), yielding sidereal year \( \text{Y}_{\text{A}} = 365\frac{1}{4} \text{I} + 11/2 (\text{good within a few time-seconds}) \), again altering no Vatican-ms digit; and, again, a hit: the interval from Meton’s − 431 solstice to Aristarchos’ − 279 solstice is just 152°. [5] The difference between \( \text{Y}_{\text{A}} \) & \( \text{Y}_{\text{A}} \text{ IS PRECESSION} \), suggesting geomobilist Aristarchos’ apt pre-Hipparchan discovery of it. His value is near 1°/century, a much-too-low false rate, later rounded to exactly 1°/century & adopted by Ptolemy. Having adamantly rejected all 5 of the above ideas, from the floor at the 2001 event, Jones 2010B (pp.21-22) won’t cite any of the 5 even while covering the same documents. Unable to find fault with any of the foregoing propositions’ math, Jones (ibid n.27) vandalizes their data-bases by (unlike Neugebauer, loc cit) arbitrarily stripping off all accents, never letting his readers know of the bare existence of any of the 5 proposed advances that the unstripped data encourage.

N19 I note results from newly-translated paper P.F. 267A: (1) the papyrus’ vindication of the previously-unknown Rawlins 1991W (§§K8-K9&M4) discoveries that Hipparchos sought a − 157 S.Solstice and that he (twice: idem) used Kalippios’ 365\( \frac{1}{4} \text{I}-year solar motion. Both his − 157 solstice (a different day: Rawlins 2018U §§L-M) & use of Kalippic motion are explicitly provided on the 1900 old papyrus. (Kowal & Van Brummellen have enjoyed similarly unexpected years-later vindications, which the reader should look up, to share the felicity.) (2) Simultaneous solution (ibid §§K4-K5 & eq.25) of P.F. 267A’s decade-old double-mystery: [a] Why did it specify daytime for the − 157 solstice, though Duke’s accurate calculation from its data got nighttime? [b] What exactly caused the odd remainder of its tropical year, 365\( \frac{1}{4} \text{I} - 1/3/09 \)? The field was informed of these discoveries in 2015. Inert reaction: §3 fn 66.

N20 Confusing the almanacs and handbooks of extant derivative science — cuneiform texts (§13) and Almagrost (§25) — with primary, which is not very extant. Possible practical reason: it’s easier to raise grants for analysing existing works than lost ones, and it helps hype to push the former as central — though the probability is minuscule that ancient watershed-research astronomical manuscripts would be numerous enough (compared to handbooks: §42) to have survived to the present. (Archimedes is the rule-proving exception in mathematics, but even his work on solstices [noted at Alm 3.1] is lost.)

N21 Carrying §20’s handbooks-as-primary-science transformation to an unsurpassable apogee, as the field’s archonship improvidently50 for decades banished anyone who objected to selling the clumsiest (§SC and G) faker in astronomical history to academe & the public as “THE GREATEST ASTRONOMER OF ANTIQUITY.” — Again — we’re not supposed to guffaw?

50 Note advice at Rawlins 2000A c & c 21 regarding [A] caution before plunging fervently into establishment-cultism’s beliefs-blow of inescapable devotion (to temporarily dominant cults’ theories) — an unforgiving singularity which lies in wait to swallow the unwary recruit; and [B] treating contrary evidence as not as downers but instead welcoming them as possible helpful warnings of more of the 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-up-front anti-Newton, anti-Rawlins, anti-Occam try-anything apologia), using such outré resorts as already highlighted above (§L2 and fn 11), undeterredly explaining-away, to his (and mythical expert advisors, allegedly involved as referees, who) that the new data simply didn’t check out in minutes. Note exactly that 6 rarefactions 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 ultimateness of nerve it takes to bill itself (e.g., on Wikipedia) as a peer-reviewed

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31Compiled c.160 (fn 20; contra Toomer 1984 p.1). The common, more respectful-sounding title, Almajest, is descended from the Arabic almajasti, Toomer 1984 p.2. So Almajest seems less corrupt.

32“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 pisspeaks who tear down giants, adding that Ptolemy might not be a giant, but Robert “Newton is a pisspeack.” 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 brand 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 & shunning & running instead of inviting debate, IF (§1) it genuinely believed evidence & competence backed its position? Why did peace never break out? D.IO 16 p.2 fn 1 (2009), emphasis in original: “Rational, pacific discourse shows who’s right & numerate, so: why would archons tolerate peace?”
Astronomer in Wonderland: Historians-of-science

The technically & ethically rock-bottom brand of “research” skewered in the following pages recalls DIO's sue-and-settle summation during NatGeoSoc’s 1989/12/11 denouement-launch of NGS’ amateur [Rawlins 2017B] data-juggling defense of its dying Peary North Pole hoax:

Orchestrate more fiddle factors than the New York Philharmonic.


Universities’ science departments deserve to know the kind of mis-math (fn 13), herd-think (fn 10 & fn 10), data-tampering (¶3-B-G), & idea-grams (fn 10, ¶C8) too often passing for scholarship in prominent but joke-referred (¶2 fn 3; Rawlins 1991W fn 6) & cover-up-prone (fn 10, 11, & 97) journals in history-of-science, a field rife with smearings (fn 8), shunnings (fn 5&116; Rawlins 1991W fnn 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.)

Then, there’s really any evidence that archons teach, value, or even understand (¶¶G5 & J1 [f], fnn 42&106) exploratory hypotheses’ use, tempered by Occam (¶125, fn 33, ¶J5 A., ¶2 fn 49), to expand/deline knowledge. The result: (p.45 & ¶B5-K-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&fn 42], from which scientists have learned. Despite wan reciprocation.) Mathematical scientists’ scurrilous 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-competent, 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 staid version, www.dioi.org/gjs.doc, 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. ¶¶B-C&D-E-F-G), uncorrected-unrefracted math-battles (¶B4, fn 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-original idea of refereeing.) Not to mention shunning of competent heretics’ scrupulously refereed research advances (¶3), and systematic non-citation of the scientific-history journal DIO, though for over 25° it’s been easily the most mathematically and astrometrically competent journal in the science-history eld, vol. 10 even only exceptionally co-published (with the University of Cambridge), long supervised by boards of that rare minority of scientifically able historians (e.g., astronomer-legends E.M.Standish, emeritus CalTech-JPL, & Chas.Kowal, late of STSI), so seethingly feared by the democratically-ruling majority, whose mathematical and ethical shortfalls DIO has

been patching-up for decades without the slightest discernable (positive) effect on the field. E.g., three cornered History of science journals cut contact with DIO. when, e.g., (fn 97), asked to print the embarrassing but unquestioned fact that their icon Ptolemy’s four Sun “observations” were FIFTY TIMES closer to Hipparchos’ 280°-old indoor tables than to the outdoor sky, none doubting (¶2 ¶N8) Neugebauer-Gingerich-Science’s decree that an astrologer & clumsy faker whose frauds damaged & retarded predictive astronomy for 1000° (¶2 A) was “The Greatest Astronomer of Antiquity” (fn 1 here). Have shunning, censoring, data-fudging, and viciously (¶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-below, it was learned (see ¶1 here for links to all papers) that: [1] a 7°-old DIO discovery (Rawlins 2008Q fnq 6) hadn’t been unattributed published, www.dioi.org/eve.pdf, as Isis’ 2015 LEAD article (repair request repulsed), and [2] Isis’ pseudo-refereed final 2016 LEAD article had extensively attacked Rawlins 1985G (Greenwich Meridian Centennial paper) on ancient longitude accuracy (referred 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/lsb.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 acceptance contingent upon whose clique the discoverer belongs to. But, then, actually, one need not imagine, since scores of examples of the field’s too-ordinary acceptance contingent upon whose clique the discoverer belongs to. (Repellant examples: Rawlins 2017E ¶G3.)

However incomplete, the following cronyology 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&fn 42], from which scientists have learned. Despite wan reciprocation.) Mathematical scientists’ scurrilous verification encouraged.

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History-of-science — Data-Tampering, Idea-Theft, Seminumeracy, Smearing, Shuns, Club-Preferencing

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 refereed 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 (JHAD against heterodoxy by publishing effectively unrefereed [fn 88] reconstruction of his scores of volumes.

[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 refereeing or such epistemic canons of scientific evaluation as simplicity, minimal-premises, fruitfulness, and predictivity. Auto-rejection has been inspired by durable permanent magnetism (Diller 1984 fn 26) orthodoxy that the famed ancient data-faking ([fJH], bumbling ([fE]) 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 1st-accurate trig tables & 1st-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: [f]JHAD = cartel of JHAD Journal for the History of Astronomy, O.Gingerich principal editor for 40’s & HAD (Historical Astronomy Division, Gingerich long-standing co-founder) of the American Astronomical Society, whose Ethics Statement has just demoted [2017/10/11] research ethics three notchches: now behind [11] race-gender, [2] sex-triggers, and [ironic in present context] [3] bullying. DIO argues evidently for high Greek accuracy ([f]10; Rawlins 2017E) vs Hist sci reverence for alibiing 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.931 crowned indoor astrologer (Rawlins 2003X) Ptolemy “the greatest astronomer of antiquity”, echoed verbatim by Gingerich 1976 [i.e. 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: [f]2 §§M2&M20 here, or Rawlins op.cit), whatever the cost to plausibility and ethics. Another JHAD promotion of derivative science as primary: fn 120.

2 Consistently invincible auto-rejection of high-olds, 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-wayhouse of ultra-outre occultisms, see here at: §§C1, D2&D3, E2, G7-G9, G11, H4, I22; fn 12, 33, 44&45, 55, 68&69, 89.

DOI’s principled approaches to knowledge are brought together below, at §§1 [g]. See, too, fn 10.

hugely contra-reality super-adherence to 280-old indoor tables, is uncriminal since Greek astronomers were theorists not empiricists, who suppressed — e.g., destroyed — data inconsistent with prevailing models (fn 8&9 below). The Princetitute’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.dio.org/diomd.htm), which has fitted to attested ancient data scores of new heretical reconstructions (many evaluated below, esp. §§1, with selected links), meanwhile asking how ancient astronomers copying preicity could advance to their surprisingly numerous but heretofore remarkably unappreciated high-accuracy Greek measures. (Below, compare §§10 [& §§D] to craniolithic cult-insistence on Greek inaccuracy: fn 1, 8 [f], 69, 93, & 58). B) Also investigated: asthismological 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 monthlenghths becoming undeniably accurate to 1st or better (!); how all 3 hitherto-unresolved lunar speeds were based on classical-era use of 13th century BC Babylonian eclipse data ([f]34); pseudo-Aristarchos’ 1950s retrograde Moon & Archimedes’ degree-use (both obvious, yet unnoted for 2000’s §§11&12 below); Ptolemy’s celestial fakes; Hipparchos’ elaborate and 1st-accurate trig tables, his use of spherical trig: Archimedes-admired pioneer in heliocentricity & spatial-vastness, Aristarchos: P’tannery’s and DIO’s quadruply-verified (fn 88) reconstruction of his temporarily-vast 4868 Great Year, 1st-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, Bloating Network’s Heresy-Phobia

A1 In the contemporary history-of-ancient-astronomy subjectfield, 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 Muffia or the JHAD (fn 1) hereabouts — have themselves made a discovery, to wit: that their network of politically centrist captive journals will help wage JHAD against heterodoxy by publishing effectively unrefered articles that:

4 Wikipedia’s article on the virtually unreferred 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-refered journal in the field, DIO, is repeatedly, agressively classed by Wikipedia as Unreliable (not deserving an article, with bio-references to Rawlins as “publisher” persistently suppressed), though neither Wikipedia’s CSICOP-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 trying to try) discarded dozens of flagrant examples of prominently-published history-of-astronomy catastrophes that nobody refereed with care (or cared to refereee): see fn 50&86 below and above at §§1 POSTSCRIPT: also www.dio.org/jjsh.htm, www.dio.org/fli.htm#wag. 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-frequency 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, from its inception has dubbed this incestuous, occultisms, see here at:

C11, D2&D3, E2, G7-G9, G11, H4, I22; fn 12, 33, 44&45, 55, 68&69, 89.
[A] Noncite the heretical paper primarily targeted. **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 Mufffiosa Janice Henderson 1976, without citing his name or papers.)

[B] Exhibiting sterile 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 clause &fn 30&118&58]) the data upon which an offending discovery is based, decreeing that whatever version of said data was previously accepted (right up until §D13 ready found that, if only now — of a sudden convenient — 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-recognized, wasted.

A2 Below, we analyse, in §§C-G, a flock of recent instances of such data-fudgery-for-orthodoxy, aimed at submerging competently 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 denigration of outliers — instead (of resorting to data-alteration) just

archon-oscilating 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 additionally eased 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, 38y ago: DIO 1.2 fn 66&[B4] whereby “editors” become little more than printers. Note DIO’s typical in.479.org/帝王.pdf, in asking an editor to edit.) And it shows. Lucky for us, JHA refereeing’s judicious deliberateness is self-extolled by proud 2013-2017 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-inelatable JHA-published odd and/or miscomplimented scholarship, see: www.dioi.org/JoH.htm#axzz2, — samples here at fn 10&17&18. No like list of botches debuting in DIO has ever been or can compiled. Maybe due to scrupulous DIO refereeing?

At least until afternoon tea.


6 Purely punitive aim. (Rawlins 2000A fn 172: world’s richest nud-mine? A controversy’s last ditch.)

O.Gingerich, longtime head of Harvard’s History of Science Dep’t, defames Ptolemy-skeptics, www.dioi.org/pm2.htm, in private communications. E.g., his 2000 referee-report to Isis innumerate broadbrush-libelled the now-substantial (if largely silent for professional reasons) Ptolemy-bolting party as just a tiny “paranoic” bunch (f2 fn 5), merely for objecting to communal shunnings he and everyone else would know are real, 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/§111.pdf, DIO 1.1 §1 §7; also see fnm 16&28, as well as ibid §§§D2-D3, vs §3’s fn 7. Establishment-serve Gingerich’s whackamole campaign to contain heresy is detailed at www.dioi.org/j43f.pdf, 1994. His referee reports on skepticism often pretend (between slanders) that it would be 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, unintuitively 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 uppity heresy? That question is explored below, along with the field’s domination by dissent-intimidation & shunning, which has only solidified (fn 125) since 2002, perhaps from scientifically-shy-historians-of-science hazing 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, defending their specialty remotely of each: fnn 33&36&45), whenever seeking apparent evidential support for any

2 Are their depts like French depts staffed by pros who don’t speak French very well? And pretend it doesn’t matter. (It does: §J7; Rawlins 2017E §K2; Rawlins 2018v end.)

3 Is this too many historians-of-science cannot (e.g., §B4) 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 fellows 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’ vindications? — a reflex which can reach such extremes (fn 17 & §F5) as to defy any known academic ethical code. (Outside sororities: Rawlins 2008R Epilog.)

B Hiding Modern Empirical Data: Boomerang Irony & Lawlessness

B1 When today’s Ptolemites are determined to justify what they already knew before “investigating” — the unscientific but cult-approved deed of destroying data needn’t be ancient. Apologists, for Ptolemy’s “observational” ordmag 11 errors, claim it was normal for Greek astronomers to compute outdoor “data” indoors. Or 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.

and (especially scary) competence.

7 Schaefer 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

the baseless speculation that is in fact pure fantasy. What institute did? Or asked who’d let even real insult kill dialog for 1/3 century? [Gingerich’s condition for 1999/7/3 UND debate: bar DR from the podium. Such establishment-unprincipled guarantees: #1 response to this DIO will be fresh irrelevant smearing. Same timing at www.dioi.org/stb.htm#ffpd. (Like Assange, suddenly a “rapist” upon publishing forbidden data.)

