DIO
& The Journal for Hysterical Astronomy

Is a University Enhanced by a History of Science Department?

Weak Science and Data-Fudgery
Yet Useful Mining of Fresh Ore
Diller’s Perfect Fit Shunned 84
History of Science Society Theft
BreakfastLunch JHA Refereeing
$100,000 Prize EclipsePair Hunt
Alltime Clumsiest Ptolemy Fakes
Unseen Aristarchos&Archimedes

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$100,000 Award for Eclipse-Pairs Within Orthodox Time&Mind Limits. See p.82.

This DIO is formed of papers (1-3) on new discoveries and academic crimes, sent to centrist journals backed by major universities’ scholars (2, 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-Tweedily sneering in-effect: whaddayagonnadoaboudit? Has academe become home to those who ignore colleagues’ sins so long as their own funds flow? Papers 1&3-4 detail credit-theft from creators Gosselin, Delambre, Diller, Newton, Thurston by: mis-attack, lockstep-cult-noncitation, data-tampering, grab, shun, fake forever-controversy (2 §H2-H3, 3 §C10, 4 §B4), enduring knowledge-advances.

Leaving each paper nearly 1 as submitted entailed overlaps, but reflected how many forums ignore corruption & fleece demos of poor stats (3 §pp.44, §F8, fn 100, 4 §§C1&C19), abyssal science (2 §N7, 3 §J7), esp. scientists’ attitude. (Ponder Ragep’s inversion at §3 fn 99!) No hist.sci forum faces its field’s hijinks&ployinks, nor suggests remedy-path, despite their deep. See also www.dioi.org/cev.pdf, www.dioi.org/je01.pdf, on other two by treating a Pliny solar eclipse as lunar, and by putting Kleomedes’ Spain in the wrong hemisphere (likewise for Xi’an & Luoyang: see Table 1, §§C5-C11, resp). We value (e.g., p.44) History-of-science’s mining of new ore but regret when sacreagred minds warp its use.


This DIO’s & others’ bluntness has to do with why historians-of-science flee us. [1] Calm scholars John 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/bs.htm) shunned&snubbed 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/is.pdf, of an original DIO 2008 discovery (reserving for 7? in ordinal 100 libraries worldwide), & [b] its 2016 laughingly math-bungled (§1 §D) attacks on DR’s 1984 Greenwich Centenary conference paper; pseudo-refereeing of Isis’ assault is Hist.scientific, 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/jio.doc) of unrifled pp.46-85 within, whose history&science “history-of-science”-Isis somehow never commented on in five emails, Isis Editor H.Cohen wouldn’t acknowledge receipt of our protests against Isis’ 2015 echo of & 2016 attack upon DIO’s researches, instead emailing (pp.98&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/jh.doc, & pretend-refereeing (§3 fn 66) journals? Are they exaggerating our import by cowing so transparently? The reader may judge.

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

1 Curious examples of Shcheglov mischarges: [a] The simple-stretch idea is alleged (Shcheglov p.693) to bear logical fallacies; none are produced. [b] The stretch-solution is said (Shcheglov §1) to be “further studies.” Whose results can never work as efficiently as plain, raw multiplication.

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[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/ciev.pdf, is founded totally (abstract-to-conclusion) upon the theory that, though Eratosthenes’ legendary Aswan-Alexandria experiment yields Earth-circumference C = 250000 stades for the Sun at infinite distance, it yields C = 252000 stades if parallactically adjusted for Eusebius’ finite Sun-distance of c.100 Earth-radii. But this result had already been published in uncited “Eratosthenes’ Too-Big Earth and Too-Tiny Universe”, DIO, 2008, 14 §1 fn 6, www.dioi.org/jf01.pdf, explored as an alternate explanation, even though reasonably 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 astroglogers mangled them, accurate maps existed in antiquity with longitudes based on lunar eclipses. Shcheglov calls such maps a “delusion” since eclipse-use is “impractical”, citing in support “badly overestimated” (Shcheglov 2016 n.8) eclipse-based longitudes of Kleomedes, Heron, and Pliny. But Heron is long known to be irrelevant; and Shcheglov miscomputes (§D below) the other two by treating a Pliny solar eclipse as lunar, and by putting Kleomedes’ Spain in the wrong hemisphere (likewise for Xi’an & Luoyang: see POSTSCRIPT below).

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

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

B Shcheglov (p.705) calls early accurate geography “quaint illusion” — & his Abstract [captioned 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 ordmag-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 4.1 §3 §F; JAH 17 p.326)
0°:1 for star declinations (ditto)
1% for Earth-circumf. precision (ArchiveHistExactSci 26 p.216; DIO 14 §1 §A & eq.28)
1% for Earth’s tilt “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-radius vs really 60)
10° for lunar-eclipse-prediction (DIO 1.16 §3 eq.32)
1° for time of lunar eclipse (Greenwich 1984 in Vistas in Astronomy 28 pp.258&265)
0°:1 for lunar limb vs Sun separation (DIO 16 §1 fn 24)
ditto or even 1° for star-vs-eclipsed-Moon gap (DIO 1.3 fn 288; DIO 16 §1 §A fn 22)
1° for solstices (Bull.A.A.S. 17.2 p.583; DIO 20 §2 eqs.21&25Table 3; P.Foad 267A)
for –145/3 equinox on Alexandria Palaestra polar-stet ring (Isis 73.2 p.263 n.17)
10° for sidereal year (DIO 6 §1 fn 36§J&I; DIO 9.1 §3 Table 2; DIO 11.1 §1 fn 14-15)
1/1000th of Mars’ mean motion (Neugebauer-Cjeccam 1984)
0°:1 for synodic month (DIO 6 §1 eqs.2 & fn 12&18; DIO 11.1 §1 eqs.1-8)
1° for anomolfic month (DIO 6 §1 eq.13 & fn 12; DIO 11.1 §1 §A3 & eq.2)
0°:1 for draconitic month (DIO 6 §1 eqs.2&19 & fn 12; DIO 11.1 §1 eqs.1&3)

Most historians-of-astronomy are, like Shcheglov, unaware of these symptoms of high Greek geography’s accuracy, Gingerich&Swerdlow, speculating without attestation (1982) that scientists kept only theory-accordant data [flatly contradicted by Hipparchos’ record] (˘3fn 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 knowing on confirming prejudice? [Classic projection from own behavior? See below POSTSCRIPT’s final line.]

That ancient geographers’ longitudes were based on eclipses is doubted by Shcheglov p.690 as “too impractical”. I’ve outdoor-eye-ball-timed enough lunar eclipses to know their accuracy is ordmag 1° [anciently somewhat vitiated by sundial graduation limitations, ordmagrly] agreeing (at 4°/11°) with the well-under-1° accuracy of pre-stretch Geography longitudes, D.Rawlins 1985, “Ancient Geodesy: Achievements and Correction”, 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 impracticality, 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 33039/20 “Arbela eclipse”. Ptolemy’s reported time (longitude) gap is 4/3 too big, so Shcheglov’s n.8 tries albining 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°:6:7 pair: Pliny’s correct 3° Sicily vs Armenia).

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°22E are 35°75 apart. Shcheglov just mis-signed Cadiz and found 3°07’6” (comfortingly consistent with Ptolemy’s false 4/3 factor for his Carthage-Arabela gaffe-gap). As for Pliny 2.72.180, Shcheglov knows Campania & Armenia are c.2° apart. [Longitude gaps between Naples & the Geography’s Armenian cities (Diller DIO 5 Table 17; 1984) Dioskourias, Artaxata, Gaggera are 1°47’, 2°01’ & 2°23’, respectively, all indeed about 2°.] So Shcheglov concluded that Pliny’s 3° is too high. Yet Pliny’s just the Campania-Armenia 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 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.686, had verified Pliny, essentially 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-reports as 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 misassigned 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 Arbela, while 6 P.M. is expressed as a word (“moonrise”) not a number. Unequal to the Latin, Ptolemy thought 8 P.M. was Carthage time. Since his 4/3-stretched map already had Arbela 3° east of Carthage (real gap 2°1/4), he linked Arbela thusly: 8 P.M. + 3° = 11 P.M. That Pliny’s original astronomical observation was mislabelled in the Ptolemy table, & that Ptolemy’s time is wrong, that is, Ptolemy’s greatest astronomer of Antiquity? Ptolemy’s times mega-dissagree with not just reality but his own tables: 2°30’! [Error about as big as quantity sought: like ±3 fn 42.] Shcheglov notes no discords nor Arbela-Carthage-mixup, though all are at fn 45 of the same paper, www.dioi.org/jl03.pdf, he’s unheedingely consulting in his nn.12&15.

F On p.705, Shcheglov’s varied attempts at “rehabilitaion” include his pure guess that Ptolemy’s sources were bad (as if The Greatest couldn’t better discriminate): “it would be unfair to blame Ptolemy for his errors, because the whole tradition he relied on was a chain to the farrident Ptolemy, all by himself. If he was this

unreliable on his own, why doubt that, when switching to 500 stades/degree, he was so

safely perched on his source map’s longitudes were overland-distance-based, so

having his forty-star vs eclipse data.

Shcheglov’s other arguments all fall apart. See above 

**POSTSCRIPT**

2 Shcheglov’s solar-eclipse misadventures [are similar to NGS-NavFou’s at DIO 21 §§ §§B3-B4, www.dioi.org/j05.pdf, also recapitulating a backfired attempt to empirically justify demeaning Greek accuracy: Evans’ 1987 claim that his 1981 measure of a star’s distance from the eclipsed Moon was ordmag 1° off, like Hipparchos’ two bad measures of Spica. But undoing mis-signed parallax lowers all three 0°.6 errors to 0°.1 or less; same for Hipparchos’ −35° Regulus error, so odds against all four errors’ being outdoor are astronomical. Evans never reveal his 1981 data; but his 1998 book repeated his 1987 analysis, a non-observed 1977 eclipse quietly subbed for the 1981 event! Sly details at DIO 16 §1 §A & fn.7. Shcheglov, NavFou, & Evans have in common that all 3 are selftorpedoed by innocence re parallax, as was early Hipparchos (vs his consistently accurate later work: [J §8B,])
Such disasters warn of peril in history-of-science’s long-persistent glorification of Ptolemy as a scientist, while viewing his authorship of astrology’s bible, the Tetrabiblos, as a factor that only culturally and historically narrow scientists would be benighted 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.

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 ‘foundation’ 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 (Isis 1982; & DIO 1994, thrice cited in “Secrets”, which Shcheglov read), Y.Maeayama (Centaurus 1984), & J.Brandt (JAHJ 2014) show that all 4 real, non-ancient Greek scientists cited above at §3 [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 Geography’s mean latitude error of ordmag 1°! In response to the disjunct, one JHA Editorial Boardperson has offered that geographers must have ignored ancient astronomers’ (So, did astronomer-geographer Hipparchos ignore himself?) DR mathematically contends (“Achievement” pp.260-264) these lighthitho-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.

“Achievement” p.262 lists 17 cities where, in the Geography, latitude matches klima. Dropping notoriously-flawed Bithynia (DIO 20/2 3.4) & way-south Meroe leaves 14 cities.