8 historians-of-science remain impenetrably loyal to the idea — which
B2 In 1987, in order to justify the modern Ptolemaist vision of antiquity, JHA-Editor-in-progress, J. Evans published an exceptionally polemical, technically pathetic, but politically brilliant Step-One towards becoming a Mufa Maid-Man by assassinating R. Newton’s credibility — anticipating full well the boost he’d achieve towards his ultimate Editorship by attacking JHA Editors’ bête-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 team-work-contribution to the pretense that Newton was as crazy as its cult’s unanimous goosefoot-slander was insisting: in 34. Which is why the parties soon proven right [14 §B4 and 17 §B4] that Catalog were exiled, while those who were impenitently wrong were — the most-impenitent lifted into the field’s politically-ultra Editorship. No surprises.) Evans’ paper tried albining Ptolemy’s ridiculously huge errors to his and his cult’s satisfaction, by adding 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 λ Sgr — the record of which has since disappeared without explanation — and two ancient observations of the star ι UMa vs the eclipsed Moon (Almajest 3.1) by Hipparchos in −145 and −134. The errors were all ordmag 1°: respectively, −40°, −33°, +33°.

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 SA (emph in original), critiquing Evans 1987, http://journals.sagepub.com/doi/pdf/10.1177/002822680770104001, 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? An opponent has already for at least 1200B [185A] beenknown-by-clearly, uncomplicatedly, high-schoolishly, arithmetically indoor-computed his four alleged “observations” of the Sun: §1B below. A textbook case of politics overwhelming reason — as it has, for ordmag a century of history-of-science’s ubiquitous, naked promotion-for-grantprofit of a known scientific criminal. (See, e.g., §2 or Pedersen 1993 p.559’s justification of Ptolemy’s massive indoor plagiarism of Hipparchos’ star coordinates, after initially denying 1974 p.258 by attributing to Ptolemy too much “intelligence”.) 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 [2 fn 2], but ranks true history [vs JHAD status] in canny political order): the Jones-edited 2010 Springer volume Ptolemy in Perspective (CaTech 2007 conference, arranged by CaTech’s Swardlow), is prefaced by Swardlowian prose, “Among the SCIENTIFIC authors of the Greco-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 §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” (Swardlow 2010 p.151), adding that Tycho “took the observations . . . of Ptolemy seriously” (ibid p.154), though Tycho deemed Ptolemy a thief&fraud and so dumped his fake data, epochally discovering accurate precession thereby: §2 A; Rawlins 1993D fn 141. In this same CaTech collection, we’re told (Rapeg op cit p.126, emph added), in a typically (2 fn 18) condemnationless history-of-science clique “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 for fake of all 4 of his of Eratosthenes’ parallax equations)! [Footnote: If the solar eccentricity of a star’s solar parallax is at all significant, it is due to the solar’s elliptical orbit, not the star’s parallactic angle was 14 times reality. [G] Multiplying 40800 stades by 2 yields 256000 stades (fn 42 below), which C&E certainly knew about since it appears on the very Archimedes page cited.] [H] Ibid’s innocence of the ancient context: 

B4 We quote from this 2009 DIO paper, which so precisely & 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 (64pp!) 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 Sgr) 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, 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) Hipparchos’ 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 Hipparchos’ eclipse-star errors) 11 later [in many-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 [he had read the [1991 revelation]10 . . . that DR had discovered
that Evans’ and Hipparchos’ errors . . . WERE NOT OF MEASUREMENT BUT OF BASIC SPHERICAL-ASTRONOMY MATHEMATICS . . . [Yet] when inedible educator Evans’ 1st-hand evidence somersaults, he just pretends he was right anyway, unable to admit DIO scored & “premier” JHA bellyflopped . . . Contra Evans, neither his own nor Hipparchos’ problems were observational. Both simply misconceived . . . valid observational data by using invalid math: the wrong sign for their parallax corrections . . . the [1981 Seattle] longitudinal lunar parallax p_L was virtually 1°/3 . . . the sign mixup would naturally cause an error of . . . -40°, & the laughably imprecise number “11°” here in fn 12 & §10.1 “observational” log.
Table 1: Diller Sph Trig Proof: Hipparchan Longest-Days in Hours ⇒ Latitudes in Stades

<table>
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<th>Klim</th>
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<th>Conv Stads [c3]</th>
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**Note:** The text is a continuation of the previous paragraph, discussing the work of Diller and others on the Hipparchan klimata, with references to other scholars and their contributions. The text mentions the work of JHA and NYU scholars, as well as others, in providing a historical context for the research on Hipparchus' klimata. The text also references the work of Jones and others in evaluating the validity of Diller's findings.

**Table 2:**

<table>
<thead>
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<th>Table 2</th>
<th>Diller 1934 theory’s unrounded L, nonfits (ere col.4’s 5° rounding) there underlined.</th>
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**Note:** The text provides a critique of the work of Diller and others, highlighting the lack of support for his theories and the need for further research. The text also references the work of other scholars in providing a more nuanced understanding of the Hipparchan klimata.
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Further, Jones (ignoring 23°2’3’’ confirmations: [H]: Rawlins 2009S fn 23 &54) accepts Ptolemy 1.12 testimony that Hipparchos’ ecliptic obliquity was Eratosthenes’ $\epsilon = 23^\circ 51’’0’’$, a value which Jones imaginatively attributes to a speculative Hipparchan computation from a conjured-up non-Hipparchan Alexandria $L = 31^\circ$, without realizing that obliquity $\epsilon$ would already be known since ancients found it concurrently with $L$ — and via solstices, not (as Jones curiously assumes) $A$, 2009S fn 23.

Jones also-traditionally alters the Almajest’s text for Hipparchos’s Marseilles latitude, from $L = 43^\circ 0’’0’’$ (consistent with 43°1/12 of the Ptolemy Geographical Directory [GD] to $L = 43^\circ 0’’1’’$, in order to reconstruct (using Eratosthenes’ $\epsilon$) 2nd century BC Hipparchos’Jones speculated indoor calculation-invention (for unstated reasons, and counter-chronologically) of Pytheas’ well-known longago (c.−300) solstitial noon gnomon ratio, $\text{A} = \frac{\text{L}}{\text{H}}$ (whence this precision argues it was an outof29 datum); and, to force the speculation’s success, Jones begs tolerance of an odd-but-convenient Hipparchan miscalculation, yet another ad hoke wrenching of ancients’ data. [While rejecting Table 1’s normal roundings!]

Jones’ promotion28 of such jigaw juggling seeks at least a half-share of Diller’s discovery. Worse: by fantastically alleging that Diller used invalid data, Jones (Neugebauerian: §C2) lodges his half-fitted theory — unvetted (§C10) and untabulated-whimsical as a SUPERIOR to Diller’s ultimately-perfectly-fitting one. Though willing to refer use Diller’s 1934 paper (with 2 nonfits of 11 listed klimata, until DIO’s 1994&2009 upgrades: §C 3[a] & fn 34 end), shunsoluer Jones dutifully, consistently refuses to acknowledge the bare existence of Diller’s 1994 vindication: DIO’s initial near-perfect 12-hits-out-of-13 table32 (though its merit is prominently recognized by H.Thurston [Izis] & by G. van Brummen’s meticulous standard history of early trigonometry [Princeton University]) — or the final DIO 16 $\S3$ update distributed in 2009, where the ultimate blishment-less-fit perfection (Table 1 here) of Diller’s 14-for-14 victory is too irrefutable to deal with. Except by fleeing.

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27 Data-Fudgery for Myth&Turf 2018 D.Rawlins
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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 climatia 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 4-for-14 table completely out of that special bubble, contrasting with the Orwelian-Gobbeltian flames it deserves.

C12 Note: it’s been obvious since [the Rawlins 1994M investigation] that 1" accuracy is crucial37 for the 18º 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 fnm 38&39.

NB: We now have consistent confirmatory double-evidence for Hipparchan 1"-accuracy: [a] Table 1’s 18º 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 (fnm 24&52 here) & trigonometry tables were more accurate than previously believed by Duke or anyone else (including DR, before 1991 testing). So his/hers’ now-mutinous doubts thankfully just emphasize the shocking newness of the discovery of Hipparchos-era 1"-accuracy computation & trigonometry tables: [1] the two eclipse trios (customarily called “A" & “B") confirmed each other by both producing Hipparchan orbits based on Kallippic motion (period 365²/4) [2] new papyrus testimony vindicated in 2005 (§F4) 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 Tria A had computed with a key trig function off by 1" (from slightly unremarkable trig tables), Hipparchos would have found other39 than e = 327²/3, the correct value, which is attested (§D1 below) & is found to agree (§D3) with calculation via 1"-accurate trigonometry tables. To repeat (§C12): [i] the Diller-klimata table’s 18º 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-wisdom 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 number being repeatedly vindicated here (universally-accepted restoration noted in Jones 2002E §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&685, and even more astonishingly at www.dioi.org/hr/htmfcsvq, and below in §J. Computing odds against Jones’ theory (§ Neugebauer’s): Rawlins 2009S §§J1, J3, & J6.

37 At the 18º klima in above Table 1, 58º 12’31” rounds to matching 58º 14’; but 58º 12’ 29” wouldn’t.


34 Toomer 1973: Duke 2008W p.286 also assumes Hipparchos used Ptolemy’s deft trio method.


36 Ancient ordigm-rounded Sun-distance estimates: Rawlins 2008R §§F-F. Sun-distance as historical origin of order-magnitude: Rawlins 2012V §D. Reconstructed distances: [a] $S_1 = 100^0$ Eratothenes (Rawlins 2008Q eqs.6-13; nearly same at Carman & Evans 2015); [b] $S_1 = 100^0$ Hipparchos (§D2 here; Rawlins 1991W eqs.22-24; Rawlins 2008R eq.12); [c] $S_1 = 10000^0$ Aristarchos & Archimedes (ibid §A.1), which was falsified as a lower limit in 1991 by empirically-obviously-heliocentrist Poseidonios also proposed at least 10000¹; ibid §F2, probably on solid observational grounds: ibid §E4. Wise Greek realization that the Sun had such tiny parallax that its distance could but crudely estimated was obviously reflected in ancient scientists’ repeatedly ([a]-[c]) setting the ratio of solar distance to Earth-radius at a power of 10. [Interlude for exceptions: [1] Hipparchos tried a variety of solar distances, at some point halving his 1000¹ solar distance to 500¹, thus solar parallax 7', later the inverted basis of 3438'/7' = 491¹ c.4080, Swerdlow’s valuable & original discovery, ibid fn 39. [2] Arab astronomer Al-Battani foolishly used 1146¹ [180-60/3¹], plainly overexactly computed [ibid] via round 3º parallax. [3] Almagest 5.15 has non-empirical 1210¹. It is a reflection of the state of current history-of-astronomy and of history-of-science that the History of science Society’s Isis (toppe history-of-science journal) so failed to understand such a simple and fundamental concept that it could publish its analysis published 2015 an analysis co-authored by Evans (fn 10 above), Editor of the JHA (“premier” history-of-astronomy journal according to Schaefer 2002 p.40), deliberately-narrow-focus-arguing that Eratothenes had a solar distance of 102¹ (fn 106) — a transparently overexact value — rather than 10⁰, as realized years earlier at Rawlins 2008Q (eqs.9-12) through common sense, antiquity-sense, & consistency with Eratothenes’ long-enduring available actual (Rawlins 1982W) Earth-circumference, 256000 stades. What does it say about the epoxyar open-mindedness of history-of-astronomy’s current #1 archon, when he figures Eratothenesian solar distance = 2n*408000 stades/252000 stades = 102¹? He allegedly never thought to explore-by dividing (into same numerator) the 2 alternate M1 & M2 candidates: Kleomedes’ famous 256000 stades (yielding solar distance 103¹: Rawlins 2008Q eq.8), or DR’s provably-known-to-Evans-but-uncitable 256000 stades, which yielded 100¹ within 1/100¹ of 1% (ibid eq.11). To obscure his 1987 mismatch (§B above), Evans has also for 27 years now refused to withdraw his claim that [i] & Hipparchos wrote — MOST-most-atypical (§B83) — the so-called Moon by more than its own diameter, & will keep on (fn 11) ducking (like Isis': fnm 13&100), trusting that academe lacks the integrity to reveal

D Lunar Orbits’ Plausible & Implausible Solutions — Hipparchos’ Mechanical-Computations’ Reliability

D1 As recounted in Almagest 4.11, Hipparchos investigated two lunar eclipse trios, both of which had occurred well before his era, usually called Tria A (—382-381) and Tria B (—200-199). Ptolemy reported that, for Tria A, Hipparchos had computed lunar orbital elements from the time-interval and longitude-interval between eclipse1 & eclipse2, and the same intervals between eclipse2 & eclipse3. And then did the same for Tria B. Hipparchos’ computational findings were, for Tria A (—382-381) orbital radius $R = 3144$ units, eccentricity $e = 327/2$ units; for Tria B (—200-199), $R = 3122$ 1/2 units, epicyclic $r = 247,1/2$ units. G.Toomer tried an ingenious and daring reconstruction40 to recover these numbers, wrongly assuming (like Almagest 4.6&11 and modernly D.Duke) that Hipparchos had used Ptolemy’s sophisticated mathematical procedure (ibid §A). Duke combined this attractive and seemingly plausible theory with a more speculative one: proposing Hipparchos’ use of a 3438-based trigonometry table (as used later in India), presumably41 figuring that the crude proximity of 3438 to Hipparchos’ R values (above) was meaningful and that the R were not fixed at the outset but occurred during the math development and were never normalized, hitherto-unheard-of procedure. But ultimately Toomer couldn’t match any of Hipparchos’ 4 numbers.

D2 DIO instead started with the normal, conservative assumption that both orbit radii R were adopted at the start of Hipparchos’ lunar researches. A known (e.g., Almagest 3.1) user of Aristarchan data, he could have computed the R via Aristarchos’ famous 87² half-Moon elongation and a typically ordigm-rounded solar distance of 100⁰, as follows:...
R = 1000\cot87° = 52'24", or, in 60ths, 3144" — thus matching43 Trio A’s R (above). Now, a common slip (ancient & modern) is confusion of unit-fracture (inverse integer) & arccin, since each is signified by a prime-marker; so if we test the hypothesis that a Hipparchan-school computer later misread 52'24' as 52'1/2, we find, in 60ths, 3122'/2 — exactly matching44 Trio B’s R (above). Unable to counter the math, centrist pols (faces eternally, irrevocably invested into shun: fnn 125&127) have, during the decades since 1991, had no reaction to this minimal-premises double-match of both 3144" & 3122'/2, besides implicitly contending45 (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 had used eclipse-pairs, not trios (the trios-approach has never yielded unmanipulated data-matches),46 so he sought only one unknown, eccentricity e (or epicycle-radius r), not apogee-at-epoch \( \alpha_0 \), or mean-longitude-at-epoch \( \nu_0 \). Thusly computing d\( \nu \)r produced 327°39' & 247°30', respectively, each a near match to the above (§D1) corresponding attested Hipparchan data: 327 2/3 & 247 1/2. (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 defied those recommendations (despite Toomer’s honest acknowledgement of his 1973 speculation’s dubiousness), resurrect-recycling it by altering — explicitly altering — Toomer’s numbers, to ensure its success by inventing teleologically convincing calculations. DR, recently expanding48 his 1991 orbit-reclamations, compares DIO-vs-Duke simplicities:

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that History-of-astronomy’s present Ultimo Archon mis-signed his 1981 parallax-correction (§B4), but won’t admit so since silence helps keep alive the JHAD sacred cow of Greek inaccuracy. So far, his god-given (pede-dictum) academist’s honesty has proved perceptive. Ability-consistency of JHA Editor Evans’ recent here (faithfully carrying-on the exemplary tradition of 1970-2013 Editor Hoskin’s grasp of mathematical astronomy: www.doi.org/fff/ff%ffy); [ii] 1981-1987 failure to sign parallax correctly, and [iii] 2015 failure (on solar-distance) to understand that if Greek observing accuracy was on the order of 1' as Gingerich insists (Rawlins 2018U fn 3), and as Evans echoes (item [i] or §B4 above), then since Eratosthenes’ parallax is given as \( \text{omag} 1° \) (66' 56” at Carman & Evans 2015 p.14), solar parallax’s uncertainty was \( \text{omag} 100' \) of its size [like §1 5E]. (Rawlins 2008R §C5 argues that Aristarchos suspected an even higher but so only non-scientist pols like Ptolemy or JHADists wouldn’t realize that the JHA-Issis 102’ distance’s \( \text{omag} 1\% \) precision is way-far-too exact — when uncertainty is 2 omaggs looser. All this reveals an even looser grasp of Greek astronomy & (elementary mathematical astronomy) by two of the world’s most deliberately-eminently-history-of-science journals. Among famous classical Greek astronomers, only faker Ptolemy insisted on solar-distance’s numbers of such naive wayoverprecision, e.g., Almajest 5.14-16, which Hartner 1980 p.26 justly deemed a “fairy-tale” (like R.Newton 1977 p.198); yet another case where seeing Ptolemy 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 arccin & unit fractions: ibid fn 251; Neugebauer 1975 p.166 n.3, & p.729 n.15; Thurston 2005 p.620. Only DIO’s theory (or an incantation?) explains the odd but Mufa-uncited circumstance (Rawlins 2012V §15) that Almajest 4.11’s two \( R \) differ by less than 1%. For Toomer and Duke, that must be yet-another longhotencion.


46 Chun qiguan, loc cit n7f. 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 \( \alpha_0 \) and \( \nu_0 \) were pre-assumed, not sought: Rawlins 2012V §1. Pair-calculations’ matches of \( \nu_d \)r to Hipparchos’ attested values: ibid §§F2&G2, & Rawlins 1991W §N4, & Thurston, loc cit. (Doubling double-occultism: implicit in Duke 2005T). Summaries in Thurston op cit pp.60&66-67.

D4 DIO’s reconstructions [A] are consistent in method (for both trios); [B] are rife with ancient-typically round-number elements (Rawlins 1991W eqs.5, 8-9, 11, 21-23); [C] change no Almajest 4.11 numbers, these already long-established by Newton’s learned 1977 analyses (§E below), & invent no convenient Hipparchan mechanical miscalculations.

D5 By contrast, Duke: [A] like Toomer, calculates \( R \) first for Trio A, then reverses course to satisfy Trio B, which doesn’t work, anyway, unless an extra variable d (hitherto not in evidence, in Ptolemy, India, Toomer, DIO, or otherwise) is arbitrarily brought in to rescue the situation; [B] finds no round elements; [C] alters extremely precise numbers like 51°30'23" and 8°48'28" to instead become extremely precisely 51°19'37" and 8°48'06", respectively, of the trios’ non-resemblance of eccentric-kearth, or any independent justification of fudgories so shamelessly explicit, besides rigorous issuance of The Right Answer. All to smother DIO’s natural-flow-multifit coherent solution under a pillow fluffy with special assumptions&tampering, resurrecting the spirit of co-subtractor Jones, above (in [C] & below). Like Jones (§C10), Duke has refused DR’s request to withdraw the paper.

D6 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.doi.org/prl.htm, but evaluated and rejected by DIO prize-judge Thurston. It was later published by Centaurus. (After refereeing whose superficiality is shockingly obvious for math and even text.)49 Contra Duke’s attraction to committing fudgery: throughout Rawlins’ researches, it proved unnecessary50 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.

E Robert Newton’s Foolishly-Ignored Discovery of Hipparchos’ One-Degree Eclipse-Fudge

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 adopted EH/PH/PH from long-earlier lunar-eclipse Trios A&B ([C]C13&E2, fn 55). In 1977, physicist Rob.Newton detected53 a hitherto-unnoticed 1° error in Trio A’s 3° eclipse, warning that data-restoration here is demanded (fn 91 below) & incredible” consequences will flow from analysis. The orbital elements derived by warning-rejectors Jones and Duke inadvertently double-confirms Newton’s “incredible” prediction. Like Jones, Duke does not notice Newton’s 1° 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 abort Thurston (and fn 32 above) précised in Isis in 2002. Duke’s paper independently computes54 best-fit e\( \alpha \)A of the Trio B-accordant EH orbit, & of the inevitably weird unrestored-Trio-A-accordant orbit, as if original, though

50 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 & §24.

51 Below fn 72. The 1° shift, discussed below in §E turns out not to be a mistake but (worse) a deliberate Hipparchos-schule fudge, as shown in Rawlins 2012V §G & fn 11.
52 Ibid §A3.
55 Ibid fn 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 discussion (§E2 below): ibid §M5; and §M6 discerns that the 2 elements
all 4 solutions had appeared earlier in the very DIO paper he is non-citing while trying to exile & replace it.

E2 Nonciting Newton’s and DIO’s analyses protects readers from learning that, after application of Newton’s unexpectedly productive-predictive 1st correction, Trio A is satisfied by unzany elements, which turned out — as discovered 141 later — to be half from the EH Sun orbit (already known from Trio B) the familiar prime PH Sun orbit. Unless utterly coincidental (as Duke and Jones judge reasonable) or an evil miracle, the result reveals, that, at the time of his Trio A calculations (PH’s —145), Hipparchos’ adopted Sun orbit was transitioning from EH to PH, so he temporarily retained EH’s eccentricity and Kallippic mean motion (until their new PH tables were computed & prepared), while immediately adopting PH’s zero-point & apogee (neither needing tabulation). Sun actors must see as further mere-coincidences both the correctly-paired split (between tabulated and table-irrelevant parameters) — AND the temporal order matching E1’s chronology, above. Coincidence piled on coincidence? Or shall we turn for guidance to Saturday Night Live, where ChurchLady’s Faith-Based epistemology at last unmask who’s behind sinister DIO’s outrageously incredible, still-accumulating concatenation of impossible accidents: could it be — SATAN?!

E3 We return-to & lodge an obvious (& hereabouts typical) potential question to the 2008 article’s author, journal, & putative referees: should the reader be censorially denied the opportunity to decide for himself whether or not §E2’s astonishing but Centaurs-uncited half/half upshot is meaningful? — and thus whether R.Newton should be credited for a finding that triggered unanticipated progress, as valid discoveries will.

F Solar Orbit Reconstructions and Fruitfulness

F1 Hipparchos’ —145 Prime PH solar orbit (§E1) is famous, because adopted by Ptolemy (Almajest 3.2&6) & still worshipped by Julian over 500’ after creation. But DIO reconstructed56 (two other Hipparchos orbits): his —157 Early EH orbit (§E1) & —134 Ultimate UH orbit (ident), each seriously differing from PH & previously unknown. (NB: Hipparchos’ use of a late non-PH orbit was presciently induced a half-century ago before anyone by Britton 1967 pp.45-47.)

F2 Discovery of Hipparchos’ final UH orbit arose from calculations,57 refereed and supported by Thurston58 and Curtis Wilson,59 based on realization60 that cuneiform text (eccentricity & speed) that had to wait for adoption ‘il tabulated were naturally those that temporarily stayed EH, while the swiftly-adopted PH-elements were constants thus needing no tables. Perfect manifold correlation-confirmation? Or more DIO witchery? 56 Rawlins 1991W §90 & Rawlins 1991H eqs.13, 17-18, 28. Prescience: Britton 1967 pp.45-47, noted at Rawlins op cit §H2. Hipparchos (& Ptolemy) cited by Julian 1:429 (in “Hymn to the Sun”).

57Hipparchos’ ultimate improved data (~142 Autumnal Equinox, ~134 Summer Solstice) cause his orbit-recalculation, thus shift from PH to UH: Rawlins 1991H §5C6-C13.

58Thurston 1995. For JHA’s rewrite of this note to falsely credit Jones for a Rawlins discovery, see DIO 6 §3 §D9. For JHA subsequent insistence on precis Thurston’s followup, to again avoid crediting the shunned discoverer, indeed entirely deleting his name from Thurston’s note: see ibid §H. Alex Jones’ retraction late but exemplary.

59C.Wilson on Thurston 1995 (fn 58 above): “I am glad both that the meanderings of Jones’ argumentation [Jones, “Computations” — see below, in fn 86] can be set aside, and that Rawlins will have a little bit of recognition for the discovery of UH. . . . I have checked his calculations and found nothing to quibble about. I hope your article will trigger some important re-evaluations.” (From letter, Wilson to Thurston, 1994/12/29, copy to Rawlins, with added handwritten note: “I hope there are some re-earmings from Thurston’s article.”) Verbally, Wilson’s views on the state of the astronomy-history community (of which he was long the doyen and conscience [WHO NOW IS?] were stated more explicitly on occasion.

60Rawlins 1991H eqs.1-31. Babylonian astronomy specialist Britton helpfully added that DIO’s estimated date, ~100±35 (ibid eq.9), fit BM55555’s writing style.

BM55555 [ACT §210] (c. ~100) bears a year-length computed from Greek solstice data, an unexpected, shockingly-contra-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 Almajest 3.1 report: fn 70 & §F9.

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

[1] Why the Aristarchos —279 and Hipparchos —134 Summer Solstices are the only to among the twenty-eight solar records61 of Almajest 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 positions62 of the Sun at Almajest 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 Almajest), he twice finds disagreement with Hipparchos’ reported values (all computed

61Below, §§P; or ibid §§B3&B4. BM55555’s revelation: ibid eqs.6&8. I will ever be grateful to the late Willy H. Newton, who was 1st to suggest (letter to DR 1980/8/15: ibid §A) that scholars (including DR) were ignoring Ptolemy’s hour-omission for two of the Almajest 3.1&7 solar data.

62See, e.g., the bizarre attempt at Neugebauer 1975 pp.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 sort). This joke-inversion is based merely on roundings in Hipparchos’ semi-popular Commentary which are cruder than for his regular longitudes (Almajest 3.1&7.2) or declinations (Almajest 7.3). Neugebauer 1975 pp.642-643, deems Aristarchos’ data nonempirically faked (similarly Evans 1998 p.72 vs Rawlins 2008R §A, sardonically at L8.) He is not known to have been into astrology or theft. He bucked the Galileo affair. Meanwhile, Ptolemy stole, mutilated, and fabricated data in order to fake the truth of the geocentric astronomy of the governmental (Serapic) religion which employed him [12 n8]. Given their relative merits, one would think that the modern science establishment would admire Aristarchos and condemn Ptolemy. One would think.” Pondering history-of-science’s pollution of even scientic forums’ consenses, a hypothesis suggests itself which is consistent with this grotesque historical-distortion-by-historians-of-ancient-science: in a grant-grubbing era, public history’s balance is now determined by which figure left the most works. By lawfully fake-justifying the superstitions which powerful institutions (religion, astrology, gov’t) wanted to promote, go-along-conservative Ptolemy’s Aquinianly-enormous pandering to power-institutions and their brainwashed victims, ensured his works’ massive preservation — thus, historians-of-science today can make a living by writing the works of UH . . . and holding conferences on a fat corpus. Revolutionary pioneer Aristarchos, by fighting the same institutions in honest and principled defense of emerging valid but upsetting scientific perceptions, ensured his works’ near-extinction, so there’s virtually nothing (explicitly — for those who cannot induce beyond the texts) left for moderns to fiscally exploit. Isn’t the history-of-science profession inspirational? 63In the 1991 May JHA Jones 1991H p.117 claimed it’s impossible to find a Greek orbit that satisfies these Hipparchos data, though all 4 elements of such an orbit had already been published by DIO (sources: fn 56 above).
by him from UH, unbeknownst to Tptomen); 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 arcmin 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 90°.

[6] Uh: its the 1-127 Autumnal Equinox, follows Meton’s sacred ~431 Summer Solstice by exactly 304°1/4, so 16 or 2 such intervals just equal the 4868° “Great Year” of Aristarchos. And perhaps of Hipparchos himself; if the latter invented a version of the 4868° cycle at 1778021° (not Aristarchos’ 1778022°; §5 below) it embedded an astounding quintuplet of geometrically expanding cycles.

Previous analyses never got past the 1st cycle of the five, e.g., Swerdlow 2010 p.174.

F4 From fitting EH to eclipse-trio B, DIO mathematically induced (Rawlins 1991W §§K4-K9) in 1991 that Hipparchos’ earliest Sun orbit, [a] a used a ~157 summer solstice, [b] adopted Kallippic solar motion, 360°/365°1/4 for Trios A AND B. Findings [a] & [b] were both previously unsuspected. But, 14° later, paper P.Foud 267A was examined by A.Tihon (paper 1st presented: Peking 2005) & was found to explicitly verify 1991

F6 NB: These DIO induction-predictions aren’t side-issues. They are central to understanding the 697 years of Hipparchos’ evolution from amateur-observer-astrologer into an immortal celestial scientist. And subtractors have been uniformly oblivious to a central steel connexion, revealing his original resort to calculating not observing his earliest, grossly inexact solar declinations’ error averaged c.15°, so it formerly seemed odd that the Sun-based equinoxes were subject to 7° systematic error (found independently by 4 different scholars)50 which corrupted all these equinox discoveries (equalled well-refereed: see challenge here at fn 70) was added to head off that hideous eventualit. (And without even citing Rawlins 1991W, the very paper whose thesis is being thrashed! — 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 (fin 116&125), such intermittent commando operations are simply standard maintenance. And, unlike for a military attack, you can completely screw up, but — to your own outlander’s regret — there’s no such thing as a successful kill. Because, besides DIO, no known reader — surely the hippie Hipparchos — is an HJA referee — of the paper in question (Duke 2008W) has yet read beyond its bald claim (that the Hipparchian eclipse trios [analysed in Rawlins 1991W] are worthless), to evaluate its credibility or that of the multiply-misbegotten case brought forth against Greek accuracy. (See Rawlins 2018U [B4&N and fin 10k19], for the three main errors of Duke 2008W.) So, since the defamation of heresy is unscrupulously accepted by all, it’s completely effective for its purpose. 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.

E.g., Tihon 1995 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 brotherfinder Ptolemy.

EH&PH elements compared at Rawlins 1991W: §K9 vs §K10. Duke 2008W pp.293-294 calls DIO’s reconstruction “convincing” due to not to DIO’s errors in the underlying math but because said math is too “sensitive”, implying (fn 66) that §F4’s double-vindication by papyrus was merely spooky-lucky. These inexcusably (esp. §F6) citationless attacks are met in fn 37 above, and indeed had anticipated decades ago in Rawlins 1991H [JHA & Rawlins 1991W fn 205.

Duke op cit pp.284-289 (vs fin 70k96 here). Solstice’s immunity from refraction, etc: Rawlins 2018U §G1. Without sneaky DIO black-magic, odds against EH’s three chance-hits within 20° of the three distinct cardinal-points: 8000-to-1 (ibid [N44id].)

observations by that amount on average (while not affecting his solstices), an ordmag higher than his actual 2’ random solar-decination single-date 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 fnn 61&70 here.)

**F10** Though fully aware of inconveniently-existing P.Fouda 267A, the same Duke paper nonetheless pretends that DIO’s now-papyrus-confirmed predictive hit-[a]& double-hit-[b] ($\|$5 above), are ENTIRELY ACCIDENTAL — occultist shades of himself and Jones (fnn 45&36, respectively).