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 grouped under it, its latitude made equal to exactly that city’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); different authors, in all likelihood. Effects of such data-tempering are obvious from errors found [in §3’s sample], while rms error 2° (as expected if due to real astronomers, like those of §3 [33]) 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 goofs. L

Given this mess, one might ask: who says there ever were accurate ancient maps? We reply by turning to the same 14 latitude-awful 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?

Dividing 4/3 into §5’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 7 appear from 8 cities’ Geography Book 8 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.

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 unstrretched 42° from Carthage 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 doctoring 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” §3 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 [111].)

S Given those Almajest tables’ Mediterranean 1°/4 klima-spacing: we can compute that the forced latitude-shifts would, for flawless cubbyholing, theoretically produce 0°3/4 rms error, ordmag-consistent with the 59’ median already found above (§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, but gains an astrophilosophical voice by instrumentation that Ptolemy’s occupation of it. Ironically, this well-intended discipline has long blinded the field to the obvious: just as his Tetrabiblos was his religion’s handbook for horoscopic interpretation, his Almajest and Geography were also world astrologer-handbooks. (The 1° fully competent translations of Almajest and Geography called each a “handbook”. Ptolemy’s exact title of what most now call the Geography was actually Geographical Directory, as DIO routinely calls it.)

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

V World maps interested navigators [Marinos?: §3 fn 105] & an expanding theocratic empire’s plagiarizing priests (e.g., Ptolemy) more than most commercial travelers. The huge factor by which 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 (Tyr, 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 was 2560000 stades (later nudged to 252000, perhaps for 700 stades/degree-convenience).

[ii] Eusebius’ Sun-distance, 40800000 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.

Ancient Accuracy Vs Hist sci Soc

Afterword: The History of science Society Hunkers and Bunkers

Due to cultish historians-of-astronomy, Greeks' patiently-won accuracy is unknown. (Perverse-ironically: it's widely believed that semi-literate Mesopotamians were better!)

Given Editor H.F.Cohen's haughty rebuff ([3 p.45], a Letter-to-the-Editor ([pp.3-8 here]), with cover letter www.dioi.org/isa.pdf, was sent 2017/3/20 to the 30-person Isis Editorial Board; separate emails to ordmag 10 board-members (requesting all 30 be informed of the letter), including Maria Portuondo (astronomer), 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 had told too "rusty": 2017/9/5.) Having heard from neither Editor nor Board, DR wrote the latter 2017/4/1, www.dioi.org/issb.pdf, hoping (emph in original) to encourage communication while correcting [Isis 107.4's] unfortunate December misinformation, unwary Isis publication of which might have been avoided, had Cohen possessed the humility to recognize he didn't understand Shcheglov's [2016 December Isis] paper except that it enticingly attacked one who was upsetting Cohen by asking Isis to publish too-accurate criticisms of his fellow poles. 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 covering up for not just Ptolemy but for Cohen, taking you all into hiding with him.

The Isis board's non-reply so far risks being interpreted as . . . doing nothing — about mere plagiarism, and miscomputed demeaning of accurate and scientifically refereed [p.45 below] Greenwich-Centenary scholarship.

Less speculatively, we know exactly what Cohen was up to, when he did not tell us he was sending the large paper [www.dioi.org/gjo.doc; now less diplomatically transformed into paper [3 below, here] to a referee until he got a negative report safely in hand [a report again not evaluating any scholarship]. Does he imagine such 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, 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 snears 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.

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 chauvinist battery of destructive, data-disregarding — even data-fudging — papers' . . . . (See [3 p.46 below].)

Whether the charge was accurate? The point held no visible interest at all for Isis.

The Greatest Faker of Antiquity: Still Foolin’ ’Em

On 2014/8/26&12/22, a somewhat restrained&spare version, www.dioi.org/pf.pdf, of the following paper was submitted to the Journal of Astronomical History & Heritage. Its referee report, while admitting our obvious expertise, included personal remarks (fn 1) echoing religious Ptolemeist O.Gingerich’s various past slanderous referee reports upon our work, naturally requesting removal of any questionable 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/oww3l.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 halfdozen simple independent evidences demonstrate that the history-of-ancient-astronomy subfield rulership’s decades-long insistence on the integrity of its ultimate icon Claudius Ptolemy has never been defensible by reason. Thus it resorted to other means. A sampling of subsequent chauvinist tactics provides an educational case study in how a subfield can be hijacked for the better part of a century by a determinedly-careerist cult, at the expense of the ability, skills, and tolerance characteristic of science, eventually more resembling a church than a research enterprise.

A INCURABLE DENIAL OF THE UNDENIABLE

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

B1 In the history-of-astronomy community, for nearly half a century, the issue of Ptolemy’s honesty, originality, and competence has been a trigger for blackballing (Rawlins 1991W [8]), shunning (Gingerich 1990; Schaefer 2002 p.40), censorship (Rawlins 1996C p.4, DIO 8 p.2), and intimidation (idend fn 1). E.g., the field-center and highly esteemed Journal for the History of Astronomy (JHA) may be just a minor shrink of perfection in its some don’t emplace with the laughter a genuine scientist can hardly stifle upon carefully examining the goal-directed imitation-science that is not merely charged here but is repeatedly proven in detail, throughout the paper, to be what has been consistently and exclusively used for decades to do what a once-ruling cult wanted done: make a case — any case (§H2 & §3 fn 66) — that Ptolemy was an honest, outdoor-observing astronomer. The referee imagines that the bad-old-days still apply, when Ptolemites could keep doubters from being taken seriously just by portraying them as those who had committed the crime of being “innovating,” and [h] of merely disloyalist June HJA (not H2) Ptolemy-plagiарizing from Hipparchos. But the original version of the paper [sent to Dr 1978/4/30], as submitted to the R.Newton-hating JHA, read instead [emph added] “Beyond the shade of a doubt this renders probable that the [Almagest star catalog] longitudes” were plagiarized from Hipparchos. But the original version of the paper [sent to DR 1978/4/30], as submitted to the R.Newton-hating JHA, read instead [emph added] “Beyond the shade of a doubt this renders probable that the [Almagest star catalog] longitudes” were plagiarized from Hipparchos. The referee’s decision is nearly an exact repeat of frenzied Astronomer Royal Geo.Airy’s equally revealing bowdlerization of “shadow” from his 1846/79 letter to Neptune-affair co-conspirator J.Challis: see Rawlins 1992W [J2 & Rawlins 1999W [H2]J. And for more unintended quasia-humor, we have the judge’s decision — as a seasoned (fn 5!) authority on non-insult etiquette and fair play towards “those with different views” — that the paper shows insufficient doses of the respect he believes is owed to those wainable-numerate historians-of-science who have repeatedly (J2) labelled physicists R.Newton and Rawlins as dishonest crazy incompetents and who have (fn 5) for years ducked debating Rawlins (compare to fn 6, below), though having the courage to serially portray his work in the most negative light to the extent of over 100 pages (1987-2008) in the Journal for the History of Astronomy, from which he has been banned (J81) for the last 1/3 of a century, during which time virtually none of Newton’s or DIO’s dozens of positive contributions to knowledge (e.g., R.Newton 1977 & R.Newton 1982, www.dioi.org/vin.htm) have been credited there, a blank entirely (J81) in current scientific certitudes&practices all too obvious from fn 5. Newfield’s theses 7 & fn 109.

B2 Unless CalTech’s Noel Swedrowski, not Jones, wrote this section of the preface — though Editor Jones is responsible for publishing it, while Jones 2005 p.21 knows that Ptolemy’s solar data were faked, and that his 180° ekumene was rigged (ibid p.55; Berggren & Jones 2000 p.76 n.53).

B3 The “premier” (Schaefer 2002 p.40) quality of the Journal for the History of Astronomy’s refereeing may be gauged from its often fascinating Original Science, e.g., here at §N7 and fn 14. Not to mention rocks at §I.2 and Pluto at fn 42. Non-approximation of these gems and the like fully justifies Rawlins’ banishment (fn 1) from JHA. As an example of just how “impossible to deal with” he is (1970-2013 referring (fn 3) but is alert to the fact that in can be in not permitting any author to argue in its pages that Ptolemy was a liar, even while allowing the defense clique to contend he was not (Evans 1993 p.145; Rawlins 1999 §§F7-F8). That the controversy has been rife with “unprofessional” acts, such as shunning, has been nationally published (Schaefer 2002 loc cit), followed soon after by a direct appeal (fn 35) to the head of the American Astronomical Society, urging supervision of its shamelessly shunning Historical Astronomy Division (H.A.D.), but the unprofessionalism of 2002 has only worsened since. History-of-ancient-astronomy’s lengthy communal monolithic and clienility insistence upon defensively maintaining — by character-assassination (fn 5) & the threat of exile (idend fn 1) for dissenters — an evidentiary-lying, perception-inverting (§N), logic-doubting (§N), holey-corpse 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 (idend) Ptolemy-skeptics a tiny bunch of paranoids for believing what he himself privately knows 8 to be true. Healthy restoration JHA Editor M.Hoskin, as quoted at Rawlins 1991W [B1]: he actually had the fantastic gall, in a 1980/7/30 letter to Hoskin, to suggest that the JHA was refereeing by the “swiftly-gauged, as against the substantial.” Considering the solid scholarship the JHA keeps publishing, like its unmatched discovery of the Winter Equinox, among so many other pearls (www.dioi.org/jha.htm#jx7), this advice was indisputably a “dumbed lie.” (So claimed patient, sedate, judicious [Schaefer 2002 p.40] Hoskin in his 1983/3/3 letter to hothead-horribilis [idend] Rawlins, announcing his exile from JHA, while suit-threateningly rejecting Rawlins’ gentle mathematical criticism of JHA refereeing for a 1982 October JHA paper — Hoskin’s letter mailed, ironically, just before JHA received the ethical author’s agreement that, after all, the paper in question was just as invalid as Rawlins had told Hoskin: see further, below.) The refereeing with fingie’s June HJA, where its refereeing is actually dream-contents (fn 5) that the view that Ptolemy faked or plagiarized is still today a way-out end-of-the-spectrum, a charge which uniformly classifies Ptolemy-doubters Dennis Duke (fn 23), Sam Goldstein, Gerd Grafthoff, Kimball Hansen, Willy Hartner, Alex Jones (fn 2), Charles Kowal, Robert Newton, Myles Standish, Richard Stephenson, Hugh Thurston, Gerald Toomer (fn 17), Bart van der Waerden, Curtis Wilson, and Don Yomeans (2005/4/28) as fringe! (Also Kristian Moesgaard, who is the 1st to realize the signicance of R.Newton’s fractional-endings test: J2). Moesgaard 1980C says of the R.Newton fractional-endings analysis: “This renders it probable that the [Almagest star catalog] longitudes” were plagiarized from Hipparchos. But the original version of the paper [sent to DR 1978/4/30], as submitted to the R.Newton-hating JHA, read instead [emph added] “Beyond the shade of a doubt this renders probable that the [Almagest star catalog] longitudes” were plagiarized from Hipparchos. The incident is nearly an exact repeat of frightened Astronomer Royal Geo.Airy’s equally revealing bowdlerization of “shadow” from his 1846/79 letter to Neptune-affair co-conspirator J.Challis: see Rawlins 1992W [J2 & Rawlins 1999W [H2]J. And for more unintended quasia-humor, we have the judge’s decision — as a seasoned (fn 5!) authority on non-insult etiquette and fair play towards “those with different views” — that the paper shows insufficient doses of the respect he believes is owed to those wainable-numerate historians-of-science who have repeatedly (J2) labelled physicists R.Newton and Rawlins as dishonest crazy incompetents and who have (fn 5) for years ducked debating Rawlins (compare to fn 6, below), though having the courage to serially portray his work in the most negative light to the extent of over 100 pages (1987-2008) in the Journal for the History of Astronomy, from which he has been banned (J81) for the last 1/3 of a century, during which time virtually none of Newton’s or DIO’s dozens of positive contributions to knowledge (e.g., R.Newton 1977 & R.Newton 1982, www.dioi.org/vin.htm) have been credited there, a blank entirely (J81) in current scientific certitudes&practices all too obvious from fn 5. 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will merely (!) require the opening of discourse® and minds.