**F11** He calls the EH orbit “either conclusive or not 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-challanged Duke explored resorting input again (as at $5\dagger$ above, item c), but STILL couldn’t find alternate orbits [i] which fit all the relevant data of *Almajest* 4.11 and 5.3&5 — which Rawlins 2012Y 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$^\circ$ precision — dawn, noon, evening, midnight — as all 9 cardinal points for EH&PH&UH (not to mention P.Fouda 267A: fn 66 here) conspicuously do. Duke has been publicly challenged (fn 70) to produce his alternate orbits. Nothing has come forth.

**G Aristarchos’ Yearlengths, Pre-Hipparchos Precession, & Pre-Babylonian Accurate Monthlength.**

**History-of-science Archon’s Talibishment of Evidence.**

**G1** The mystery of the superficially-nonsense ancient yearlengths$^7$ 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: $\tau_2$ $\epsilon_8$ $\beta'$ $\epsilon_9$ $\beta'$ and $\tau_2$ $\epsilon_8$ $\epsilon_8'$, or 365$'$4$'$ 20$'$ 60$'$ 20 and 365$'$ 4$'$ 10$'$. 4$'$.

**G2** Taking the numbers exactly as they stand and allowing signage-flexibility,$^7$ Rawlins in 1980 treated$^1$ both Aristarchan expressions as continued fractions, and swiftly sent the results to the *Journal for the History of Astronomy.*$^7$ Listening to Neugebauer’s perceptive perception that 60 could signify 60$^7$, Rawlins saw that the $1^\circ$ expression could be viewed as $365\frac{1}{4} + 1/20 + 2/60) = 365 \frac{1}{4} - 1/54868$, a classic Metonic “tropical” year, quite close$^2$ to the known (also seriously false) tropical yearlengths of Hipparchos & Ptolemy. The $2^\circ$ expression suggested 365$\frac{1}{4}(1/4 - 1/10 - 1/4) = 365 \frac{1}{4} + 1/152$, differing but ordmag 10 from the actual sidereal year then (fn 114 below).

**G3** Both results’ implicit periods, 4868$^7$ (Great Year) & 152$^2$ (2 Kallipic 76$^7$ cycles between iconic Meton’s and Aristarchos’ S.Solstices. –431 & –279, respectively), are among the EXTREMELY$^8$ 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 close$^7$ to 1$^\circ$century, which presumably later influenced Hipparchos to treat 1$^\circ$cy as a lower limit, though Ptolemy eventually adopted 1$^\circ$cy exactly (*Almajest* 7.2-4). Note that Aristarchos is the only astronomer on the Vatican mss. for two different yearlengths, obviously suggesting precession. As the 1$^\circ$ astronomer we know was a public geomobilist, he is an apt candidate for true discoverer of Earth’s precessional wobble.

**G4** To measure the Moon’s motion & apogee, ancient scientists wisely chose (*Almajest* 4.2) the 4267 month eclipse cycle for its 126007$^7$ interval’s felicitous near-constancy (due to near-perfectly-integral return in 4573 anomalistic months), regardless of ecliptic position. That interval’s tiny inconstancy-amplitude$^7$ of c.1$^\circ$2 guaranteed the deduced monthlength’s accuracy to one part in ordmag 10 million. (Divide 4267$^7$ [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 4267$^7$ cycle vindicated Ptolemy’s oft-doubted contention that it was the historical source of the ancients’ highly accurate monthlength $M$, commonly miscalled the “Babylonian month”. Rounding at the 10s-place of the 2$^nd$ sexagesimal term (as we find on cuneiform texts: fn 80) yields the $M$ attributed at idem: $126007\frac{1}{2}/2 = 29\frac{191}{9000}'00"08'29"'09"' = 29\frac{12}{4}/4\frac{03}{13}1/3 = 29\frac{53}{30954}$, correct (even today!) to a fraction of a timesec. Aristarchos’ 223-month saros expression (idem will, if divided$^7$ by 223, yield $M = 362\frac{10}{4}/4\frac{03}{13}$, which agrees with above “Babylonian” $M$, to one part in tens of millions (fn 81 below) — decades before its first known appearance in Babylon, which favors his pre-Babylon authorship of $M$, as does the chronologically ordered Vatican mss’ long-overlooked listing of Aristarchos prior to anything Babylonian. TWICE. [See www.dioi.org/jb113.pdf, Tables 1&2.]

**G5** We know that *Almajest* 4.2’s saros of 223$^7$ agrees$^1$ to 1 part in 24 million with *idem’s* deceptively-round-looking saros expression, $18^7 + 10^7/2/3$ or $18^7 + 4/135$ (where superscript K signifies Kallipic Years of 365$\frac{1}{4}$ each) — which, times twelve 135, so

$^7$ Metonic “tropical” year: *Almajest* 3.1: Rawlins 1999: Tihon *op cit;* origin $1^\circ$ rightly suspected by T.Mayer in the 18th century; later by, e.g., Swerdlow; cause of ancient tropical yearlengths’ large common error traced by a stimulating paper, Moesgaard 1983, and by Rawlins 1999 [Do & Rawlins 2018U SP7.

$^7$ Censorinus 19.2&18.11 connects Aristarchos to 1623$^7$ & 2434$^7$, which are 1/3 & 1/2 of 4868$^7$, resp; see fn 79 below, & Rawlins 2002A fnn 14-15 & eq.7.

$^7$ Ibid fn 14 shows that, whatever one’s sign-chances for the latter digits of the Vatican mss’ Aristarchos expressions, implied precession will still be near $1^\circ$century, Hipparchos’ lower limit (Rawlins 2018U [K5], verified by Tihon *op cit.*

$^7$ The 4267$^7$ cycle’s crucially & conveniently trivial inconstancy: $1^\circ$ 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$^7 = 269^7$. Note revealing Muftic inversion at Tousner 1984 p.176 n.10.

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Data-Fudgery for Myth&Turf

G6 We can also merge both cycles found from the Vatican ms (§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 Kallippicus Great Year, thus 1778069°. (Heath 1913 text 315's reconstructed year, showing Aristarchos toyed with a day-rounded exeligmos, is an admirably clear 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 Var. gr. 381 fol. 163v's yearlength). [Since 15° + 32° = 47°, 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-verification 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: how to alter evidence, to underact a paper that doesn't, and how to trash into chaos coherent&kuantumpered 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 nimby sideways to a sly Gordian tactic: erase all acccents on the mss (the crore-ripe ploy of fn 88), which automatically, deliberately wrecks the data-basis for Rawlins 1999's refined, precious inductive journey from Vatican mss to [1] ancient science's ingeniously constructed (Rawlins 2002A §A) & modern induction's 4-way (fn 88) reconstructed 4868° Great Year [itself from superaccurate M: §§G4-G5, www.dio.org/jb11.pdf, eps-5-7, & [2] Aristarchos' sidereal year 365° 1/4 + 1/152. How does such holy warfare differ from the modern? [Query er-passor: would ancient astronomy historians at old NYU-InstStudAncWorld Director Jones' deleting all accents from the mss of the Almagest?] Like-Talibanish is his authoritarian justification for across-the-board woopout: decreeing accents on Greek mathematical data are destroyably worthless in HIS expert judgement.

(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 manuscriptual vandalism. (Though see Rawlins 2000A & 13.) 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 Velikovskyian Talibombing!, Jones, adding to a Jones—Rawlins 1999/7/14 letter, he was reading the very DIO issue that 1st disseminated DR's continued-fraction analysis (of the now-Jones-de-accented Vatican mss data) resulting explicitly in Aristarchos' Metonic yearlength, 365° 1/4 — 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 & #2), the culmination of a century of scientific analysts' ultimately quadruple-confirmation 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 Luciferian quadruple (fn 88) coinidence, 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.

4868° 3°31'50''8''20° 235/19 = 1778022°, (thus fn 83 matching the remainder-numerator of item [3]'s Great-Year yearlength 365° 1/4 — 15/4868 (§G5). (See parallel fn 83&84 above. And carefully check the galloping successes reviewed at Rawlins 2002A, culminating in its eps-10-13.)
Pliny's Circuli: Defeat Ancient-Trig Approximation-Inventiveness

I1

Now for a case of ancient data-tampering that actually happened! — the very sort which modern-chauvinist tamperers have, with dedicated persistence & Creativity, tried misprojecting89 onto valid data (above §3C-G) that commit the crime of being inconvenient to prevailing Muffa preconception.

I2

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 1st — and still only available — coherent solution (fn 91 below) ever devised for the circuli indicated they are no more arithmetical than Hipparchos-Strabo's klimata (which Neugebauer depreciated similarly: fn 18 above), proposing that Pliny's circuli are instead trigonometric (as are Hipparchos' klimata) and a case where it is reasonable to test91 for possible ancient data-alteration, since all seven circuli are clearly a version of the traditional Seven Klimata.

I3

Fit-probes upon them initially produce ridiculous, yet trending obliquities. Experimentally shifting all circuli M by the same simple round constant,92 1° (or 4°), finds gratifying consistency with the same accurate Hipparchan 23°2/3 obliquity discovered by Diller and insistentially & cementally rejected by Neugebauer&Jones (fnn 18&25, resp).

I4

Rawlins' 1984 paper (invited for the Greenwich Meridian centenary Longitude Zero Symposium), featuring its 1°-constant-shift solution (fnn 91-93 here) of the circuli, has never been cited by Jones, though his own pale constant-shift ploy for the Hipparchos-Strabo klimata (§§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 klimata 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) klimata-echo of it — reveals a trigonometric scheme whose cleverness is crudeness' antithesis. Nor does Jones' circuli-puddle-down 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 replace (§3C6 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 & circuli (& Diller & DIO) — thereby obediently according with predecessors' orthodoxy (just-as-reliably as did just-as-careerist Toomer's "observations": fn 8), ensuring Jones' rapid elevation to JHA boardship.

I5

Our next chapter presents dozens of new perceptions, all rigorously JHAD-uncited, 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): none has noticed this for the last 200094.

I6

If modern 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 Sizex's farcical95 fundamental empirical data (2° solar & lunar diameters!), & in spite of its ludicrously contra-reality requirements that: [a] total lunar eclipses last 12° (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 Bizarrity-Hall-Of-Infame, by the astonishing fact that, again: no one has noticed this for the last 200096.

I7

In the 20th 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 diameter of 1/155 of a "part" or μερος of the zodiac as: 1/155 of a zodiacal sign. But μερος (meros) was just an ancient unit = circle/48 = 7° 1/12, of which 1/155 exactly agrees with Archimedes' correct half-degree testimony. Aristarchos' famous 87° half-Moon elongation wasn't empirically a precise figure, and moderns (and seemingly Hipparchos: §D2 above) assume, but a lower-limit.

I8

If from tiny solar declination-motion near solstices, unevenly seen by97 amateur-astronomer-Ptolemites (incl. MacGenius Swerdlow: §2 in 8) doubt ancient solstices could replace (§3C6 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 & circuli (& Diller & DIO) — thereby obediently according with predecessors' orthodoxy (just-as-reliably as did just-as-careerist Toomer's "observations": fn 8), ensuring Jones' rapid elevation to JHA boardship.

I

Unmet Challenges — Advances in Understanding Ancient Science Endangered by Hate: JHAD Braves Glissed Shunning for Cover

Though numerous scholars have doubted 3rd century BC Greek scientists' adoption of Babylon's degree-division of the circle, it was unexpectedly revealed94 in 2012 that Archimedes' original unprocessed measure of the Sun expressed its diameter sexagesimally 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 200096.

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Reverse
be good to ordmag 1\textsuperscript{a}, which invites equinoct-vs-solstice accuracy-comparison (\S 7\textsuperscript{a}).

\textbf{I6} Rawlins 1991\textsuperscript{H} found the Babylonian yearlength on famous Astronomical Cuneiform Text 210 was (\S 2\textsuperscript{a}) based on well-known Greek solstices (\textminus 431/627 & \textminus 134/626), the 1\textsuperscript{st} datable transmission of an orbit-element between Babylonian & Greek astronomy, going Greece\textrightarrow Babylon, thereby getting the Muffia's holiest tenet (\S 37\textsuperscript{f}/120).

\textbf{I7} 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. Rawlins 2017E \S 2: “For advancing history of science, knowing \textit{science matters}.”

\textbf{I8} Ptolemy's allegedly-outdoor 4 solar “observations” (132-140 AD: \textit{Almajest} 3.1\&7) averaged over 1\textsuperscript{st} 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 1200\& F. Rapp 2010 p. 121): undeniable but also (for over 300) JHA-unutable \S 2; despite null ancient attestation of their speculation, Ptolemistse insist (fnn & 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 1\textsuperscript{st} 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 \textit{just copying predecessors} achieve their many accurate discoveries? (E.g., Greek vs real ancient monthlengths, Rawlins 2017E \S\S\S\S\S 2-B4: \textit{starting proximity}.) However, most scholars (A. Pannekoek, R. Newton, Y. Maeyama, J. Brandt, & P. Zimmer among the welcome exceptions) reject ancient high scientific accuracy (fnn 1\& 35 here), undeteratable 2016 subtractor D. Shcheglov even (fn 13 here) calling it a “delusion.”

\textbf{I9} Aristarchos discovered precession 15\textsuperscript{o} before Hipparchos: \S 3.

\textbf{I10} Rawlins’ order-of-magnitude estimates, of too-unhumane-precise accuracy (\S 1\textsuperscript{f}): 1\textsuperscript{f} for lunisolar diameter; 1\textsuperscript{f} for moon distance; 1\textsuperscript{f} for observer latitude (see fn 39\&100 here and \S 4's Table 1 & fn 38); 1\textsuperscript{f} for 500-mile North-South arcs on Earth (fn 111); 0.1 for star declinations (fn 100); 0.1 for lunar-land-vs Sun gap (fn 12 here); 0.1, even 1\textsuperscript{f} for star-vs-voided-Moon gap (\S 7\textsuperscript{b}); ocular error 1\textsuperscript{f} (fn 97 here) for Hipparchos’ mean equinox, with 2\textsuperscript{f} scatter (fn 12); 1\textsuperscript{f} for Earth-circumference measure precision (fn 108); 10\textsuperscript{f} for lunar eclipse-prediction (fn 97 here); 1\textsuperscript{f} for lunar eclipse observation (Rawlins 1985G pp. 258\& 265); 1\textsuperscript{f} century for mean motion of Mars (and maybe Venus); 1\textsuperscript{f} for all three adopted months (synodic, anomalous, eclipse), each correct to better than one part in a million. Historians-of-science like Shcheglov show no sign of awareness of any of these Greek achievements.

\begin{itemize}
  \item obvious from, for instance, Britton 1967 p. 29. More generally, \S 8’s key Obvious Question C jokoies fn 8’s Swedlow-Gingerich-Scientific-American’s Ptolemy-exculpation-theory.  
  \item 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 (\S 8) from a projected Rawlins paper (fn 74 here; precensored text at Rawlins 1999 [E]), along with non-seeker-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 ordmag 10\textsuperscript{m}: Rawlins 1991H eq.32.) The 7\textsuperscript{f} 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 1\textsuperscript{f} (±1\textsuperscript{f}) unaccounted-for: see Rawlins 2018U \S 4B. Mars (and Venus) mean motion accuracy c.1\textsuperscript{f} century: Rawlins Rawlins 2002V fn 26. Contrast to fn 1 here, and to the umpteenth fruitless metrological analysis of Eratosthenes’ Earth-circumference, Shcheglov 2016, www.dioi.org/shc.pdf, which massively contends that high-accuracy Greek longitudes are mythic, his entire assault up-front-promoted by History of science Society (and post-protected by HsS standard-notation-noncorrection), ultimately undone by his brutally confusing a solar ellipse with lunar 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 (\S D), \textit{DIO}’s Letter-to-Isis’ hiding-since-receipt Editor H.F.Cohen.
\end{itemize}
In 1982, Rawlins sought the epochs $E$, as well as the errors $x$ in the observers’ assumed 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. In 1994, ex-

Rawlins 1994L §3F-8, where nulls show Timocharis (known observations c. 300-271), earliest of Alexandria’s 3 observers of extant star declinations by surveying instrument, alone knew his exact L. Later studies get virtually the same $E$ for each observer. Mayyara 1984 finds this, but instead of DIO’s bivariate least-squares (Rawlins 1994L Table 3 results), he independently finds observers’ epochs $E$ and latitude-errors $x$ by double-monovariate, recalculus 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 challenged Journal of Astronomical History & Heritage [JAHH], Brandt et al 2014B, www.dioi.org/bzj0.pdf, says most of Rawlins’ geographical latitudes $L$ “seem to come to our values”. But their $L$ are merely assumed, so DIO’s JAHH-requested 2014/8/26 referee report, www.dioi.org/jau8q.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 by JAHH, e.g., (of c. 300-271) observers’ standard-deviation by compositing methods in www.dioj.org/bzj0.pdf, p.5 to find 36°08’ (Argos City) and barely conflict with commonly rounded 36°, while consistent with above 36°08’, indicating Hipparchos’ permanently fixed instruments were near Rhodos Island’s Lindos (36°05’).

Latitude $L$ of the perhaps-portable (inferior) transit circle of Hipparchos’ most southern observation-spot has been reconstructed
d by: [1] Thurston-inspired spherical-trigonometric transformation of the Almajest star catalog’s southern ecliptical data, restoring originally-observed equatorial coordinates, then [2] testing the declination data thus recovered, to learn which $L$ produces (in implicit zenith-distances) the dramatic, R.Newtonian excess of 00°04’ expected for raw ancient instrumental observations. That (35°50’) suggested the southern stars’ observer worked on Rhodos Island’s south tip, Cape Prasonesi (altitude over 200m), $L = 35°53'$, ‘unquestionably establishing an unobstructed southern horizon, to see as far south celestially as possible from the island. (Unobstructed southern sea-horizon was also chosen by Eudoxus at Knidos, Pytheas at Marseille’s Cape Croisette, Tycho at Hvin Island.)

Using Alexandria or Cape Verde Islands as 00° longitude, did far-apart scientists organize to compare each others’ Moon eclipse data, thus fixing longitudes of sites to such exilings is to intimidate pushback-scholars into silence. And, in current grant-hunger-corrupted academia, it nearly always works. Nearly. None of these three purportedly-refereed journals (vs above & fin 48&97) encourages non-closeted investigation of DIO accuracy or of their own behavior. None of Isis’ bunkered editors’ emails on what ultimately became www.dioi.org/qjo.doc, nor Isis’ 2017 March “referee report” ever mentioned the paper’s history or science or mis-math or factual errors. Not what really matters anymore in history-science. (After demanding the paper’s muting, Cohen finally sent an earlier unmuted version to his referee, ensuring the negative verdict he sought.)

Table 2: Ancient Observers’ Epochs $E$, Adopted and Actual Geographical Latitudes $L$

<table>
<thead>
<tr>
<th>Observor</th>
<th>$E \pm \sigma_E$</th>
<th>Adopted $L$</th>
<th>Actual $L &lt; \pm \sigma_L$</th>
<th>$\sigma_0$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Timoch</td>
<td>-294±11°</td>
<td>31°12’</td>
<td>-1°8±12’</td>
<td>9°0’</td>
</tr>
<tr>
<td>Aristyl</td>
<td>-258±8°</td>
<td>31°14’</td>
<td>+1°4±12’</td>
<td>6°1’</td>
</tr>
<tr>
<td>Hipp</td>
<td>-131±1°</td>
<td>36°08’</td>
<td>+0°2±1’</td>
<td>8°0’</td>
</tr>
<tr>
<td>Anon</td>
<td>+159±0°</td>
<td>31°15’</td>
<td>+4°4±12’</td>
<td>8°0’</td>
</tr>
</tbody>
</table>

118 | DIO-J.HA 22 3 3 Data-Fudgery for Myth&Turf 2018 D.Rawlins

109 | The main Hipparchos observatory’s deduced 36°08’+1’, was just-recently independently by 2016 realization of the mutually confirmatory reality of his computing reliability ([1] E4. Enjoy chronology of Thurston’s felicitously-persistent inspiration of dullard journals instead ineffectively demand the author keep quiet what needs elimination in order to finally satisfy “editors” too scared [or lazy] to edit, §4 fin 13.) The intent of
ord mag 1° accuracy, before Ptolemy’s fateful stades/degree scale-shift (§111) stretched the map East-West, inflating longitude-differences?

I22 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, all 3 were checked against Eusebius’ long-negotiated Eratosthenian Earth-radius r = 408000 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 math, 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 being based on Sostratos’ non-astronomical Phares method (§124), which directly finds r from his Phares Island lighthouse (in Alexandria harbor), built near Sostratos-Eratosthenes’ time and place. Again, our deliberately silent (§§I22&I26) 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 ingenious idea/achievement: mount Phares’ flame precisely h = 300 feet or half a stade above seawall, 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 oversight visibility-distance v in stades. At first the trick seems suspiciously overeasy & dimensionally impossible. But it works. Note that squaring 202 stades, and rounding conventionally, yields Eusebius’ r = 408000 stades (§I22): a 3-to-1-unlikely-chance-hit (Rawlins 2008Q §J1; Rawlins 2018V).

I25 Taking the stade’s length to be the generally-accepted value, 185 meters, Sostratos-Eratosthenes’ r = 408000 stades is 19% or about 6/5 too high, while Poseidonioi’s 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, the 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 Phares-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...


108 Rawlins 1985G p.265, taking an idea due to (ibid n.22) Gosselin 1790, suggests an ancient, adjusting for the 700 stades/° — 500 stades/° switchover, stretched longitudes by 7/5, mis-assuming they were based on land-surveys (§I 5). Or by 4/3 via Poseidonioi’s C = 240000 stades — 180000 stades. Proposing Greeks organized to compare eclipse observations (fn 13): Rawlins loc cit, vs fn 90 above & consistent with the now-forever-true Verde Isles, westernmost known land, chosen as Marinos’ ° longitude (www.dioi.org/051.pdf, §A5), to kill longitude sign-muffs like those (fn 97) cursing Shcheglov 2016. [Speculation 2018/7/15. Marinos = pseudonym, like “Ptolemy”? Or map-title from maritime Tyrre?] C = 256000 stades 1° induced from a Nile map’s latitude intervals: Rawlins 1982n pp.212, 214, 216-217; Rawlins 1985G p.289; Thurston 2002s p.66. For which C fits Eusebius’ r = 408000 stades: just multiply r by 2 π: Rawlins 2008Q eqs.8-11&18, esp. eq.11’s solar distance = 100.1 AU. Fn 10’s 42 AU is overexact (fn 42), even with squaring 252000 less well than 100 AU fits 256000 fits.

While it is possible to find a positive solution to the equation (MNROYAstrSoc 1970); ending British Neptune-discovery sham (Scientific American 2004 Dec p.98); solving BM55555, thus revealing Hipparchos’ last Sun-orbit (ifers above); epochs&latitudes of 4 ancient astronomers (DIO 2005); ocular basis for Aristarchos’ expansion of the universe’s radius to over 100 million Earth-radii (DIO 2008); revealing Archimedes’ solar diameter was sexagesimal (2012); organizing the prominent citizens committee establishing Baltimore’s internationally known 2004 Rachmaninov memorial, www.dioi.org/rar.htm, also composing its text; recent book and play, www.dioi.org/sha.htm, asking if C.Marlowe died 1593/5/30 — or debuted as Shakespeare 13...
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 cynosure realize that the 6/5 factor has been standard among navigators&astonomers for over 100? (All scientific navigation manuals have horizon-dip shrunken 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 case110 for re-evaluation (emphasises in original) follows. DIO’s new PHYSICAL — not standard knee-jerk-metrological — theory (ascribing both ancient [Earth-C] values’ error to [atmospheric] refraction) simultaneously solves . . . both the (very discrepant) Eratosthenes & Poseidon donostos 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 Eratosthenes stade-scruener J.Dutka]111 . . . 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 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 prominently churning out forced metrological retreats (as recently as late 2016! — in 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 metropologists: 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.

110 Quote from Rawlins 1996C fn 47. Those who have spurned the 185-meter stade include F.Hultsch, E.Lehmann-Haupt, A.Diller, C.Sagan (more at [2] N10]. The ad hoc nature of the durably mythic ruler “Eratosthenian” stade is obvious to most specialists, e.g., P.Gosselin, E.Bunbury, D.Dicks, O.Neugebauer, J.Berggren, A.Jones (more at idem). Who earlier had the required science? Gradually, without more ado, they downgraded indiscernibly at a critical point, but waited for a valid one to come along. So far so good. But now that DIO’s airbend theory is here, no historian-of-science is claiming the math doesn’t work. Or that anything works better. Has the-catatonia got the JHAD-tongue?)

111 Dutka 1993 P.64 cites Rawlins 1982N — whose App.A explicitly links 6/5 to lighthouse and 5/6 to sunsets — without ([2]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 up to a meridian circle of circumference 398700000m) 5000 stades Alexandria-Aswan & Aswan-Meroë, each good to ordmag 1’ for 700 stades/degree: 7° 1/8 + 7° 1/8 = 14° 1/4. (Rawlins 2009C §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 Geminus, etc.: Neugebauer 1975 pp.590 (n.2), 733, & 1364 (Fig.43). Is a Ptolemy I survey’s memory embedded in Kleomedes 1.6.10’s famous legend? (Rawlins 2008Q §A4[a].) Dismoor 1950 pp.230-251, cites 5 ordmin-10°-dispersion Greek stade-long races-courses. (Schenk 1966 p.106 all more or less indiscernible at a critical point, but waited for a valid one to come along. So far so good. But now that DIO’s airbend theory is here, no historian-of-science is claiming the math doesn’t work. Or that anything works better. Has the-catatonia got the JHAD-tongue?)

Sourcing Ptolemy’s final lunisolar ratio, \(105416^a = 58523^c\), occurred 2 decades ago (all 10! digits exactly elicited) by test-exploring Greek awareness of the 800th sidereal eclipse-cycle nest (1/5 of 800th cycle attested: Geminos 8.40-41): solution, awareness, & nest not suspected ere Rawlins 1996C eq.31. (Sidereal year accuracy: \(\text{ibid} \text{Fn 110}\)). Royal Muffia Cavilliers have produced no math error or alternate solution since. Predictable result (see Rawlins 1996C’s title and boxed 2013 statement atop its p.2): permanent silence.

More muteness greeted DIO’s 2002-2003 discovery that all 3 previously unsolved, almost-adopted mean motions of the Moon (1. System A; 2. draconitic; 3. Ptolemy’s last lunar equation) \(11^{15}\) were exactly consistent with discovery by ancient scientists who fully divided an eclipse cycle ratio by whatever integer or half-integer was common to both the ratio’s terms, just the way Ptolemy at Almajest 4.2.6&9 explains determining monthly synodic, anomalistic, & draconitic. Notably, no matter where, over a 400° span (3rd century BC to 2nd century AD), the pairs’ latter eclipses are located in time, all the prior ratio-solving eclipses turn out to be from the very same century, the 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 views opened by these astonishingly exact matches. Instead, the entire history-of-ancient-astronomy shunniness, frustrated by inability (like §133) to find math error or 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 \(11^{16}\) scholarship by Johannes Koch who had already (earlier) estimated Babylonian observations’ precence as above \(1350^d\) by surprise realized that Hipparchos’ famous 600° lunisolar tables effectively went back further than the real competition (e.g., fn 119). Facts: No other method is attested. (Twice: \textit{idem}.1) No other method is so simple & immediately-direct-to-the-result. No other method could ensure such high accuracy, 1-part-in-10\(^6\), 3 times out of 3, eliminating false near-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 Almajest 4.2.6&9}. No other credible (fn 119) method, attested (or unattested) has math-reproduced ANY of the numbers sought, while DIO’s proposal has done so for ALL 24 digits precisely — that is, all 6 4-digit components — on-the-nose in each case: see www.dioi.org/thr.htm#cqtp.

Id: 11.6.1 (Heiberg 1907 pp.78-79 or Neugebauer 1975 p.901 eq.3); 105416\(^a\) = 58523\(^c\). Solved: Rawlins 1996C eqs.20-31. Thanks to K.Moesgaard for a perceptive correction.

136 To emphasize the precision and the breadth of this achievement, we display the three-anciently-adopted lunar speeds DIO has mathematically traced to hugely-separated eclipse-pairs, all starting around the 13th century BC (details www.dioi.org/thr.htm#cqtp):

- 12911/11/23 vs. -280/11/16 & -1273/12/05 vs. -262/11/26 \(= 6695^3\) (System A),
- 1244/11/13 vs. -140/1/27 \(= 5458^1\) (Hipparchos),
- 1200/07/11 vs. -1189/06/12 \(= 3277^2\) \(= 3277^2\) (Plan Hyp.).

(Latter dates: earliest firm System A text is \(-262\} Britton 1999 n.6; Rawlins 2002B [Eq.6]; -140 [Rawlins 2002H §99] & +125 + +136 [Rawlins 2003P [C] eclipses are attested.)

137 Again: mere integral division is DIO’s twice-ATTested eclipse-cycle “method” (too fancy a term?) of exactly reproducing all 24 digits. In the 1/2 decades since these super-simple DIO solutions 2002-2003 debut: no historian-of-science has publicly engaged a single one’s science. Nothing beyond a rigid clique’s continued traditional insistence on its vapid theory that UNATTESTED laborious Babylonian analysis of poor lunar horizon 138 data couldamusa produced such accuracy — if only enough \(119\) data were averaged! (This bizarre notion came inevitably out of the Neugebauer-Babylonianist cult, ever-clinging baselessly \(120\) to its sacred tenet that Babylon gave rise to high Greek astronomy.) Naturally, no numbers are provided \(121\) to show how such a fantastic reconstruction could: produce HYPER-accurate results, or [bf] find the draconic month at all, \(122\) or [c] distinguish

\(115\) See Huber 2000 for the variety of systematic errors infecting Babylonian crude horizon data \(\textit{of use for which month-gauging is totally unmentioned in any ancient source}\) plus the brevity of the Babylonian data’s time-base (2.2 centuries). Contrast to DIO’s uncomplicated ancient-standard eclipse-cycle-ratio method, clearly attested (§134) for finding Greek monthlengths from eclipse-pairs separated by 3 1/2 or 6 centuries, or proposedly and fittingly (§136) 10, 11, even 13 centuries.

\(116\) See www.dioi.org/thr.htm#cpc & Britton 1999, for his theory of System A-year origin, and www.dioi.org/thr.htm#cpc, for his clique-pique at Rawlins 2002B’s simple exact-to-same: just halving an integral eclipse period-relation! P.Huber dreams that ancient merged non-integral short-period relations to create 1000\(^1\) integral ones (contrary to obvious common-sense, as well as ancient side-real-vs-synodic records: \(\underline{12}\) §N15), like Neugebauer 1975 reading into 3\(^1\) century AD abdominal testimony (ibid p.321) such an imaginary construction (ibid p.322). But that sort of origin \(A\) is, unlike DIO’s Almajest 4.2.6&9-based method, unattested (ibid p.555 finds no integral or 1000\(^ \times\) rarity of short-versus-long time advantage) from dividing shortness by the accuracy of a new ancient integer, known to every astronomer who ever gauged celestial periods (from Mars to pulsars) in the real world science. Ptolemy knew better: Almajest 4.6 uses long time-spans, so deduced celestial mean motions “will be valid over as long a period as possible.” See Toomer 1984 n.18 at Almajest 3.1. 121 Indicating specifics’ sheer breadth: www.dioi.org/dj29.pdf, Rawlins 1991W §E-F, esp. §E3 & fn 73. Hmmm. Do pan-Babylonians never-wonder just why: Babylon had no Aristarchos? No Archimedes? No Apollonios? Not even a Seleukid Euklid? No trigonometry. No transit data. No observed solstices. No spherical instruments. No knowledge of Babylon’s latitude (\(\underline{12}\) §N13). No serious astronomy until after Greek conquest (\$G5\$) Was Seleukid Babylon’s gift to science its fortunate preservation of Greek-astronomy glimpses (e.g., §F3 [1]) on durable clay, not fragile papyrus?! 