B2. Ptolemy regularly fabricated data from which he perversely claimed he derived his models’ elements, even while practicing the very opposite. Loyalist Pedersen 1974 p.23 originally disagreed but summed up the esteemed astronomer J.Delambre’s view of Ptolemy as “a scientific cheat, swindling with the very method of science and betraying the empirical character of astronomy, setting forth results computed from theory disguised as empirical data in support of this same theory.” Ibid p.258 also originally rejected the charge that Ptolemy stole from Hipparchos the thousand star-positions in the Ancient Star Catalog — even while suppressing citation of Delambre’s 1817 discovery of proof (§K1) that Ptolemy avoided, 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 archet was off his latitude by 1°/4 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 shriekanalysed 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 catalogue to a scientific basis — above politics, smearing, stunning, 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 Gingerich had brought P.A.S.P. up to snuff on who and isn’t reliable, Skeptical Inquirer Editor K.Frazier asked him to debate Rawlins on Ptolemy in S&hq; Gingerich (1978/2/2) explained his refusal by calling Rawlins “exceedingly paranoid” for “suggesting that a cabal has been suppressing the consideration of [R.]Newton’s work” on Ptolemy. When finding that Frazier had sent a copy of this helpful character-profile to Rawlins, Gingerich 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 unindictable 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 channel — accuse someone of insubordination (e.g. Isis) to suppress heresy (e.g. Newton, Einstein even Whipple) till P.A.S.P. at the head of this note). At Gingerich’s insistence, U. Notre Dame’s 1999/7/5 debate 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 supPLICATE any chance for personal persuasiveness, e.g. the acceptance of Gingerich in a 2000 referee report to Isis on an article (Thurston 2002S) appreciative of Rawlins’ inductive successes, typically promoted freethought-in-theory while inserting an element aimed at ensuring that heretics’ irresponsible abuse of freedom would be properly recognized as what could only issue from a disordered mind: “To say that the paper should not be published will only fuel the paranoid claims of the small [fn 1] group fighting Ptolemy, who believe that a cabal of [Neugebauer] ‘mutina’ [sic] are pressing forward from being aired in the same manner. On the contrary, I have received another paranoid’s letter regarding said zany’s own recent book-review (emphasis added): “So far the Neugebauer cabbie has not been heard from. Perhaps my merely mentioning [R.]Newton in a review of Neugebauer has placed me beyond speaking terms.” (See www.dio.org/pml1.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-association-sensation fantasy, Gingerich adds one more fantasy, soberly describing himself 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 deitf 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 fisherman.” (Eminent Victorians, 1918, Manning, chapter end of part 9.)

® DIO has long had a standing offer to publish debates, with quite novel rules, www.dio.org/debate.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 Mufirossi.] Such openness to airing the findings of what the referee calls “those with different views” (fn 1) contrasts revealingly with the degree of dissent-toleration exhibited by the referee’s own circle.

® 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.]

in fact had stolen it. Pedersen 1974 p.258 added (emph added): “Our general impression of [Ptolemy’s] moral and intellectual integrity would be damaged beyond repair if we had to believe that he simply derived his catalogue from a previous work by Hipparchus without the slightest acknowledgement of the fact.” Yet when it later became obvious that the theft had indeed occurred, Pedersen flexibly decided that stealing stars didn’t really prove Ptolemy was dishonest, after all (Pedersen 1993 p.559). Agreeably reverting to the 1974 Pedersen, Evans 1998 p.262 says: “At stake is Ptolemy’s reputation as an astronomer; at issue are his honesty and reliability as an observer.” At stake? Well, not-receely — for either 1974 Pedersen or JHA Editor Evans. Each could always be counted upon to evade every negative evidence, trying to make the issue Ptolemy’s integrity uncontestable thus irreparable, so never having to admit their original mistake in defending it. And far, far harder: to face the shame of having for decades (mostly behind-the-back: fn 5) gang-condemned as fools and cranks who those turned out to be more prescient (§M3 and fn 12) than their denigrators. Pioneer skeptic Robert Russell Newton is deceased. Nothing can now compensate him for the hateful, largely- whispered bile heaped upon him to prevent his case from getting a fair hearing while he lived: sampling at fn 35, plus MacArthur Genius and amateur® scientist Nobel Swerdlow’s branding this brilliant physicist a Velikovskian “con-man” (see DIO 1.1.3 §[D2-D3]; and for who’s really Velikovskian, see below at §N19, and Worlds in Collision p.330). Given such a heavy long-term investment in their position, it’s an easy prediction that not even the nine ultra-obvious evidential items set out below will cause Swerdlow or Evans — or anyone else in their shrunken® chauvinist
that explains multiple features of the available evidence. Ptolemy’s fraudulence is that single simple theory which does so.

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 inherently highlight defenders’ abi–incoherence: §§B3 and most especially Rawlins 1992V §§C31-C33) 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-concept 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.

B5 Ptolemy 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 précised by Thurston 1998A) — but also of various lesser-known yet shockingly clear points that leave no reasonable doubt. These follow.

C VENUS Versus Venus

(AND THE VENUS-MADE-ME-DO-IT DEFENSE)

C1 The Greatest Astronomer of Antiquity’s sloppiness with his Venus swindles was so extreme that he inadvertently reports (Alm 10.1-2) having 1st-hand “observed” the same event — the 136 AD greatest evening elongation of Venus — on 2 different dates 37Δ apart (136/11/18&12/25), at 2 different positions 37Δ apart, and at 2 different maximum elongations from the mean Sun, differing by 1°5 [nearly the Sun’s semidiameter!]: 47Δ 1/3 versus 47Δ 16/3. Not only do these data disagree with the outdoor sky, they also disagree with Ptolemy’s own Venus tables. (Similarly for the Sun [§D5] and the Moon [§G3].)

C2 This is by far the funniest 14 & most astonishingly inept fake in the entire history of the oldest science. (But watch Ptolemy again!again try mightily to match it at §§E5&G3.)

D ILLEGALLY BLIND: SKY OR SLYLY MULTICORROBORATION

D1 The Greatest Astronomer of Antiquity’s four solar “observations” (Alm 3.1 and 7) are on average about 50 times nearer15 to his indoor tables than to the real outdoor sky create Reasonable Doubt, Rawlins 1992V §C20 responded: “Besides a range of specific evidence of inaccuracy, 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 ‘Enigmas’ (bid §C22) of the case immediately evaporate.”

[¬Ptolemites are hereby asked to specify any that don’t.]
To appreciate the grossness of the illegality here, consider its sheer enormity (as emphatically linear, and planetary contexts throughout) Newton 1977 for his 3 equinoxes, Ptolemy is alleging 1st-hand visual sightings of the Sun’s center on the celestial Equator at times agreeing to ord mag 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 (§D5 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 ~-23/0/cy. (A rate consistently bi-miscomputed at CalTech’s Swerdlow 2010 p.152, item 3.) So: when is Ptolemy’s Sun correct? Obviously that epoch must be 137.547 (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?

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

E1 At Alm 5.12 and 13, The Greatest Astronomer of Antiquity provides and computes celestial positions using his assumed geographical latitude L for Alexandria: 30°58’ — an erroneous value swiped from Vitruvius 9.7.1 (probably based on observation by asymmetric gnomon, not transit circle). For c.8000 sites, Ptolemy’s Geographical Directory (GD) lists, in Books 2-7, geographical latitudes L and geographical longitudes E east of the Blessed Isles (discovered at Rawlins 2008S §F, to be obviously the Cape Verde Islands), uniformly rounded to the nearest twelfth of a degree. At GD 4.5.9.5 he gives 31°15'0” for his religious home, the Serapic temple at Canopus. The Alexandria and Canopus values are each too low by 14’. No regular celestial observer — Ptolemy’s pretense (at, e.g., Alm 7.4) — can be this far off and not know it.

17 Go to New York Times Science’s 2009/9/8 essay of a century of establishment promotion of another scientifically unverified myth, a study in ceremonial 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 Grabholt’s analyses.

18 From chats with Ptolemistors over many years, the following alibis are recalled: [1] Ptolemy’s —65’ solar error has been speculated as due to his having constructed his system before most or all of his solar “observations” (pretty dumb, since the system depended on the Sun: §D3), so he was stuck with the error and decided [see Ragep at §3 fn 9] not to re-do his whole scheme. (How is this a defense against a charge of knowing pretense? And plagiarism, since §D3 the system’s error is just that of Hipparchos’ solar tables, 2/3 centuries later.) [2] The adherence of “observations” to theory (Sun, Venus, etc) is explained as due not to fraud but to Mere fudging or “adjusting” of real presumed data, though whatever positions the “observations” were being fudged to agree with were SECRET indoor calculations, so either way it’s fraud. [3] The embarrassment that Ptolemy’s tabular mean Sun was correct for only Hipparchos’ time has been explained by presuming that Ptolemy thought Hipparchos’ observations were inferior to his own but still gave them efforts at explaining them away. However, as in §B3 [b], we find no substantial connexion between chauvinists’ several desperate and disparate alibis (other than the common aim of rescuing Ptolemy), while by contrast all three oddities are mutually-corroborative of each other through the simple theory that simultaneously, coherently, and fruitfully explains them: Ptolemy faked.
E2 Moreover, such an error (see math of Alm 5.12 and 13, or Rawlins 1994L eq.1) would carry exactly into the “Clean Dozen” unfudged and unused Ptolemy-contemporary stellar declinations δ reported at Alm 7.3 (the only honest Almajest star data from Ptolemy’s era). These, however, show (Rawlins 1994L §F9) that the observer’s error20 in assumed geographical latitude L was ±4°2’, about 9 standard deviations distant from Ptolemy’s −14’. (Similar disconnect for the 1025 celestial latitudes β of the Alm 7.5-8.1 star catalog.) So the δ were plagiarized from a contemporary anonymous observer who knew his L.