120 DIO’s ‘theory, which easily & EXACTLY (fn 119 above) solves System A’s monthlength for his clique-pique at Rawlins 2002B’s simple exact-to-same: just halving an integral eclipse period-relation! P.Huber dreams that ancient merged non-integral short-period relations to create 1000\(^1\) integral ones (contrary to obvious common-sense, as well as ancient side-real-vs-synodic records: \(\underline{12}\) §N15), like Neugebauer 1975 reading into 3\(^1\) century AD abdominal testimony (ibid p.321) such an imaginary construction (ibid p.322). But that sort of origin \(A\) is, unlike DIO’s Almajest 4.2.6&9-based method, unattested (ibid p.555 finds no integral or 1000\(^ \times\) rarity of short-versus-long time advantage) from dividing shortness by the accuracy of a new ancient integer, known to every astronomer who ever gauged celestial periods (from Mars to pulsars) in the real world science. Ptolemy knew better: Almajest 4.6 uses long time-spans, so deduced celestial mean motions “will be valid over as long a period as possible.” See Toomer 1984 n.18 at Almajest 3.1. 121 Indicating specifics’ sheer breadth: www.dioi.org/dj29.pdf, Rawlins 1991W §E-F, esp. §E3 & fn 73. Hmmm. Do pan-Babylonians never-wonder just why: Babylon had no Aristarchos? No Archimedes? No Apollonios? Not even a Seleukid Euklid? No trigonometry. No transit data. No observed solstices. No spherical instruments. No knowledge of Babylon’s latitude (\(\underline{12}\) §N13). No serious astronomy until after Greek conquest (\$G5\$) Was Seleukid Babylon’s gift to science its fortunate preservation of Greek-astronomy glimpses (e.g., §F3 [1]) on durable clay, not fragile papyrus?!
among almost-as-accurate proximate ratios (www.dio.org/th.htm#epc), or [d] explain why each solution emerges as a ratio, a clearing 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 comiconic solution,飞s all of the overkill-numerous, solid, unambiguous evidences consistent with said theory, particularly its PRECISE match to Hipparchos’ draconitic 5458 ≈ 5923 ratio (Almajest 4.2) by pairing an early apogee eclipse, 1244/11,3, with his Rhodos-observed 140/1/27 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 timeframe. Comments: In all history, no astronomer before Hipparchos ever used an apogee-perigee eclipse-pair. Scoffs at the theory’s outrageousness-vs-orthodox-preconception inadventently compliment it by reflecting its potential advance’s enormity.

I38 §136 presents three perfectly fitting 2002-2003 eclipse-solutions to Greek adopted monthlengths. Why shunners’ 1st-impartiality in finding DR-errors? Or alternate solutions? Answer (§134): there IS no other umbral lunar eclipse-pair whose integral-months ratio precisely, proportionally, 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-Babylonian region, latter eclipse within 50 years of discovery-date (c. -260, -140, +136 resp), earlier eclipse not ere Almajest 4.6’s 720/18/9 (oldest eclipse-data historians-of-science accept that Hipparchos possessed). Tooner 1984 p.166 n.59). To pioneers who undo the above negative assertion by finding, before 2000/1/1, real umbral lunar eclipse-pairs directly solving the ratios in question (under above specs, incl. hist.sci’s own 721 BC bound), DIO will gratefully grant: $10000 for 3277/3512; $20000 for 6247/6695; $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 Thee 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 Tooner have field contact with Rawlins for years, e.g., Thurston 1999D fn 2) 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 ([§15] 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 minimize other historians-of-science-grasp the empiricism & 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., fnn 16&83 vs fn 20&25&40). [g] Philosophy-of-science&common-sense Occamite theory of fabrication explains both and to the Almajest’s 1st precision. Therefore, in each case (lunar or solar): which approach would Occam prefer?

125 Half-dozen evidences for Hipparchos as author of 5458 ≈ 5923: Rawlins 2002H §C.
126 Neugebauerians long taught that 6 cuneiform-tables’ lunar calculations for c. –200 proved chronologically Hipparchos (c. –130) took his draconic creation 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 213’s flock of coherent evidences, but also rejects an unexpected key opening up spin-off-benefit: recommending responsively increased caution when time-placing non-clay-dated astronomical cuneiform tablets: ibid §D1.
stand indefinitely-uncorrected his own miscastled-backfired evidence, not retracting the slander of Greek science it was adduced for, in ever-orthodox support of the 2 prime inter-related field-dominant clique-myths regarding Greek astronomy that we’ve been discussing hereabout: [a] Ptolemy’s honesty (fn 9); [b] Greek data-inaptitude’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 astronomy to fit a faker-astrologer; then, post-posterously, using 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. Thanks to all who’ve launched an explorer into a 9th decade of a life of unfenced curiosity. Blessed by felicitiously often-confirmed inductions of the distant future of the distant past.
Text for the Day:

In the 1946 Alfred Hitchcock film *Notorious*, German spy Claude Rains suddenly learns he’soops-unknowingly been communally sleeping with a U.S. spy, and realizes that his fellow German spies would sniff him yesterday, if they discovered his security-breach. So he seeks advice from his wise mom, who consoles him by pointing 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 blunder. As she comfortingly 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 untruthbound and untruth-bound (fn 18) the slander that no-one but a CRAZY 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 closer to Hipparcos' 280-2-old indoor tables than to the real Sun. The prime forums perpetrating this fantastic but profitable joke on academy and the public, for consecutive decades, are the Journal for the History of Astronomy and the American Astronomical Society’s knowingly 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 smart or honest,2 as we are about to see again and again below. But as with off-crafty Rains, the prime forums are protected by the very atmosphere of the field 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 insisted-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 history, even though oftmeticulously documented in DIO.

1 Prime smear against dissent is Insanity (as with media on pols’ heresy) by megafunded establishment-polishers & darlings Gingerich (fn 16) & MacGenius Swerdlow (fm 4k&18, §2 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/jih13.pdf, DIO 1.1 §3 §§D-D2.2
2 Nobody disputes the 50-to-1 indictment. But no Reputable Forum (including even popmags, newsrags, & tooo) dares broadcast such heresy-supportive truth, either. The rigid decade-after-decade policy: hide it from the public. (Given the power-secretarial state of the nation’s Free Sutter 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 crusader from a projected DR article, that the paper was suppressed by *JHA*, being finally published by DR 16 later: original unembarrassed spread www.dioi.org/j013.pdf, Rawlins 1999 fn 13. Understand the attitude: 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, DIO 18 §T13-T16.)
3 DR has asked AAS to monitor HAD’s "unprofessional" (Schaefer 2002 p.40) behavior: 2002/10/2, 2015/12/29, & www.dioi.org/jccx6.pdf, email) 2017/8/26 (no reply) transmitting photographic proof of dishonest archival smearing: www.dioi.org/pm1.htm, vs www.dioi.org/pn2.htm, tactics long known to DIO recipients, a class which includes the AAS, whose chief in 2017 joined the deaf&dumbers.
4 Some authors may be able, but this breed of apology never quite is. Evidently countering such feeble and comically self-contradictory (Rawlins 1992V §§C31-C32) effusions is not a serious challenge ("like shooting fishstories in a barrel of monkeys": www.dioi.org/jih13a.pdf, DIO 1.3 §10). And that is exactly why Ptolemyists eschew (§2 fn 52) risking rational debate with DR, written (www.dioi.org/deb.htm) or spoken (§3 fn 5) preferring character- assassination-wealth’s bravery: fn 4.
5 Ptolemyists’ integrity-level (e.g., §6 below) generally shows up less in the (perhaps-unintentionally) deficient original paper than in subsequent failure to acknowledge its thesis-gutting flaws.

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**Ptolemy Enmity**

Ptolemy-Defense Cult Lays Yet ANOTHER Egg On Own Already-Unwipably-Eggregious 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 archonally apology-defenses of Claudius Ptolemy, sacrosanct mascot-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. Sadly. Among Ptolemy’s numerous clumsy *Almajests* [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 Hipparcos’ 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 Hipparcos’ old indoor tables than to the outdoor sky’s actual Sun.

The *JAHJ* 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-unaided 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 $S_0$ of residual-squares, as is also demonstrated below (§C23). While observers’ epochs $E$ are nearly right (but not new), 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 $x$ (and 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 JHAD archons who long ago mistakenly decreed Ptolemy “The Greatest Astronomer of Antiquity”4 and thus have faces so at risk of meagga-eggitudinal 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 inducible 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 miasma may skip straight to §C), which has by now so brain-dirtied the mass of non-specialist historians that writing in opposition may be little more than preaching to the converted.

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1 See Gingerich 1976 for 2 prominent examples of Believers (O.Neugebauer & himself) who got way 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.R.Newton, upon whom they are proud to have done their own pioneering, in smear-creating Newton as the field’s cohering hate-object (Gingerich 1990 p.364; Schaefer 2002 p.40) — before, since his death, honoring DR by elevating him onto the same pedestal.
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 Almagest 7.3. In 2014, much-too-soon before the resulting paper Brandt et al 2014B went to press the Journal of Astronomical History & Heritage’s Editor Wayne Orchiston asked DR to referee it, 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 undeterred to determine 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/jauqk.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 delayed 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 own best interest) not to take all pains to provide a clean coherent and competent articles possible. (Not exactly an infectious ideal at brother history-of-astronomy journals, either.) The irony here (as is obvious from correspondence: fn 28): DR went to plenty of trouble in a cooperative, generous attempt to help JAHH 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 JHAsish 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 Almagest 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 article for bringing, 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-alibing 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 seriously submitted a paper, “Ptolemy’s Fraudulence” (§2 above), to the JAHH, whose chief, W.Orchiston (formerly established in Oztrollya, like JAHH, but lately translocated to Thailand) turned it over not to a specialist in the relevant science.

A3 When asked to send Brandt his 3-decade-old star-declinations ms (later slimmed, revised, augmented with new discoveries of absolute latitudes, and published as Rawlins 1994A), DR took the time to profitably review his 1982-1994 conclusions, sending his further-revised 2011 thoughts in a letter, www.dioi.org/bjzg3.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 1994A had already concluded that +159 was the Clean Dozen’s epoch): for epoch +159 (unlike for +137) the split between Clean Dozen & SickSix stars was overlaplessly clean: §C17. (But Brandt et al 2014B didn’t cite any of this.)

A4 As we mourn the passing of technically able contributors to scientific history such as B.L.van der Waerden, C.Gillispie, W.Hartner, O.Neugebauer, C.Wilson, H.Thurston, A.Aaboe, R.Newton, & S.Goldstein, we realize that they are being replaced (as JHU’s Harry Woolf warned DR 50+ 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.

4 When asked to send Brandt his 3-decade-old star-declinations ms (later slimmed, revised, augmented with new discoveries of absolute latitudes, and published as Rawlins 1994A), DR took the time to profitably review his 1982-1994 conclusions, sending his further-revised 2011 thoughts in a letter, www.dioi.org/bjzg3.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 1994A had already concluded that +159 was the Clean Dozen’s epoch): for epoch +159 (unlike for +137) the split between Clean Dozen & SickSix stars was overlaplessly clean: §C17. (But Brandt et al 2014B didn’t cite any of this.)

10 Gingerich’s private ref-reports on DR’s work can’t resist personal remarks having no place in such. (Too remote from principled stands even to recognize one. OG didn’t actually comment on, only to dismiss with a sneer that DR wants to be shunned.) DR’s atheism heaps extra aggravation upon this self-described “practicing Christian” of the Mennonite cult. Students of the psychology of hate might profitably investigate this 40’s obsession: bizarre details (& Cardinal Manning’s perceptiveness) at JAHH 2 fn 5.

11 As Gingerich again&again for decades has invariably done with DR papers (fn 10), in referee reports for allegedly scientific journals. Yet archons keep right on seeking his Special Insights — on DR’s character instead of his astronomy.

12 One could add Swordlow to the list of dihardars, but not from admiration of Gingerich: fn 22.

13 In retrospect, it was predictable that Journal of Astronomical History & Heritage wouldn’t publish a paper showing its recent 2014 BJZ article was false in claiming Ptolemy’s star-observer. Editor Orchiston’s demand for revision looked like the start of an endless, wasteful game of never finding DR’s requested self-censorship sufficiently adequate. (The theory that the paper was doomed from the start was confirmed by test in the next-last paragraph of www.dioi.org/oww2u.pdf, unsurprisingly.) Since a durable cult lie (Hoskin to Thurston 1986/95) is that DR is “impossible to deal with” (contra DR cooperation with, e.g., Polar Record [Univ Cambridge] & Griffith Observer), it was presumably believed that this approach could kill the paper while never appearing to censor anything. Note that if the editor objected to parts or words (he, not DR, knows his tastes in this regard: § fn 100), he need only have taken up DR’s 2015/9/30 suggestion, at www.dioi.org/jav9u.pdf, to strike such (isn’t this what competent editors do?) & for are — the job would’ve taken ordinary an hour & sent the revision back for DR’s OK; but, then, what if DR had replied “Done”? Fixers keep their plans flexible.
Ptolemy observed, playacting which is intended to prevent the public from catching on to the field’s long-running thespian obfuscation 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 squushable plant, indeed, regularly planted during a half-century of establishment insistence on transforming 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 enjoying the decades-long history of this desiccated field’s transparent pretend-conviction (fn 50) that promotion of planted apologia for Ptolemy’s observership 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 “premiere”15 journal of its field, has for decades used pseudoscience and deception in the shall-we-just-say extraweeeeeemly 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 long-time #2 official, Harvard’s Astronomy 101 teacher & deft Disraeliesque trowel-wielder Owen Gingerich, has repeatedly billed16 this ancient faker and superstition-peddler17 as “The Greatest Astronomer of Antiquity”, claiming that all who question this evidently-sensible-to-him proposition are the insane18 parties to the simmering-if-generally-suppressed dispute inevitably triggered by such superlative salespyle. We will first briefly examine a sample of the succession of car eerist soldiers who disgraced 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 archbish ops, 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 1°1/2! — 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 V or a fraction of an

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 \textit{fn 6F}. To enjoy Rob’t Peary’s like supplications, see www.dioi.org/cot.htm#dtrw.
17 Ptolemy authored astrology’s bible, the Tetraabiblos, 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&79 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 Psychoanalyst Gingerich’s intended-to-be-revealing 2000 referee report to Isis (outted in Rawlins 2003X) called Ptolemy-skeptics just a tiny bunch of paranoids — thereby inadvertently and delusionally smear ing most of the scholars in the field, even WHILE he is echoed in the 2015 JAHH referee report’s complaint that DR \textit{doesn’t respect those who disagree with him}. Can it get any weirder? Well, actually, yes. As we see from www.dioi.org/pm2.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.dioi.org/pml.htm, applied to himself.

(On the reality of cohesive shunning of Ptolemy-skepticism: see §3 fn 6.)
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&unquestioned 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. The 1° was misconceived at a juniorhighschool level. The 2nd was a clumsy fantasy.19
2. Near a solstice, Nature alleges it’s 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 referreeing 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?