E3 Confirmation is achieved via statistical induction (Rawlins 1994L §F8) of the exact latitude L = 31°1/4 by assumed by the observer of Alm 7.3’s Clean Dozen stars — that is, 17’ higher than Ptolemy’s stated latitude of 30°58’ (§E1): a hard conflict which alone shows that he had plagiarized the Clean Dozen from Hipparchos-Strabo (Strabo 2.5.39; Neugebauer 1975 p.1313; Diller 1984 fn 23; Rawlins 2009S fn 16). Question: Since his Alm had already (§E1) put Alexandria instead at 30°58’ (consistent with GD 4.5.9’s S’-rounded 31° value) why didn’t an Alexandrian22 & allegedly-outdoor observer notice he’d thus inadvertently stretched by ordn MAG 7.1 the 7-stade-long embankment connecting Alexandria to its Pharos, an embankment explicitly named Hepstadiion (επισταδείον: Strabo 17.1.6)?

E5 So by carelessly23 copying disparate data L from Vitruvius (§E1) and Hipparchos (§E4), “astronomical observer” Ptolemy adopted — simultaneously — two L’s over decades, have never noticed either? — which could have saved themselves a half-century of diﬃcult and trenchant-embarrassment, by recognizing the obvious right away and promptly moving on to careers of open-ended inquiry instead of sterile (fn 12; Rawlins 2009E fn 7), quasi-theological apologetics, with sacred-cow-conclusion-set-in-advance. In cement. The answer is revealed by another question: which route has been exclusively rewarded (fn 37; Rawlins 2018Ex 1: Table 3) — consistent with Alexandria’s L = 31°1/2’. Same process produces the Clean Dozen’s epoch, 159±8’, in fine accord (ibid fn 45) with the 10th century Suda’s dating of Ptolemy to epoch Marcus Aurelius 1 (160/7/14). Previously unnoted vastness of Alm 7.3’s leapfrog anachronism: the Clean Dozen’s L’s were off by more than the c.160 AD (as just shown) and then merged with the S’-rounded which was precessed-faked so inadequately for stated epoch 137 AD that their coherent 5 stars’ mean is (fn 37) instead correct for over 100 before 160 AD! There are two unknowns when analysing ancient star-deciliation lists: the observer’s epoch E and the error x in his assumed latitude. For the four observers whose star-deciliations are discussed in Alm 7.3, the curious paper, Brandt et al 2014B (discussed also in fn 37), gets mostly non-outlier results for epochs E yet for all 4 cases messes up the other unknown, x, the error in the observer’s assumed latitude. For the 4 observers, the figures given (op cit p.331) for the latitudes’ “accuracy” (which the authors compute instead of x) are: Timocharis 0°7, Aristyllos 0°18, Hipparchos 0°24, Ptolemy (?) 0°3 — values which are oversmall by an ordn. Likely-wasted-at-present wake-up to the history-of-science community: outside of DIO (3 Table 2, or Rawlins 1994L §F9 & Table 3) no paper on the Alm 7.3 declinations has ever correctly understood how to find both E and x & associated standard deviations. The 2014 paper also errs in dropping near-solstitial stars for being weak indicators of epoch — forgetting that they are superior indicators of δ, and thus in gauging the accuracy of the 4 men’s adopted latitudes for their observatories.

21 Along with §§C, D, and F, the star-deciliation analysis proves that Ptolemy’s observerness (or his authorship of the models he reports) is not established by the mere fact that some of his purported observations are datable to his time.

22 Yes, Ptolemy clumsily double-lists L values for sites other than Alexandria (e.g., Heliopolis-vS-On and Syene-vS-Elephantine: details at Rawlins 1985G p.260 and n.6). But none were the world’s cultural center he purported (by his defendants) to have been familiar with and from where he reports 1st-hand astronomical observations (Alm 5.12 and 13), an occupation which if real would have quickly and accurately provided Alexandria’s L — and had already done so (Rawlins 1994L §F9 & Table 3) for genuine outdoor astronomers Timocharis, Aristyllos, and (above, §§E2-E3) Anonymous.

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

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

E6 Also revealing of Ptolemy’s degree of empiricism is his astonishing listing of the Pharos (§E4) at exactly the same L (§E1) — 31°05’ — as for his home Seraph temple at Canopus from where he had only to look down the Mediterranean coast after dark to see it. Besides the 31° distance (min) 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 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 mean synodic motion have been empirically and mathematically based for both works, if the respective derivations involved discrepant 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 speed is anyway precisely computed not from the alleged observational base (angular-arc-traversed/time-interval) cited at Alm 9.10 but from the period relation 52200/1680224 found at Alm 9.3. [Full calculation of 5° discrepancy: Rawlins 1987 p.236-237.]

[It was specifically this fraud that most convinced van der Waerden Ptolemy was “a liar.”]

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

24 Check Rawlins 2008Q (§C1 & eq.23) for the Pharos’ sea-level visibility-distance, about 20 nmi, or 1½ of great-circle measure on the Earth’s surface. (Ibid eqs.23 and 24 reveal [using www.dioi.org/cot.htm#kchg] that Sostratos measured the distance as 20.2 nmi.) The remark on Ptolemy’s-eyesballs of course assumes that our Greatest Astronomer even knew (or cared) which way was north. (Canopus today is Alm Qir, site of 1798’s Chapter 1 in Horatio Nelson’s serial destruction of Napoleon’s fleet; also near the 1894 birthplace of mystic [and #3 Nazi] R.W.Hess.)
H  STAR CATALOG TESTS AND ANOTHER DOUBLE: PTOLEMY AS LOSER-MAGNET

H1  Had Ptolemy observed the Ancient Star Catalog via armillary astrolobe (described at Alm 5.1) with its ecliptic ring off by his notorious \(-1^\circ.1\) mean longitude error, the real and instrumental eclipses would be tilted by \(1^\circ.2\) vis-à-vis each other (since the instrument

\[E = \frac{\Delta E}{\sqrt{\sigma^2 + \sigma_E^2}}\]

foreign eclipse times couldn't have supported the longitudinally-stretched geography (§G4) he borrowed (with credit)\(^25\) from Marinos of Tyre. Instead, Ptolemy's vast opus provides (GD 1.4.2) but one\(^26\) example: two longitudinally much-separated reports — 500\(^{\circ}\) old! — of the famous Arbela — 330/9/20 lunar eclipse's start, saying it was seen there at 23\(^{h}\) and in Carthage at 20\(^{h}\), thus proving that the 2 places are 3\(^{h}\) or 45\(^{\circ}\) apart in longitude.

G3  However, Pliny earlier reported the same data very differently: 20\(^{h}\) (8 PM) for Arbela (modern embattled oil-city Irbil) and 18\(^{h}\) (6 PM) for Sicily, whose west end — major city Lilybaeum — was part of the Carthaginian empire, and of longitude similar to Carthage. Modern calculations\(^27\) show that non-astronomer Pliny was quite accurate, while The Greatest Astronomer of Antiquity was amazingly wrong, over 2\(^{h}\) off for Carthage, 3\(^{h}\) off for Arbela. The former error nearly equals the entire actual 2\(^{h.1/4}\) longitude gap between the sites, and the latter error far exceeds said query. 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\(^{h}\) Carthage and 3\(^{h}\) Arbela. How explain such an entertainingly disastrous fabrication? Start by consulting Pliny 2.72.180 on the —330/9/20 lunar eclipse. Reading the passage carefully, one sees that no numerical hour is given explicitly for the western apparition in Sicily, merely: moonrise (“exoriens”). By contrast, the Arbela time is given as the “23\(^{h}\)” hour “after sunset, or about 20\(^{h}\), 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, “seconda hora” appears nearer in the sentence to “Sicilia” than to “Arbellam”. This obviously suggests that Ptolemy used Pliny or his source but (evidently unable to read Latin well) took Pliny’s 20\(^{h}\) time to be Carthage.

G4  But how did Ptolemy arrive at 23\(^{h}\) for Arbela? Since Gossellin 1790, it has been obvious that multiplication by an expansion factor (Diller 1984 §C5) had been applied by Marinos or Ptolemy or their source to a prior map’s accurate longitudes, creating the oversize longitude intervals of the GD. Rawlins 1985G eq.15 showed that the expansion factors were either 7/5 or 4/3 in the region under consideration. Assuming that the earlier accurate map correctly put Arbela 2\(^{h.1/4}\) east of Carthage, then expansion by 4/3 would produce 3\(^{h}\), 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\(^{h}\) to 20\(^{h}\), thus arriving at his fantastic 23\(^{h}\) time for Arbela.

NB: This solution adds powerful new evidence favoring the theory (still-foolishly-doubted: §G4; Rawlins 2008Q §J & Rawlins 2008S fnn 13&45) that the GD falsely corrupted an accurate prior map by expanding its longitudes by a factor of 30%-40%. Collecting §§C&E with the present case, we now have 3 separate Ptolemy double-false fakes on display here.

\[^25\] Was Marinos cited partly because (unlike Ancient Star Cataloger Hipparchos) he was still alive to complain if incorrect! 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.

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

\[^27\] The Battle of Arbela was fought at nearby Gaugamela and 11\(^{d}\) after the eclipse. We find actual Local Apparent Times of the —330/9/20 eclipse’s umbraal start: Carthage 17:43, Lilybaeum 17:52, Gaugamela 19:56, Arbela 19:58. So the Gaugamela-Lilybaeum difference in geographical longitude \(E = \Delta E = 2^{h.04}°\); Gaugamela-Carthage, \(2^{h.13}°\).

\[^28\] Rewards handed out to those who attacked the R.Newton satan include JHA boardship (R.Newton 1991 fn 2) and a MacArthur for miss-man Swerdlow. (It’s hard to find good help anymore.) Among other examples: maid-men Evans and Schaefer were elevated at JHA not long after their massive bubbling 1998 and 2001-2002 attacks on Rawlins’ work on Rawlinson as the World’s First Philosopher. (The truth here may actually be deliberate.) Selecting boardmembers by such criteria will damage mean-IQ atop JHA for decades to come.

\[^29\] D.Duke’s statistical studies indicate that very nearly all stars were appropriated. If Evans and Schaefer were right that Ptolemy 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 apheres) 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.)