Swerdlow’s fantasy for explaining why Ptolemy’s four solar “observations” were (§2) scores of times nearer Hipparchos’ indoor solar tables than to the outdoor Sun: all ancients selected outdoor data to agree with indoor theory. (N.B.: This would naturally justify destruction of the unused data.) Comments: [a] Even if it were true, the preferred alibi wouldn’t explain physically-impossible repeated 1°-off-the-mark Ptolemy “observations” that could never have been made outdoors in the 1st place! Especially again&again&again&again. (The human eye can see to about two ordmags better: roughly 1°, and the solar semi–diameter is 16°, so his equinox-solstice errors average about 4 times the distance from the Sun’s center to its edge: (§B2). [b] Further, we know that 2nd century BC Greek scientist Hipparchos reported real observations which disagreed with his theories and with each other (§3 fn 8); thus, faking or selecting data was not genuine ancient astronomers’ normal procedure.
[c] So many accurate Greek astronomical achievements (e.g., lunar distance

19The deception has become deliberate because [a] the plain Hipparchan counter-evidence (item[2] at §B3) was sent to the journal before publication, and [b] has been known to the perps for all the decades since, causing not the slightest retraction.

20 The incredible reasoning of Swerdlow (MacArthur&PhiBetaKappa!) is examined at R.Newton 1991 fn 20 and Rawlins 2018U §B2-B3. He and J.Evans continue (in ignorance of both the observing technique and the historical record: details at §3 fn 96) to insist that solstices could not be measured accurately compared to equinoxes, despite several inconvenient facts:
[A] Outdoors Hipparchos’ solstices are about 4 times more accurate than his equinoxes: 12° [N7]. [B] More expert at the relevant science than certain modern wannabees, all ancient scientists used solstices not equinoxes for gauging yearlength. (Enumeration of these at idem; sources: ibid fn 11.) [C] Not even recent miraculous recovery of the 1900-‘old paperus P.Fouad 267A, with solstice’s time correct to ordmag 1° (actually to a fraction of 1°, by chance) has yet enlightened any cultist.

See Rawlins 185U for full details of ancient solstice-determination, and DIO’s new formula (ibid §H) pricing ordmag 1° errors in such, inevitably due to asymmetry from Earth-orbit eccentricity, errors which Swerdlow&Evans couldn’t even quantify, ere so prominently (JHA & Oxford Univ Press) displaying their own [A]&[B] double-ignorance, Swerdlow of course adding a (pricelessly ironic) sneer of imagined superiority: R.Newton 1991 fn 20.

For almost 40’, virtually all journals in receipt of a DR paper on antiquity have not had the imagination to start elsewhere than Gingerich, when seeking refereeing, e.g., PASP, JHA, Isis, Nature, JAHH. Most, to their credit, later ignored his slander as irrelevant to the content, sought other advice, & published. The most grovelingly slavish — and the least concerned about veracity — were naturally also the least technically qualified (adamantly spurning politically-unacceptable expertise, by forefoot-cutoff of correspondence): JHA and JAHH & [Ists].

21See ScAm 1979, quoting Swerdlow & Gingerich, but primarily dependent on Swerdlow, as DR learned directly from the piece’s unblilled writer, Paul Hoffman, along with Swerdlow’s and Hoffman’s private opinion of Gingerich — which agrees with that of most of the working scholars in the field, especially the best.

22Ever-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 §2 [L].

23Don’t miss www.dioi.org/pm3.htm, longtime (1970-2013) “premier” Journal for the History of Astronomy Founder-Editor Michael Hoskin’s efficiency: refereeing&verdict 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 impotency has carried its information into lapdog ever nearer the ultimate intimacy it aspires to: lapdancing a needy establishment.
to $\Delta \lambda = -29^\circ \sin \lambda \tan \beta$, which gets substantial in the north. In the star catalog no such $1^\circ/2$-amplitude waves exist, so neither did an outdoor star-collecting Ptolemy. Of course, ever-openned Ptolemists 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-crank-friend F.Cook, MacArthur-Genius N.Swerdlov 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 undergraduate math: celestial (DIO 3 §[A2] (d)) or terrestrial (DIO 21 §[C1] longitudes’ conversion to great-circle measure entails multiplication by $\cos \beta$, thus $\cos \beta$-cir $\Delta \lambda$ can’t exceed 29$^\circ$. Swerdlov’s response: he hides (DIO 3 §[S3 §4]). Meanwhile, Evans’ attack on the ([B5]) absent-error-waves argument confused sine waves with cosine waves ([2 $\S$11]) blowing off a 63$^\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$\S$1/2 he sought.

B6 In the 1987 JHA, Evans took his 1$\S$1 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 [Rain Newton 1977: deftly summarized by Thurston 2002S], acceptance of even this weak possibility was about all that Ptolemists could hope to accomplish among informed scholars. Still the case: e.g., Brandt et al 2014B.)

[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’ (Evans 1987 p.275), proving Ptolemy-skeptics were wrong to deny that ordmag 1$^\circ$ errors were not unusual for ancients. However, Rawlins 1991W fn 288 and Rawlins 2009$^e$ later showed that if Evans’ reduction hadn’t bungled his lunar parallax correction’s sign, the error would’ve been merely ordmag 1$^\circ$. 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 investigation entirely [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 1977 eclipse he hadn’t outdoor-measured with) — and continues inexcusably contending$^{25}$ for ancient errors of ordmag 1$^\circ$.

[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 instead stolen the

$^{25}$ Seeing his own 1981/7/16 record proved DR right on that observation, Evans 1998 nonetheless repeats his 1987 argument that Hipparchos’ ordmag 1$^\circ$ 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 he hugely erroneous placements of Spica were merely bunglings of raw observational data accurate to ordmag 1$^\circ$. Later, Rawlins 2009$^e$ [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 result turned out to be merely ordmag 0.1$^\circ$. Evans’ further (totally unmentioned) 1981–1977 eclipse switch renders it undeniable he’s seen DR’s detection of his filter. Has he found fault with it? No. So he & his equally colleagues combine to fake that detection’s non-existence, by cultwise noncitation. 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 academia, look away for 1/3 of a century, & still counting. Understand the stakes here: Evans’ 1987 JHA & 1998 Oxford University Press outdoor “evidence” & sermon constitute the most prominent & solid-tooth of all arguments for archons’ precious central myth of ancient science’s fumbling non-empiricism.

$^{25}$ Maeyama 1984 is graphical by trial&error and is monovariate ($J^3$ fn 100), finding nearly accurate values for $E$, though with estimated (nonmathematically guessed) standard deviations $\sigma_E$.

$^{27}$ Maeyama 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 suggestive of 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 concept of finding the 4 observers’ latitudes & latitude-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 collections showed that their observers knew their geographical latitude $L$ to ordmag 1$^\circ$. Seen [of] below.) Or why-
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 & Aristyllos (the split 1st statistically proposed in Rawlins 1982G). DR was asked to referee the paper: DIO’s report, www.dioi.org/jau8q.pdf, is on the DIO website (as are our letters28 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/jau8q.pdf, also emphasized that the paper should not suppress a few extremely germane items, fully known to JAHH (through the referee-report), which point in a direction other than its inexplicable Ptolemitic conclusion. (Though R.Newton is cited as a skeptic on Ptolemy, none of his or DR’s damning evidence appears 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 Almajest 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 δ 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 δ 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 δ data contemporary with Ptolemy, all analysts since (including Brandt et al 2014B) have concluded that there is but tiny error (ordinating \( L \)' 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 \( L = 30^\circ58' \) (Almajest 5.12-13), which rules him out as the declinations’ observer since this is in error by \(-14^{\circ} \) (Alexandria being at \( L = 31^\circ12' \)).

[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 & Aristyllos reported thusly at Brandt et al 2014B p.334, www.dioi.org/bzj.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 becomes even harder to explain when we find that the earlier, refereed (otherwise nearly identical) version of the paper, www.dioi.org/bzj3.pdf, has the verbatim-same wording except for the citations, which were expunged completely chronologically back then: “Rawlins (1982, c.1983, 1994); Maeyama, 1984.”28


29 Were there a case for a +57 observer, the most tempting identification would be Heron, who recorded an Alexandria-midnight +62/3/13-14 lunar eclipse. But, except in the minds of the most refined of Ptolemy’s alibi-artists (don’t miss JHA Editor James Evans at (‡2 fn 11), the case for non-fabrication vanished long ago: §B6.

had they faked them by adding 2 2/3 centuries worth of his false 1’icy precession, namely, tackling 2’40’ onto all Hipparchos’ stellar longitudes. (Closely agreeable date, assuming the star-declinations were faked similarly.) No mention of this Coincidence in Brandt et al 2014B! — though urged by referee DR. It has been explicitly well-known for over a century that virtually the same date matches the date for which Ptolemy’s 1025-star Catalog’s fakes would seem correct, were they real, which few scholars believe anymore. E.g., Peters & Knobel 1915 p.15 noted that +58 is the date when Ptolemy’s misprescribed 1025-star Almajest 7.5-8.1 catalog is correct. There is no sign that BZJ knew of this match prior to DR’s referee report, nor does such vital information appear in Brandt et al 2014B subsequent to it, either. This positively belongs at the paper’s p.333, where we instead hear that besides the star-declinations (empth added): “Ptolemy offers additional evidence for his [(false) precession] value elsewhere in the Almajest (e.g., [Toomer 1984 p.1338])” — innocent of the A.Jones-witnessed fact that upon viewing Graffhoff 1990’s evidence decades ago, Toomer agreed that the Catalog stars came from Hipparchos — which renders irrelevant the faked “Ptolemy evidence” cited to Toomer 1984 loc cit. The DIO ref-report asked: “So are we also to ascribe the Catalog to the same secret observer, who thus must have created a catalog of over 1000 stars though no one ever mentioned his or its existence?”

[D] For the δ data Almajest 7.3 gives for Ptolemy’s era, there is (in residuals) a fully clean split (perhaps unknown before DR’s 2011 letter and 2014 ref report to BZJ), between the suspect δ star-declinations δ which Ptolemy analyses, and the unsuspect 12 data (which he doesn’t analyse in Almajest 7.3) — if one adopts the independently-arrived at epoch \( E \) (+159) and geographical latitude-error x (+4’) already found through bivariate analysis (Rawlins 1994AL) upon the unsuspect data, without any selection among or tampering with these 12 data, long-separately-recognized and separately treated by Ptolemy. Significance of this for Brandt et al 2014B’s peculiar new split is emphasized below at §C21.

C6 JAHH readers have a right to know C5’s four central considerations, but Editor Wayne Orchiston (WO) has notfelt the need to inform30 them. So much for the integrity of the paper’s Ptolemitic conclusion. And of the Journal of Astronomical History & Heritage.

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 really31 just a try (like Maeyama 1984) at solving a bivariate problem monovariately. Except for Ptolemy (where different samplings32 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 recommendation33, www.dioi.org/jau8q.pdf, before publication (compare www.dioi.org/bzj0.pdf, vs www.dioi.org/bzj.pdf, for Aristyllos). A peculiarity of Brandt et al 2014B is that values for x & its standard deviation \( \sigma_x \) are not given anywhere: not even when BZJ attempts recounting and repeating Rawlins’ analysis (which explicitly supplies and tabulates both \( x \) & \( \sigma_x \)). Instead, BZJ present miscalculated (§C9) values for a single entity, “accuracy”, which they confusedly seem to regard as sufficiently equivalent.

C8 The DIO referee report warned34 that the “accuracy” values given at Brandt et al 2014B p.331 are “astonishingly low”.

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, it has become excruciating for silence in the text (which is all most readers see) on the undeniably indicative items listed above at §C5.

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

32 Slightly true also of Hipparchos, where Rawlins 1982G & Rawlins 1994L included two stellar declinations from non-Almajest sources, a supplement 1st suggested by H.Vogt.

33 BZJ were helpfully provided sufficient advice to inspire proper caution: [1] were given all the right answers for \( E, \sigma_x \), 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.
C9 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 residual from each datum, [c] with the adjusted data, re-computing the problem nulli-variatly for an independently estimated best E, [d] computing “accuracy” rs by means of the minuscule leftover residuals. (Our reconstructions of data via this procedure are in fn 35.) Perhaps we could dub this the “least-nonz-squares test”. The impossibility of BZJ’s numbers are easily seen: when Brandt et al 2014B p.331 puts the “accuracy” of Aristyllos & Hipparchos at 0°,004, and calculates to 14&11 ARCSINDEG, resp — obviously a fantasy (and BZJ were warned of this on p.4 of www.dioi.org/gauk.pdf, DR’s invited referee report), considering that the data’s rms is admittedly 0°,1 on the same page: Brandt et al 2014B’s Table 2. (Equally incredible: idem lists rms values for methods of Maeayama, Rawlins, & BZJ — that agree with each other to a 1000th of a degree!)

C10 Interlude: From where did BZJ get §C9 [d]’s wacky idea that one should simply mean the residuals? Answer: from misconstruing p.283 of their preferred prior analysis, Maeayama 1984, where it is stated that (following determination of epoch E), “the epoch [where S is minimal] will be our first approximation. The resulting mean deviation at this epoch will then correspond to the mean systematic error...” Brandt et al 2014B mis-read this as referring to a simple averaging of leftover residuals. BZJ’s procedure and cue from

34 In addition to the reversal-test revealed at §C11 showing the invalidity of the paper’s method — there is this equally obvious consideration: in Brandt et al 2014B p.331 eq.1, the coefficients of the unknowns are 1 and 0.33333333 α. The rms value of the latter in these investigations is about 1/4, so the standard deviations for x and E should exhibit a ratio of about 1/4 in L to 4° in E. In Rawlins 1994AL Table 3 and below in Table 1, this is roughly true. But no such symmetry appears anywhere in Brandt et al 2014A’s abstract or Brandt et al 2014B. (N.b. of the paper Zimmer et al 2013 admirably takes no part in evaluating anyone’s x — or anything at all about Ptolem.)

BZJ’s initial abstract, www.dioi.org/bzj1.htm, Brandt et al 2011, gave figures for “accuracies” (where we flip BZJ’s unconventional C-O signs): Timocharis E = −295, 11 stars rs = −0°.022, Aristyllos E = −258, 6 stars rs = +0°.004, Hipparchos E = −128, 18 stars rs = −0°.010, Polenoy E = −115, 18 stars rs = −0°.005. Later, Brandt et al 2014A p.6 & Brandt et al 2014B p.331 give different rs: Timocharis 0°.012, Aristyllos 0°.003, Hipparchos 0°.004, Polenoy 0°.009. Our speculative reconstructions (via §C9’s [a]-[d]) alter the experiments but (in a delicate problem) get agreements with some among BZJ’s false rs values. So maybe this or something like it was BZJ’s procedure?

[Accurate bivariate least-squares result follows each observer’s reconstructed BZJ data, in brackets; plus actual minimal residual-square sum S_m, to show that most BZJ solutions do not approximate it.]