R rotates about the equatorial not ecliptic pole: as we can see from, e.g., the educational paper model Evans has helpfully disseminated, so (Rawlins 1982C p.361 & Fig.2) we’d find error waves of amplitude 1°22 in the Catalog’s latitudes \(\beta\) (cosine waves: \(\beta = \sin B\)) and northern longitudes \(\lambda\) (sine waves: \(\beta = \cos B\)). We don’t. (Amusing details at Rawlins 1992V §§C13-C15 & fn 31). Also see 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’ stars are statistically quite dispersive in the \(\alpha m\)’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 it thus became obvious c.1990, even to the most religious, that many Ptolemy stars were Hipparchos’ stars, however, no archon was ever going to admit in print the plain truth: the establishment had been blindsided by proof that its challengers had been right all along — that Graßhoff’s test had now unexpectedly surprise-vindicated the long-loathed Tycho-Newtonan position that the Catalog was stolen. (The post-disaster spin of some was that the three pioneer skeptics had just regrettably not proven their case clearly enough to be understood by the judicious archons atop the American Astronomical Society’s Historical Astronomy Division [H.A.D.]) — surely a truth-determination criterion to live by — while Graßhoff had. Which translates as: [a] ashamed refusal to acknowledge that — until the truth hit them in all their faces — believers had been too limited and predisposed to see anything significant in the same evidence from which skeptics had drawn the right conclusion years ahead of final proof; [b] denial of credit to unapproved first perceivers, according to a principle handed down to us from on-high, the JHA itself [quoted at Rawlins 1991W fn 127, 128] authored, “the first speculative occurrence of an idea is generally far less significant than its later emergence, possibly in other hands, supported by persuasive arguments.” The post-Graßhoff era has been especially fertile for indiscriminate fallback apologues by Ptolemy’s (selectively) malleable modern choir, as the politically ambitious realized that the JHA would ever so gratefully\(^28\) publish anything that muddied the clear evidential situation, in order to save archons from facing apt appreciation for decades of falsely denigrating now-vindicated scholars: simply pretend vindication either never happened or isn’t 100.00000000%. Question: is anyone empathizing with the cornered defenders’ needless pain here, caused by artificial extension of the Controversy? (Puts one in mind of equally needless ongoing misery from other kinds of poverty than intellectual — mass-agony likewise of insuficient concern to those who subsidize its perpetuity, to ensure their own perpetuity in ofce.) This is so literally pathetic — Chauvin’s shade shutters his orbs in shame at what his legacy has come to. [1] Memory-hole-unapologetic for his original 1987 and 1998 arguments that Ptolemy probably outdoor-observed the whole catalog, Evans now just hopes that Graßhoff 1990 hasn’t proven that all\(^29\) stars were copied from Hipparchos. [2] Schaefer (2002) says the Yale Bright Star Catalog also grabs previous catalogs’ stars, so what’s the concern? However, both these defenses of Ptolemy’s integrity plainly founder upon his claim of 1st-hand observation of all 1025 stars. And argument [2] is on the logical and ethical level of defending a bank-robber by pleading
that, well: doesn’t everybody withdraw money from banks? The BSC does not claim 1st hand observation, while The Greatest Astronomer of Antiquity explicitly 30 does 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” JHA-H.A.D. (JHAD) solipsistically hallucinates — like Dr.Frederick Cook or Alger His — that if we just never confess, then no one will ever know the truth: that we Experts lost what has correctly been advertised nationally by Schaefer 2002 as the hottest controversy in the field. [iv] But neutral observers increasingly and snickeringly do know — which is marking certain JHADists as losers to scholars they themselves have long been assuring the world are crazy dishonest paranoid incompetent cranks. And we’re not supposed to giggle? (You begin to see why the seething losers can never admit it?)

H3 Pickering 2002A [B1 & Fig.1 points out a history-of-science-ignored ultra-simple disproof of the Catalog’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 $L = 55^\circ$-9. impeded these difficult Sgr stars’ availability. (By recording Fomalhaut, Tycho actually went down to within $26.6$ of the horizon: Rawlins 1993D Table 17.)

I Catalog Fractions: Jekyll’s Slyde&Hyde Coverup — Knowing Destruction of Data

I1 The Ancient Star Catalog (Alm 7.5-8.1) has an obvious excess of 00° endings and 30° endings in the latitudes $\beta$, 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 $\lambda$ is 40°. I2 R.Newton 1977 (pp.245-254) showed statistically that this odd circumstance was simple to explain, once he discovered the key and unlocked the longstanding mystery: when Ptolemy stole Hipparchos’ stars, he naturally left the Catalog latitudes $\beta$ unchanged, while updating all Catalog longitudes $\lambda$: 1°/century-pressuring 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 sliding each longitude $\lambda$ by 2°40’, 00° endings became 40°; 10° became 50°; 15° became 55° and were rounded to 00°; 20° became 00°; 30° became 10°; 40° became 20°; 45° became 25° and were rounded to 20°; 50° became 30°. (Note how the odd endings 25° and 55° got eliminated.) The frequencies of endings in $\lambda$ and $\beta$ are displayed by Rawlins 194L Tables 1 and 2, and the whole slyde&hyde process is verified via $\chi^2$ test (ibid §§B-C).

30 Before the Catalog’s theft became plain, no historian-of-science was insisting that Ptolemy wasn’t claiming observorship (Rawlins 1982C n.3). Schaefer’s dodge (§H2 item [2]) was just the latest in the simple tradition of evading facing skeptics’ vindication. One even accent Ptolemy’s use of the word “we” when describing purported 1st-hand observations — a tack which wishes to refuse 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 $L$.

13 Most critiques of Ptolemy’s chicanery point primarily to the excess of 40° endings (vs 00° endings) in the Catalog longitudes $\lambda$, but (thanks to the 00° ballot-box being deliberately?) stuffed with the entire sample of rounded 55°’s) the most shocking frequency-contrast is elsewhere (Rawlins 1992V §C22 item [2]: Rawlins 1994L fn 5): the spectacularly greater number of 10° endings than 30° endings. (Before Ptolemy added 2°40’ to Hipparchos’ $\lambda$, these were 30° and 50° endings, respectively.) Looking 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 $\beta$, 30° are roughly twice as common as 10°, but this is reversed for the longitudes $\lambda$, by far the strongest confirmation of R.Newton’s hypothesis for explaining the longitudes’ odd fractional-endings distribution.

14 Maintaining the Jekyllian pretense to being a genuine, respectable, outdoor astronomer, required the sneakiness of hyding the otherwise-glaringly-odd 55° and 25° endings (which Ptolemy’s addition-thievery had produced from formerly 15° and 45° endings), by secretly rounding them to 00° 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.

15 So R.Newton (§J2) explained why longitudes $\lambda$ are near-bet of 15’s and 45’s (only 32 in all: five 15°, no 45’s), though appearing with roughly expected frequency for latitudes $\beta$: twenty-eight times more often than for $\lambda$.

16 Two other little-known extras regarding the Catalog: [A] Rawlins 1994L §§E4-E7 found statistically (at high odds) that the compiler of the Catalog’s southern stars observed from a place where he had estimated his geographical latitude $L$ at a value ending in 5/6 of a degree, consistent with the southern tip of 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’ $\lambda$ 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’ $\beta$ 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.

17 The root of the persistence of the embarrassingly-long (given the evidence’s imbalance) “debate” over the Ancient Star Catalog, is that sneaky (§J4) stealing ordmag 1000 stars is unabiguously, undeniable a scientific crime, verifying the justice of the Newton book’s Neugebauer-klan-hated, JHAD-ensurig title, The Crime of Claudius Ptolemy.

18 Some Ptolemists have improbably dodged Ptolemy’s other (Sun, Moon, planets) fudges by claiming they’re just innocent pedagogical illustrations of his theories (ignoring the inconvenience that he repeatedly calls them real 1°-hand outdoor data — Rawlins 2002V fn 12); but over 90% of the Catalog’s stars are never used in Ptolemy’s “illustrations,” so the threadworn PedaDodgical Ploy cannot excuse his explicit claim (§K1) of 1°-hand observation of all 1025 stars, a theft statistically lock-proven by Graßhoff (above, §H1).

19 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 “merc” Fudger from the naked thief.

32 All 5 stars with 1°7/4 longitude endings are ecliptical and are that rare (like Tau informatae): not copied from Hipparchos. DIO found that these 5 oddballs’ conjunctive sources (Rawlins 1992V fn 20 and Rawlins 1996C fn 108) were three lunar eclipses (Babylon, Hipparchos, and perhaps Menelaos) and twice Venus.
**K APPENDIX 1: GRUSOME TESTABILITY WARS**

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

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

37 Similarly, a recent paper (Brandt et al 2014B; see also fn 20) tries to deny the certainty of Ptolemy’s fabrication (from Hipparchan data) of any star declinations δ found in Alm 7.3. Various modern scientists have noticed that Ptolemy “proves” his false 1°/cy precession from his 18-star sample using, “up a pole of the Sick Six declinations (the ‘Ephemeris De revolutionibus’ — an epoch that’s nearly the same for Ptolemy’s declinations—list AND his Catalog. H.A.D. would insist upon its ofcers engaging in rational academic discourse. Instead, we’ve had 4 decades of their dereliction and/or equally inexcusable non-comprehension (despite the author’s vain 2002/10/2 request of stonewalling American Astronomical Society chief Milkey that he look into the conduct that has characterized the controversy), while since 1968, JHAD missionary Gingerich — soon after joined by Swerdlov — has (Rawlins 1994S §§B5-B8) lauded jehad against infidels and tried hermetically whackamoling all public debate on Ptolemy (reincarnating those who burned books and sorcerers in the Dark Ages, to contain another heretical disease), privately contacting any forum or party or person who dissented or was about to: Nature, Science, Scientific American, Rawlins (1974), CISCOP. Martin Gardner (1978), Horace Judson (2004), etc — to assure them that the skeptics are dishonest crank incompetents, correctly confident that the non-specialist gullie would believe he need not check alleged supporting evidence of such eminent personages [§p.87]. Apparently, the notion that a MacArthur Genius and a Harvard professor could themselves be either shall-we-say Limited (fn 8) or shall-we-say not-overly-truthful? (fn 5) or both, has heretofore seemed just too outre for non-scientists and writers (unfamiliar with the JHAD) to believe, thus popular media (if we naively assume power-secretaries’ naïve) continue to be near-100% buffaloo by our dedicated geniuses. (Rawlins [like even Gardner&Judson] was similarly fooled [1974/11/15, back when he wasn’t yet into Greek astronomy], for over a year, by Gingerich saying Newton was just a crank; see Rawlins 1994S §C3]. Question-in-passing: how do scholars of the refined character and competence of Swerdlov and Gingerich keep getting to be eminent in the 1st place? Their JHAD is even now hoping to assert further Dean-Wormersesque Double-Secrecy [§N17] by acquiring Double-Peek fail-safe control over
various of the non-cataloged Alexandria-visible stars were conveniently attachable to nearby constellations. And we know that Ptolemy was (or copied) a star-attacher: Alm 7.5-8.1 lists dozens of “informate” stars which are in the vicinity of traditional constellations though still outside them, but which he nonetheless appends to them. This 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 (bright but uncataloged e Car [m = 1.9] is less than 6° from Cataloged δ Vel: PK886), despite being easily visible from Ptolemy-era Alexandria (pulling away through declination magnitude—μ ranging between 2° 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 limit18 μ0 for capture. Even more peculiarly absent from the Alm catalog are α and β Gru. Both of pre-extinction magnitude μn ≈ 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 Hipparchos’ horizon), they could have just been set aside as a new19 constellation. After all, [i] There already was a two-star constellation, CMi (PK847-848); [ii] Ptolemy was inventor of the new20 asterism Antinous, which he formed c.130 AD from six21 stars “around” Aquil (Toomer 1984 p.357).