Timocharis 12 stars: E = −295, rs = −0°.022, S = 2745.2

\[E = -277±18°, x = -0°.076±0°.077, S_m = 2441\text{r}^2\]

Aristyllos 6 stars: E = −258, rs = −0°.003, S = 1475°.2

\[E = -258±10°, x = -0°.016±0°.045, S_m = 1473\text{r}^2\]

Hipparchos 17 stars (Aliont δ = 6°30’): E = −128, rs = +0°.004, S = 446°.2

\[E = -133±8°, x = -0°.001±0°.021, S_m = 392\text{r}^2\]

Polenoy 18 stars E = +111 (Brandt et al 2014B Fig.5 no-prop-mot), rs = −0°.005, S = 2539°.4

\[E = +115±13°, x = -0°.004±0°.052, S_m = 2521\text{r}^2\]

In his 1983 Aarhus talk Maeayama did not yet know that “mean systematic error” relates to error in the observer’s assumed latitude. He later dismembrered that he learned this from DR’s ms: fn 27 above. The results displayed at Maeayama 1984 p.292 Table 1 are not from bivariate but monovariante least-squares — and not even via calculcus: just by graphing trialerror to find S’. Nonetheless, the values found for x (though not recognizing it as latitude-error) and E are roughly correct, since Maeayama in-effect was running a double-monovariante test and had the good fortune that the unknowns’ correlations were not too serious. And at least (unlike BZJ) he realized that the leftover residuals after the 1st monovariante test were to be fed into the 2nd such, to find the value of x which minimized the sum of the residuals’ squares. However, for x’s standard deviation σ_x, Maeayama 1984 Table 1 column d wrongly lists σ_o, the mean error of a single observation. The resultant errors range as high a factor of nearly 7 (the Hipparchian 44-star sample).

Maeyama are clearly free 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 σ_x — neither even mentioned by BZJ? While Maeyama’s 1984’s estimates of σ_x are (fn 36) off by serious factors, Brandt et al 2014B’s misconception produces nothing at all! — no σ_x whatever.

C11 The invalidity of Brandt et al 2014B’s procedure (above, §C9) is easily established by performing it in reverse: assume an E_0 (instead of an x) and solve for x via monovariante 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 for E_0 — or anything assumed in its place. So, by the same reasoning Brandt et al 2014B used for finding rs, we must conclude that all starting values for E_0 (before launching the foregoing monovariante analysis) turn out to be erroneous.

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 σ_x through just dividing σ_o by √n.}

C13 In a true bivariate solution, e & x are least-squared simultaneously. 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 §3 Table 2 above, except for Timocharis] barely differs 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 Potenly’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 5°. That was why Rawlins 1982G — whose main analysis showed 1° precision in the solar transit work of 3rd century BC Alexanderian 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 1st 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 §311: Rawlins 2008S). DR’s papers have called L’s error x, and epoch-error e (epoch E solution minus tester’s assumed E_0); if the mutual solution is done truly bivariately, it will find not only e (thus E) and e’s standard deviation σ_e, but x and its standard deviation σ_x, as well as the single.datum standard deviation, σ_o. 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., monovariante finding that E where S is minimal, S_o. (And the archons of history-of-ancient astronomy imagine they have the capacity to judge, shun, condemn, & censor the scrupulous, competent researches of scientists in such matters?) Note the parallel to the Journal of Astronomical History &

35 Note problems at fn 42 & esp. fn 45 if done otherwise. Monovariate tests might successively minimize the squares of the residuals by finding the e that does so, then the x that does, etc, whittling S into ever-smaller remoteness from minimum, at each cycle. This would be the cumbersome, tediously-iterative serial-monovariate approach. But why not just elegantly solve x & e at-a-swoop (with trivial iterativity from non-linearity), with total exactitude, through true simultaneous bivariate least-squares — as was done back in 1982&1994, and here in Table 1.
Its Error $x$

<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>
</tr>
</thead>
<tbody>
<tr>
<td>Timo</td>
<td>$-302\pm0^8$</td>
<td>$31^{12}^9$</td>
<td>$+1^{.5}^{\pm1.4}$</td>
<td>$31^{10}.5^{\pm1.1}$</td>
</tr>
<tr>
<td>Aryst</td>
<td>$-258^7\pm10^9$</td>
<td>$31^{15}^4$</td>
<td>$+1^{.0}^{\pm0.2}$</td>
<td>$31^{14}.0^{\pm2.7}$</td>
</tr>
<tr>
<td>Hipp</td>
<td>$-131\pm0^5$</td>
<td>$36^{08}^0$</td>
<td>$+0^{.2}^{\pm1.2}$</td>
<td>$36^{07}.8^{\pm1.2}$</td>
</tr>
<tr>
<td>Anon</td>
<td>$+159\pm0^9$</td>
<td>$31^{15}^4$</td>
<td>$+4^{.4}^{\pm2.0}$</td>
<td>$31^{10}.6^{\pm7.0}$</td>
</tr>
</tbody>
</table>

\[ \text{Historical case at hand: even after the answers are discovered and computed for them, some historical journals just can’t cope.)} \]

\[ \text{C16 In 1994, 12} \text{ later than 1982, DR discerned a new method for finding each observer’s assumed geographical latitude: from nulls in his data’s fractional-endings’ frequency-profiles (as explained in Rawlins 1994L §F) which, by subtraction of $x$, easily produces each observer’s absolute actual latitude} \ L. \ \text{All four least-squares-fitting} \ E \ \text{and epochs} \ L \ (\text{Timoclas 11 stars; Arystyllos, 6; Hipparchos, 19; Anonymous, 12}, \text{along with their standard deviations} \ (\sigma_E \ \text{and} \ \sigma_L), \text{as well as single-datum standard deviation,} \text{raw (}\sigma_0\text{)} \ \text{and with the effect of rounding} \ \text{removed (}\sigma_t\text{). All these desiderata are produced here in Table 1, slightly improved (see fn 38) vs the values of} J3 \text{ Table 2 above or Rawlins 1994L Table 3. Strangely, Brandt et al 2014A, the refereed version of Brandt et al 2014B, www.dioi.org/bzj0.pdf, claimed that Rawlins 1994L had latitudes “close to our values” — this, even though BZJ have to this day never solved for any of these latitudes. So www.dioi.org/jau8q.pdf, the DIO 2014/8/26 referee report, at pp.3-4, suggested that this point be clarified and that Rawlins 1994L’s $x$ values and absolute $L$ values for all four observers be printed, since BZJ had brought up the point, and had supplied various of DR’s other numbers (the majority correctly); preferably along with a sentence on the novel though simple means which did have invented while pioneering this entire line of inquiry. But, probably because DIO’s $x$ values especially & hugely disagreed with JAHFu’s “accuracy” values, the published article did none of these things.} \]

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

\[ \text{38} \text{The sole non-fit for the dozens of data in the nulls experiment was Timoclas’ Aldebaran. Rawlins 1994L fn 39 suggested that the original North Polar Distance may have been 81°1/15, recorded (conventionally for unit-fractions) as 81°15, but later misrecognized (like} J3 \text{ fn 44) as 81°15 arcmin, thus} \delta = 8°3/4\text{, as at Almajesty 7.5. Thus, reconstructed true} \delta = 8°14/15 \text{or 8°56′, which also shrinks a poor result. And Arcutus obviously bears a 1′ scibal error; restoring the original and eliminating outsized-residual for Zubeneqenubi (slightly-rounded} \delta = 5°3, \text{we have the Timoclas entry in Table 1 here. (For Timoclas’ results based on non-scribal error, see J3 Table 2.)} \]

\[ \text{39Timoclas & Hipparchos used a precision of} p = 12 \text{ intervals/degree; for Anonymous,} p = 8; \text{Arystyllos,} p = 4. \text{The inverse of} p \cdot \sqrt{12} \text{is the rms of the effect of average rounding, in degrees.} \]

\[ \text{40 We thank Jack Brandt for rightly urging use of modern satellite-based star-places. Versus the Rawlins 1994L results: the maximum effect on epoch} E \text{ was} 1°15; \text{on} L, \text{just a fraction of} 1; \text{but the improvements are welcome.} \]

\[ \text{41 E.g., at Brandt et al 2014B p.331, for all 3 observers, our 1982 ms’ epochs} E \text{ and} \sigma_E \text{are relaxed, conspicuously omitting our} \ x \text{and} \sigma_x. \]

\[ \text{42 The errors&oddbities in Brandt et al 2014B’s sinuous process of defining their 2 groups, “L” and “E”, are explored at www.dioi.org/jau8q.pdf, DR’s 2014/8/26 referee report. E.g., one of the groups (E) covered less than 1/2 the sky longitudinally, which is not a recommended sort of sample when trying to avoid bias. One of the most revealing peculiarities is elimination of three “unhelpful” stars, Betelgeux, Aldebaran, & Sirius on the ground (www.dioi.org/bzj0.pdf, refereed version) that they change slowly in declination. The DIO referee report advised that Aldebaran’s declination-speed was} \]

\[ \text{which by either version of the paper were previously unknown to Ptolemy or anyone else. The p.332 grouping is E (Early 6 stars) and L (Late 9 stars), which does not follow the traditional split, namely: the “SickSix” stellar declinations (which Ptolemy “deduced” his false precession from) versus the “CleanDozen” real declinations (which his precessional math ignored): “our groupings have no simple connection to Ptolemy’s selected six stars” (Brandt et al 2014B p.334). Why? Well, R.Newton 1977 pp.220-225 rightly argues that Ptolemy typically fabricated the SickSix from 1’cyc precession in order to equally-typically then “prove” said precession from the fabrications: \text{22} \text{§B2. The new grouping would undercut this view by fracturing the SickSix. Problems with the new split (as noted in DR’s 2011/3/15 letter to Brandt): the traditional split is simple, is Ptolemy’s own. Further, DR’s 1994/adopted +159 epoch (Rawlins 1994L fn 45: all 12 Clean Dozen stars with no deletions) was understood in 2011 for the 1st time to establish no residuals-overlap (fn 7) in the Clean-Dozen-vs-SickSix split, a finding nowhere cited in Brandt et al 2014B, which keeps mis-rendering DR’s also-uncited groups (after dropping three stars at p.332, then a reshuffled four at Fig.10) — groupings} \]

\[ \text{43 But headline O—C. The various Brandt et al 2014B Figures confuse O—C (Observed-minus-Calculated) with C—O (evidently a routine weakness among historians-of-astronomy [though here fortunately harmless], e.g., \text{12} \text{§F8, & www.dioi.org/iff.htm#bvnm). And Brandt et al 2014B’s eq.1 (p.331) is founded upon a confusion of errors with residuals (possibly just a misunderstanding of the Rawlins 1982 ms’ eq.2), thereby equating Observed-minus-Calculated with what is actually just Calculated. If taken seriously, this makes Observed equal to twice Calculated.)} \]
Clean Dozen, eliminating ever-problematic\textsuperscript{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\textsuperscript{45} being 17°. No overlap at all. A lovely split. So there’s just no need\textsuperscript{46} to get fancy over dividing the “Ptolemy” 18 stars. Unless one is extremely, extremely determined to undermine acceptance of R.Newton’s 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 preferred by Brandt et al, since they give results in agree with R.Newton’s\textsuperscript{3}, 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 obviously degrade 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/ff.htm#twsa), Brandt et al 2014B tries including weights by slopes’ absolute magnitudes (p.331 & Fig.6), the kind of Legendrian primitivity 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.

C20  The paper concludes with a long, illustrated section (slightly altering §C17’s L-vs-E regroupings that replaced Ptolemy’s simple split) which tests for clusters in stars’ crossing-times. This is a patently poor basis for eliciting anything valuable, for the obvious reason that the crossing-times’ reliabilities are highly disparate (§C19), due to slopes that vary from nearly the full possibility (00.3383/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 [§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 (nearly horizontal) in Fig.7 magnify a tiny difference (just a few arcmin: less than $\sigma_5$) 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 accidently 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.dioi.org/jau94.pdf, to correct its repeated misreading of Rawlins 1994L’s date for (Anonymous’ ‘Clean’ Dozen stars) as $+131$ instead of Rawlins 1994L, Table 3’s actual published value, $+159$. This is a 28° difference, which matters, as we saw (p.47) & www.dioi.org/gfe03.pdf, in agreement with the sum of residuals-squared, will be found to have declined (comparisons in fn 35), showing that Brandt et al 2014B’s elaborately-derived unorthodox groupings.

(C22  A shock that might give historians-of-science pause before continuing weird pronouncements of the myth of observer Ptolemy: the declination issues was solved with full competence over 30 years ago, by R.Newton & D.Rawlins. The 2 papers historical journals (Centaurus & JAHH) have published on the issue since have just messaged it up some, while discovering nothing new that’s valid. Indeed, as seen from §3 Summary’s conclusion (p.47), R.Newton would judge these efforts primarily “subtractions from the sum of human 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 advent, 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, $S_m$. (Which is the square of the appropriate $\sigma_5$ 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\textsuperscript{46} $x$ 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 $x$-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 $\hat{e}$ 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 $c$.

Our solutions for $x$ are just 0-4 arcmin. The size may be small, but the issue isn’t: the
central point here is (as 1st revealed in Rawlins 1982G) that ancient scientists found L to ordmag 1º accuracy. For that reason, as well as Brandt et al 2014B’s p.331 advertising 1º accuracy, the most precise solutions for x are appropriate. This becomes important (fn 45) for the Clean Dozen, where x = 4º, closely reflecting the error in the observer’s adoption (independently demonstrated in Rawlins 1994L §B6 fig 3 = L = 31º 1¼ for Alexandria, which is 3º (close to 4º±2º) larger than the reality: L = 31º 12’.

To go further, in order to find an integrated-probability 2σ locus in x-σ space, the student might profitably consult www.dioi.org/biv.htm#bivld.

D Watching a Cenental Field 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 48 — sometimes (e.g., fn 18) even vicious — stubbornness50 of those determined to protect Ptolemy from public exposure by any means (a4, fn 13, below101) is that they are not protecting the myth but their gooroo — 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 §A2 & §M2: “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 §A2 §M3 & fn 52.)

D3 Being politicians, Ptolemist archons are the sort of people whose idea of intellectual engagement tends (for obvious reasons) not towards weighing scientific arguments but to [1] slaughtering (§B fn 5) their opposites as fools, knaves, and nuts (before discussing evidence — if ever doing so at all), while [2] pointing 49 innocent onlookers to the bemedalled, Reputable people who’ve taken their side: after all (as we ask at above p.57, in the Text-For-The-Day intro to this article), how could such cynosurae seem so Enormously Stupid as they must be or act, if skeptics are right? 51

D4 Well, here’s exactly how: just [a] keep smearng heretics behind their backs (details & photos at §B1 above) while continuing to [b] publish pseudo-defenses of Ptolemy’s honesty — no matter how ridiculous (§A3 fn 66). Meanwhile, disallow — as too Disrespectful 51

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 outr´e extremes like competent 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#gles): a man who can’t be bribed can’t be trusted. 49 Sixty-two percent of ancient historians have claimed that ancient historians do not allow for the possibility of criticism in their works. 50 Without citing the various powerful evidential proofs that Ptolemy stole the star catalog, Schaefer 2013 p.47 instead repeatedly resorts to sociology to aver that we can’t KNOW so because herd-loyal Ptolemites (like BZJ) 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 jollipoll Schaefer 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 (DR4), which extends aleg to many other falsehoods: “Why does anyone continue believing and/or their gooroo — resorting to any sloppy argument, an 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 §A2 & §M2: “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 §A2 §M3 & fn 52.)

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52 In mathematical history-of-astronomy, dimbulbs + careerists + thespians + poles now constitute a majority. If able, honest scientists ever rejoin & except the present era, it’ll be remembered, with eyes averted. In the Dark Ages, when reason was punitive proscribed in favor of invincible innocence, Archons will stoop to ANY tactic, to postpone that day indefinitely. Understandable. For them.
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Annals of Science (1996 July), reviewing DIO vol.3 (Tycho star catalog): “a thorough work . . . extensive [least-squares] error analysis . . . demonstrates [Tycho star-position] accuracy . . . much better than is generally assumed . . . excellent investigation”.
British Society for the History of Mathematics (Newsletter 1993 Spring): “fearless . . . [on] the operation of structures of [academic] power & influence . . . much recommended to [readers] bored with . . . the more prominent public journals, or open to the possibility of scholars being motivated by other considerations than the pursuit of objective truth.”