K2 Stars α, β, and δ1 Gru are missing from the Alm star catalog, though all were visible from Ptolemy’s Alexandria (μ = 3.3, 3.9, and 5.2, resp) and were attachable to nextdoor PsA, as suggested by Ptolemy’s including (into PsA) γ Gru (PK1022), a star only 5° from 2 PsA (PK1021) versus 10° from α and δ Gru, and 7° higher than δ in declination, which connects to why γ Gru was visible to Hipparchos while α and β were below his horizon, and δ’s μ of 7.1. So, why was γ Gru Cataloged while the other 3 Gru stars weren’t: [1] the three’s distance from Ptolemy’s PsA? or [2] Hipparchan invisibility? Answers: [1] Gaps exceeding 10° between constellations’ prime stars aren’t rare. Cep: γδβ (PK76&77) 11°. Aqr: βκε (PK632&636) 12°. Peg: γκδε (PK316&318) 17°. Hya: βκδγ (PK196&917) 22°. So the no-availability-of-constellations argument is slaughtered by [2] the obvious explanation: for being the Catalog’s sole Gru star, namely: of α, β, γ, and δ, only γ was visible to Ptolemy.

The sickFive, whose weighted mean error vanishes c.59 AD — since he faked it to co-prove false 1/2 precession from Hipparchos’ atypically very-inaccurate Arcturus δ which was accidentally correct for a time later than his actual epoch by (you guessed?): c.50°. To emphasize this glaring giveaway by summation: if the sickSix sample were actually observed in 59 AD (& its SixFive do indeed have near-null declination error for that date), it is remarkable that the lone Ptolemy star (Arcturus), whose δ-error vanishes c.50° after the LMO, should just-happen-to-also-be the lone Hipparchos star whose δ-error also goes null c.50° later than Hipparchos. (To be exact, 52° after Hipparchos; 56° after LMO.)22 Hipparchos’ μ0 was a bit dimmer than 5: §L3. For his deep south sky, virtually every identifiable star brighter than μ = 5 appears in the Catalog. Note: this is about the μ (Rawlins 1993D §4) at which Tycho’s normally high star-cataloging accuracy begins to fade. (Tycho had 14% less sky visible to him than Hipparchos, so he had to go a bit deeper in μ to approach his goal of netting 1000 stars.)

Evans 1984 had argued for Ptolemy’s originality with the equant, but only 31° later is adding his UNoriginality to squirm out of a religious paradox. See Sweboldow similarly at Rawlins 1992V in 43.

Not just the date argues for Ptolemy’s Serapic temple being the source of Antinous’ intermittent celestial immortality: additionally, there is a copy of that very temple in the Canopic Vault of Hadrian’s Villa, outside Rome. Go to www.dioi.org/cot.htm#hvr for further analysis, plus Rawlins photos of Hadrian’s Canopic temple replica (with poolside stone crocodile), as well as of a classical-era Antinous statue (Glyptotek, København).

41 Has it previously been noticed that not one of Antinous’ six stars appears in the Hipparchos Commentary’s Aql, or indeed, anywhere else in that work? The stars were PK295-300; or from Bayer: η, θ, δ, ε, κ, λ Aql. (Of the three λ endings are 40° or 10°, vs two randomly expected, a slight hint that Hipparchos observed them after writing the Commentary.) The group was named for beheaded Emperor Hadrian’s recently Nile-drowned teen boytoy (Rawlins 1992V in 44) and presumably to thank the emperor for his visit and for favoring (perhaps initiating) imperial sponsorship of the Serapic astrological-medical superstition the Canopic temple specialized in.

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

L2 A passing allusion 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 as to make his putative observations’ declination-range deceptively look22 as if the observed were at Hipparchos’ latitude L instead 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 apostolos’ 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 Rhodos’ southern tip, Cape Prassonesi (see fn 42 for geographical latitude \( L \) and height \( z \) above sea-level), Hipparchos’ list [a] exhibits a startlingly clear\(^{44}\) split at a post-extinction magnitude \( m_0 \) slightly less bright than 5 (obviously his effective limit for capture): the stars dimmer than \( m_0 \) are not in the Catalog, while those brighter than \( m_0 \) 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 upchuckle.

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 next pretender as the “Greatest Astronomer of Antiquity”? [a] Were public attacks on a famous scientist resented by science’s politicians as endearing science funding?\(^{45}\) — but astrologer-mathematician Ptolemy was not a scientist. Not empirical.

\[\text{any other altitude than } h = 0^\circ;\] see the lucid and irrebuttable discussion at ibid [F11]. Further, thanks to an astrophysics recovery by B. Goldstein, we now have the fact (Rawlins 1993D §8.8) that Ptolemy said in so many words that 15th 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 15th magnitude stars’ visibility on-horizon entails (ibid §8.8) ancients’ eyesight perceiving 12th magnitude stars (\( \mu = 12; \) and see §4 for comparison’s sake).\(^{45}\) \( \mu \) and \( L \) are traditionally and rigidly linked as of antiquity’s “greatest and brightest” magnitude (\( \mu \leq 14 \)). So: why didn’t Archimedes beat Clyde Tombaugh to the discovery of Pluto?

\[\text{Define split-Vagueness V in §3.3’s list [a]: minimmest Cataloged star’s } \mu \text{ minus brightest non-Cataloged star’s } \mu.\] Testing Hipparchos’ \( \mu \) (\( \gamma \) Ara vs \( \epsilon \) Cru) at Cape Prassonesi (height \( z = c.200^\circ \text{m above sea-level} \) for 5 assessed magnitude opacities: 0.14 mags/atm (negligible aerosols), 0.15 (Rawlins 1982C), 0.17 (Evans 1987, #2), 0.20 (Evans 1987, #1), 0.23 (Schafer 1991): V = 1/5, 1/4, 1/3, 1/2, 3/5, respectively.

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

\[\text{Among those tested by Rawlins 1982C Table II, the only major star (\( \mu \text{ < 3} \) that seems unambiguously to be missing from the Catalog is } \mu \text{ Vel (} \mu \text{ = 2.7; } \mu = 3/4 \text{ for Hipparchos, 3/5 for Ptolemy). A speculation at DIO 4.3 114 showed how star PK964 could be a mangled version of a position originally based upon an hypothetical observation of } \mu \text{ Vel, high by } 1/3 \text{ in R.A. (3° great-circle), but in declination fully accurate to ancient precision.}\]

\[\text{The half-century Ptolemy Controversy should have been over in half an hour — had all participants amicably cooperated at the outset and sat down together to compare data and enlighten each other. Had defenders realized early on, before positions hardened in ignorance of, e.g., the significance of Ptolemy’s Alexandria’s geographical latitude \( L \) (§1.1) and real ancient scientists’ high-accuracy achievements (§M3), participants would (Panglossianly assuming open minds all around) have soon shaken hands, parted in peace — and moved on to more challenging historical mysteries.} (\text{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 gooreos). 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 \( Alm \)’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.\]

\[\text{[c] Inevitably-feebly attempts to logically back up prominently published one-sided (fn 11) salesmanship, pushing Ptolemy as The-Greatest, put one in mind of Aquinas’ voluminous Reformation-germinating mistake of trying to defend by reason that which cannot be defended by reason. [d] Did damage to Ptolemy’s sacred-grant-cow value trigger the stunning (§B of R.Newton’s valuable insights? With the Almajest as [i] the central surviving ancient work on mathematical astronomy and [ii] suffused with fraud, a grant-raising problem was presumably feared (perhaps needlessly: [j]), leading to attacks on Newton, but far more tragic and longstanding promotion of a now-widely-accepted misperception of all of ancient astronomy} — just to cover for Ptolemy’s fudges, by deliberately (fn 46), falsely claiming that everybody-did-it (§M2) in antiquity — a distortion that’s gatewayed a 180° inversion of truth for a range of ancient-science issues, as detailed at [N, below. Above option [d] was the most likely place for the original flame of rage at Ptolemy-skeptics to have started. Newton used to note that the last century has seen numerous charges of historical fraud in the physical sciences, but none produced a fraction of the ferocity of Ptolemists. [Wherever there’s a weak, rationally-indefensible tenet, advocates are left with no other way to protect it than (e.g., Rawlins 2017C fn 1] by suppression, banishment, indiscriminate argumentation, and circulation of way-overdone baseless or irrelevant personal denigrations against opponents. So when we see such phenomena we should sense said weakness. As a general rule that can save plenty of time and bother: [Since almost all protected ideas are false, protection is itself evidence of falsity.]\]
Ptolemy as the quintessential or ultimate ancient scientist — knowingly rejecting the inconvenient fact that his genuinely empirical predecessor Hipparchus, 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 alicant Ptolemy by wrenching academe’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 lack of accuracy (or both) 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 §E1) 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 [f4 Table 1] or Rawlins 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 not have been possible for these measures to progress to such admirable accuracy, if researchers had merely tailored data to previous values. Back in 1979, did Ptolemist ever take 0.5 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 (fn 34 & fn 2) prominent Johns Hopkins physicist R.Newton, whose analyses of Ptolemy employed math which historians-of-scientific couldn’t even understand much less perform. Before such fateful investment, did they ever know (fn 12) of the inescapable (§F2) Mercury inconsistency, or (§H1) the absent-error-waves test?

Can Mufosis show they even ever looked for an explanation of those remarkable millionth-precision lunar-period accuracies (§J3)? 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 deaf-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 warns today’s budding archons how a missetatement — initially from preconception and careless science, then in heedlessly hot outrage at heretical challenge (Swerdlow 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, whose has not yet still ever actually 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, uncited. However, our question to deities isn’t: have they ever had the faintest 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 mogul 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 retreats and fanatics of the history-of-science discipline, their plans could hardly be improved upon. Which actually would be quite unwise... in that such men as Neugebauer, Aaboe, Britton, and others are brilliant despite their blanks/spot.) 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 unruly 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 §N15 item [F]. [3] Mars’ $\Delta$m mean synodic motion is accurate to ordmag 1/10000 century, yet the “observations” on which Ptolemy’s proof of it is purportedly based are off by ordmag 1" (Rawlins 1987 p.237). [4] The $\Delta$m proof of Mercury’s mean synodic motion was already shown above (§F2) to be precise, by one who started with the answer and (if the Canopic 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 $\Delta$m is seldom understood (P.Huber the happy exception — Rawlins 1991W fn 224), namely, its near-exclusive use of ordmag 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 over-determination — where the number of equations of condition exceed the number of unknowns sought.

N2 Deeming (Rawlins 2008R $\S$A) the infamous faker Ptolemy an observing astronomer, while antiquity’s ultimate fabricator was the immortal empiricist Aristarchos whose universe was rightly at least billions of times larger than Ptolemy’s geocentric misconception.

N3 Even claiming (Rawlins 2008R fn 10) that indoor Ptolemy (errors ordmag 1") was a better outdoor observer than Hipparchos, whose errors were actually an ordmag smaller.

N4 Accepting that Greek astronomers were not primarily empirical ($\S$M3; DIO 1.1 p.624; 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., [M13]: also: memorable Aaboe conversation, 1976/3/9). (Note: Oxford U. Press was warned by 1997/7/17 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., [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 junior-high maximum-height problem, MacArthur-Genius Swerdlow keeps asserting (with Evans’ and Jones’ evident assent: fn 11) that solstices could not be determined accurately, and likewise (fn 8) that outdoor maximum Venus elongations must’ve been so crude that Ptolemy was forced to compute them 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 subject to recognizing the prank.) Due to just such an HJA-published every-science, Ptolemy’s even perversely teach — complete with now-ironically Pompeus sneers (quoted, R.Newton 1977 loc cit) at sub-JHA untouchedables — that ancient equinoxes were more accurate (fn 11) than solstices, from their own unfamiliarity (e.g., 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 10th effect of deviation from quadraticity: [N19 below], not to mention [b] ancients’ historically uniform choice of solstices not equinoxes for year-length-determination. Have those who’ve been disbelieving Greeks’ ability to measure solstices accurately (Swerdlow, Evans, Duke) noticed that the newly available papyrus P.Fouad 267A ($\S$N19) has tried to enlighten them by directly surprise-testifying to an ancient solstice which was accurate to ordmag 1°? Just-luck? (Like another trio of just-lucks at idem? Meanwhile, note that Duke not only wrongly doubts that Greek observations were sufficiently accurate for trustworthy solstices [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 $+3°, +0°, -2°$, and $-1°$, resp, indicating that Hipparchos’ rms solstitial systematic error (1°.6) 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: $\S$N 97] 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. 38


N9 We recall how “Mr. History of Astronomy” (AAS-HAD Newsletter #51 Page One) dream-creates his idea of ancient realities. Gingerich 1976 p.477 on a temple-bound faker—mathematician geocentrist-astrologer out in kookburg Canopus (1°U), who was just as skilled-ineducable by his real-science world (1°10-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 JHA criteria (§H2 [b]), just a passing “splendid speculation tossed out during a vigorous discussion between the Alexandrian mathematicians” (www.dioxy.org/sti56.htm).

48 Irony: despite four reliable solstices by Kallippos, Aristarchos, & Hipparchos (Rawlins 2018U Table 3), ancients never got close to an accurate yearlength, as far as we know. In 1777, Brigham Young Univ astronomer H.Kimball Hansen conceived a simple method ancients could have used for accurately finding the year’s length. [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 securely immobile than human equipment. [2] On some date around an equinox (no need to be at just 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-DQ) which do not apply to the remote Babylonian data which ultimately & fortunately made possible the hyper-accurate Greek lunar periods of Rawlins 2017E §B2-B4.
Historians of science exceptionally ignore the perfectly Occamite 3-for-3 hit-success of the spare atmospheric-refraction theory that explains and fits both ancient Earth-

Only 2 ancient Earth-circumference values were widely adopted: Eratosthenes’ (really Sosratos-Eratosthenes’) 500,000 stades, and Poseidonios’ 180,000 stades. A 216,000 stades was later adopted each in succession, though they exhibit a previously unexplained gross disparity, the former being over 40% larger than the latter. DIO’s revolutionary simultaneous solution of BOTH these C values from the same simple theory (math & sources at ibid eq.28) realizes that each Greek C differs from actual C (216,000 stades) by almost exactly a factor of 6/5 (within c.1% in each case), Eratosthenes’ high by 6/5, Poseidonios’ low by 5/6. Hmmm. It happens that there are 2 very obvious stay-at-home Earth-measure methods (one even semi-attested at Pliny 2.65 ignored by even an enterprising Pliny!) — (a) Sun-rise & sunset (Rawlins 2008Q [A4]). Resolution arises since atmospheric refraction causes horizontal light rays’ curvature to be 1/6 Earth’s, thus the light-house-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, despite evidence 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 judging the stade at all, but just by adopting the standard 185 meter value. [Thus serving as the final, controversy-ending proof that 185m was indeed the true length of high antiquity’s stade.] Our achievement here may be compared to the controversy’s endlessly wheel-spinning metrological-solution literature: sniffing & searching through ancient lore in search of hints of oddball stades, which of course existed all over the place before the Ptolemies presumably regularized the measure by defining their empire’s royal stade at 185 meters, commonly miscalled “Attic”, which we may instead (below) dub the “sexagesimal” stade. Unique in the centuries-long history of the debate, DIO’s solution is physical not metrological. Again (§11, note 119), more difficult sexagesimal matches are targeted data within about 5% of ancient data wess. [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 metrological rehashes), that keep folding journals’ pages, can only match 1 out of the 3 (and even that match is usually several times looser than 1%). Notice the astronomer-defeating surprise that the Pharos solution of the famous Sostratos-Eratosthenes C is geographical, not astronomical. [Speculation follows.] But refutation is effected when we realize that the uncluttered (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 sources had obtained an accurate \(C\) to which Greek science had naturally applied standard Greek sexagesimal division to the Earth’s meridians (the process’ step 1 is even attested: §3 fn 111) to define the stade so that \(C = 60\) or \(216000\) stades. I.e., sexagesimalization (triple division by 60) of Earth-C 40 million meters produces a geocentrically 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 how long ago were the stade-long-footrace markers now found there (Engels 1985 p.298) actually established? If these races must have remained valid races, there must have been (at some point) at least one true stade found at Dinsmoor 1950 pp.250-251, which presents, for five ancient stades, the disparate lengths of the traditional one-stade-footrace. The only one which is 185m is also the only one (Athens, reconstructed +143) that is post-Ptolemy I. In the Hellenistic world, where else than Egypt would terrain allow a long flat North-South arc, of ordnmg 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) seems unlikely (\(C = 410\) in 1 part in 106), that the cities’ latitudes, \(C = 410\) in 1 part in 104, the latter being reliable to indicate Earth-C so accurately that hypothetical surmises’ 18+5m conclusion was trust-worthily 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 reigned over his own new bootie in the Domesday Book. The above sexagesimalization method possible tholodolies (transit instruments, with the superior technology that effected the conquest of Babylon, which lacked such advanced as theodolites and trigonometry tables), given Timocharis’ accurate theodolite-based stellar killings 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 §§F7K9). From solar observations (e.g.,
astronomers did their highest precision celestial work (Alm 7.3) by recording angles in the old
predominantly 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
radius 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 Baby­
lon arrived at its naked celestial tables (Dicks op cit §C: DIO 13.1 §2 fn 4.) 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) than not necessarily proving, mere derivative degeneration from its
conquerors’ superior technology. (See also Rawlins 2018U §4.) An obvious & devastating
point, never previously emphasized for its implications: not a single trig table survives in any
form from Seleukid-era Babylon. Babylon observed no solstices or equinoxes (Neugebauer
1975 p.366), or meridian or vertical observations of any kind, because (Rawlins 1991W
§E3) Babylon had no transit instruments (vs Greek celestial transit data from c.300 BC
onward, at Alexandria and Marseilles), and accordingly didn’t know or care what the city’s
gerographical latitude L was. Which may explain why the only attested figure for it, 38°,
[i] is illusory, strictly in Greek records, not a word on L. [ii] L is in any case, in conformity with mental
another crushing blow to Babylonianist pretensions, and [b] is too far north by 2°28'1
148 nmi. Finally, while Greek planetary order was physical — Mer-Ven-Mar-Jup-Sat —
Babylon’s was astrological, Menaclef: Jup-Ven-Mer-Sat-Mar.

N14 Failure to learn anything from the first of DIO’s eclipse-cycle solutions (§3 §33),
in which equating 9460 monthly synods with 781 sidereal years (the interval between two
attested local-midnight lunar eclipse records, Babylon —719/3/8-9 & Alexandria (Heron)
625/3/13-4) neatly recovers all 10 digts of Ptolemy’s previously mysterious final lunar-equation
(Rawlins 1996C eqs.21-31): 8523 tropical years = 10546 sidereal months. (Had HJADists
not shunned this remarkable match, they might well have anticipated the vaster
discoveries of §N16, 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 § [4]) reconstruction of Venus’ accurate pre-blunder synodic motion.

N15 Among the most egregious of all inversions of ancient astronomical procedure:
our uniformly on-the-nose huge-cycle-solutions of no less than 4 long-mysterious periodic
lunar mysteries (§§N14 and §§N16-N17) fall upon locked-up minds, disagreed without a
glance by jeering HJADists who (frustrated in their inability to find error in hersy’s math but
determined [§B3] to reject the obviously probable in favor of the preconception-accordant improbable)
resort to whatever dodge will serve. Besides jeerleader Jones’ private carelessly
unchecked & glaringly one-sided misfire (§N16), there’s a long-popular theory (item [E]:
pu into writing privately by P.Huber, likely inspired by Neugebauer [also primarily a
mathematician, like Ptolemy] that is so inverse-contrary to sensible scientific practice as to
gain special popularity among gapting cult-minds, thirsty for any refutation of undeserving
outsiders’ proposals, thus not just suggesting but insisting-upon HJADists’ pure speculation
that very long relations (therefore must have originated in splicing together all 6 much
shorter relations, & no other hypotheses need apply. The truth was 180°—opposite from
this rigid position, of course: ancients’ more-convenient short relations descended from
less-handy ultra-long empirical ones [DIO 11.2], for reasons about to be explored, below.)
Which demonstrates yet again our HJAD’s unerring attraction to the erring. Considerations:
[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 = (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 degrees, from the cycle’s imperfect), & (see item [D]) [ii] too far west, not sidereal. [D] Genuine, huge, observed integral period-relations of the
planets were percieved in Ptolemy’s Planetary Hypotheseis (Neugebauer 1975 p.906 Table 15), mostly on the order of 1000', verifying to all but spulse-claiming HJADists
that long cycles were recorded in centuries— separated pairs of raw outdoor observations
of stationary points at the same star, as explained by Neugebauer 1975 p.390, producing period
relations without remainders, just as in Plan.Hyp. (Neugebauer loc cit also 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.555
proves an atypical ancient text that for Mars spans sidereal cycles of length 32' (5° above
of 15 synodic years) and 47' (4° beyond 22 sidereal revs), to produce 79' (1° above
of 37 sidereal revs) with the advantage of reduced (but still non-zero) remainder. (Neugebauer
or source mistakenly renders the three day-remainders as degree-remainders.) But there’s
zero testament for the hilarious idea that any ancient scientist did (or would expect) to
construct a reliable 1000' period-relationship 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 (unremnndered) period-relationship of the direct outdoor-
obtained type which was already empirically available anyway ( & more accurately 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 unremndered outdoor-observed 427-sidereal-yr integral-period-relationship, not

N16 Out of typically exceptional and (www.dioio.org/thru.html#bsxv) unconfirmed certainty
that 13th century BC Babylonian observations couldn’t have occurred, privately scoffing at
& non-citing as utterly, a priori-ridiculous the only solution (§§I34-I37) YET discovered
for (any, much less) ALL of the last 3 hitherto-unsolved anciently-adopted lunar motions
([A] System A; [B] draconic; [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 returns), 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
tester at §N14 lacked such divisibility.) Consult esp. the half-dozen next evidence and fits
(Rawlins 2002H §§C3-C9) backing the theory that the draconitic month was determined
by Hippiarchos, by using the very same back-end —140/1/27 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 origins from shockingly obvious 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-ordmag—a million or better.
The proposed method: [1] 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 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 ($\S$M5) significance of the Almajest’s three lunar periods’ high accuracy — but to then let shunning dictate refusal to cite the 1st solution ($\S$N16) anyone has yet achieved, for how these periods were obtained?! That’s non-citation with an impressively unanimous lockstep. . . . (Is there a prize we don’t know about, for this special brand of perfection?) To summarize, cultists keep secret from JHA readers and the larger public BOTH: [1] the historically revolutionary empirical significance ($\S$M3) of the inescapable fact that all three ancient lunar-speeds are micro-accurate, and [2] the eclipse-cycle source ($\S$N16) of such accuracy. (Let Animal House’s Dean Wormer top THAT Double Secrecy.) And don’t miss the saddest&sadist-masochist part: all disbeliefing archeologists 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 revealing in the mathematical recovery of the greatest heritage from Babylonian astronomy, not to mention the earliest major source: observation and not observation for roundly 2000 years of precise eclipse-records, now-lost, but invaluable to classical-era scientists, who, without the huge $N$ ($\S$N15) 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. Rejections of DIO’s 3 big-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 ($\S$N14 & $\S$J3 [$\S$33].) [b] Is it JHAD-banned to use intelligence to induce Greek astronomers’ possession of the $\S$N16 front-end eclipse-records? — just as DIO induced ($\S$N19) Hipparchos’ — 157 solstice & adoption of Kalippic motion, both of which (after same JHADists’ scoffs) turned out ($\S$N19) 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 inductive mysteries here. Who desires a field with no challenges, no advances beyond texts?

N18 Rigid, total, as-usual-unanimous, high-olds-oblivious rejection of 5 new data-match-based proposals central to the field: [1] Computing from his saros-based 4886 Great Year, Aristarchos originated the “Babylonian” month, 29 $1/10$0’050’, decades before Babylon ($\S$3 G4; or Rawlins 2002A eqs.-4.8). [2] DR’s 2001/6/27 British Museum lecture showed (ibid: eqs.-9.11; or $\S$3 G5) Aristarchos applied to this the Metonic cycle (235 months $\equiv 19$), resulting in his Metonic “tropical” year $Y_A = 365\frac{1}{4}/1 - 15/4868$ (fatefully off by $6\circ$, virtually same as Metonic cycle itself). [3] Years before item [2] was discovered, the matching recovery of exactly this yearlength was discerned in Vatican-held ms data listed under Aristaehos’ name onvat. gr 191 fol. 170’ (data at Neugebauer op cit p.601, $\tau \varepsilon \delta ^{9} \kappa ^{2} \delta ^{2}$ or 365 4’20’ 60 2’ if expressed as the continued-fraction $365\frac{1}{4}/1 + 1/(20 + 2/60)$). [Rawlins, op cit. eqs.-12.13, this is $Y_A = 365\frac{1}{4}/1 - 15/4868$, verifying above item [2].] All without altering any Vatican document number and much aided by Neugebauer 1975 p.602’s perceptive interpretation of $\varepsilon$ as sixtieths. [4] The sidereal-year companion data, Aristarchos, op cit $\varepsilon \delta ^{9} \kappa ^{2} \delta ^{5}$ or 365 4’10’ 4’ from at gr 381 fol. 163’ (Neugebauer 1975 p.601), we write as continued-fraction $365\frac{1}{4}/1 + 1/(4 - 1/(10 - 1/4))$ ($\S$G2; Rawlins, op cit fnm 14-15), yielding sidereal year $Y_A = 365\frac{1}{4}/1 + 1/2$ (good within a few time-seconds), again altering no Vatican-ms digit; and, again, a hit: the
References

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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 pipsqueaks who tear down giants, adding that Ptolemy might not be a giant, but Robert “Newton is a pipsqueak.” Did Flanagan even know that Newton was the scientifically brilliant Space Sciences Supervisor of the Johns Hopkins Applied Physics Lab? Not if he listened to the mathematically-challenged [N7 and fn 8] trio cited in the piece he published [Swerdlow, 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 incomprenhesion 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? DIO 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-rockbottom brand of “research” skewered in the following pages reveals DIO’s pseudo-fakery. In summary, during NatGeoSoc’s {1989/12/1} dawn around the launch of NGS’ amateur data-juggling defense of its dying Peary North Pole hoax:

Orchestras more fiddle factors than the New York Philharmonic.

A physicist-astronomer examines the integrity, refereeing, technical skills, & evaluation-criteria of history-of-science archon journals, taking ancient astronomy as a test case. His qualifications for this review include such researches as: long-world-standard edition of Tycho’s Star Catalog, www.dioi.org/vols/w30.pdf, DIO vol.3: efficient eigenvector method for Isis’s resistance to the be-

Universities’ science departments deserve to know the kind of mis-math (fn 13), heresy (fn 5&11, data-tempering (§§B-G), & idea-claims (fn 10, §C8) too often passing for scholarship in prominent but joke-refereed (§2 fn 3; Rawlins 1991W fn 6) & cover-up-prone (fn 10, 11, & 97) journals in history-of-science, a field ripe with smears (fn 8), shunnings (fn 11&16; Rawlins 1991W fnn 171&173), threats (fn 10, & rejection of normal science (bizarre details: idem & §126; fn 100) if favoring heterodoxy, with research-advances’ acceptance contingent upon whose clique the discoverer belongs to. (Repellent examples: Rawlins 2017E §G3.) Further, there’s little evidence that archons teach, value, or even understand (§§G5 & 11 [f], fn 42&106) exploratory hypotheses’ use, tempered by Occam (§I25, fn 33, §I A, & fn 49), to expand&define knowledge. The result (p.45 & §§B5-C-G) regarding advances in ancient astronomy, is inevitably more destructive than constructive.

While incomplete, the following chronology is a start towards top academic institutions’ enlightenment re current-history-of-science’s failings. (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 receptivity.) Mathematical scientists’ scrupulous verification encouraged.

Volunteer referees welcome (since the perps lack the will&skill): dioi@mail.com.

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Continuing the history-of-science cult’s staunch tradition of exiling and gagging-smeared-such-math-competent, even eminent intruders as van der Waerden, R.Newton, H.Thurston: despite physicist D.Rawlins’ half-century of astronomical-historical researches (samplings above & p.2), a stand version, www.dioi.org/gjs.doc, of the following please-clean-your-

However incomplete, this corrects a start towards top academic institutions’ enlightenment re current-history-of-science’s failings. (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 receptivity.) Mathematical scientists’ scrupulous verification encouraged.

Volunteer referees welcome (since the perps lack the will&skill): dioi@mail.com.

Historic astronomers’ most prominent historians-of-science ALTERING DATA (esp. §§C-D&F-G), uncorrected-unredacted math-batches (§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 (§I), and systematic non-citation of the scientific-history journal DIO, though for over 25 years it’s been easily the most mathematically and astronomically competent journal in the science-historian’s vol. 10 even only exceptionally co-published (with the University of Cambridge), long supervised by boards of historians of that rare minority of scientifically capable 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, 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-year-old tables than to the outdoor sky, none doubting (§2 §N8) Neugebauer-Gingerich’s Science’s decree that an astrologer & clumsy faker whose frauds damaged & retarded predictive astronomy for 1000 years (§2 §A) was “The Greatest Astronomer of Antiquity” (fn 1 here). Have shunning, census-data, censoring, & viciously (§4 fn 2) defending naked fraud by a cult-glorified pseudoscientific superstition-peddler (long notorious among scientists) devoted to merely-tooled-to-insistently-normative? Simultaneously with Isis’ resistance to the below-

Institute for the Study of the Ancient World’s Director, tfully brilliant Alex Jones. After decades of observing science-shy historians-of-science and watching limited

Values the ability, and openness at history-of-science’s most eminent & incestuous forums, which now exist in a state of such evidence-immunity and no-consequences self-rule that they have for

Oppositely, the manyways-obvious (fn 42&97) truth of historical decision (refereed scientists have learned). If this is the top of history-of-astronomy, one can imagine what’s going on underneath. But, then, actually, one need not imagine, since scores of examples of the field’s too-ordinary amusing scholarship are catalogued at www.dioi.org/jbh.htm, the oddest being “science” as the Earth’s East Pole (Winnie the Pooh Chap.9): [a] the 1976 Dictionary of Scientific Biography 13:321 discovery of the Autumn Solstice and [b] JHA 22:2:119’s 1991 discovery of the Winter Equinox. See §§C-G for data-tempering by top pols, incl. the NYU Institute for the Study of the Ancient World’s Director, fitfully brilliant Alex Jones. Isis’ 2017 coverup of its 2016 sham-refereeing disaster (p.8 here: 7 largely-obvious undetected errors, 2 of them crippling) is just the latest example of the level of equity, ability, and openness at history-of-science’s most eminent & incestuous forums, which now exist in a state of such evidence-immunity and no-consequences self-rule that they have for a 1/2 century been tragically & punitively insisting (awful details: §2 §N8) on the very opposite of the manyways-obvious (§110) truth of an issue as central as Greek astronomical

continually becoming ever-more-incapable of self-righting the field’s ship.

After decades of observing science-shy historians-of-science and watching limited mentalities (fnm 8&96) like careerists Noel Swerdlow and Owen Gingerich be elevated to eminence that empowers their dementedly vicious smears (e.g., fn 34 & photos linked

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

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

DIO-J.HA 22 ‡2 The Greatest Faker of Antiquity 2018 D.Rawlins

44 DIO-J.HA 22 ‡2 The Greatest Faker of Antiquity 2018 D.Rawlins 45
History-of-science — Data-Tampering, Idea-Theft, Seminumeracy, Smearing, Shuns, Club-Prefereeing

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 Aliibiers 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 (Johns Hopkins University Applied Physics Laboratory); and [b] are being smothered by a chauvinist battery of destructive, data-disrespecting — even data-fudging — papers, whose logic ranges from desperate to supernatural; displaying scant evidence of refereeing or such epistemic canons of scientific evaluation as simplicity, minimal-premises, fruitfulness, and predictivity. Auto-rejection has been inspired by durable permanent magnet (Diller 1984 fn 26) orthodoxy that the famed ancient data-faking ([§18], bumbling ([§1 §E] mathematician-astrologer Claudius Ptolemy was “The Greatest Astronomer of Antiquity” (like hype at, e.g., fn 9) whose allegedly-outdoor solar observations’


DIO argues eternally for high Greek accuracy ([§10; Rawlins 2017E) vs Histsci reverence for allibi-ing inaccuracy, to ameliorate Ptolemy’s gross fabrications, e.g., inventing relation of theory & evidence (Ragup cruelly: 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 [AAAS!] & Gingerich 2002. Since Ptolemy’s Almagest contains much of what survived from ancient mathematics & math-astronomy, it has become accepted-in-practice that grantmanship requires continuing pretense that this invaluable astronomical handbook (the 1st great modern translations call Ptolemy’s Almagest and Geographical Directory “handbooks”: see each’s title in References below) was primary science (fn 9), not derivative (which it obviously was: [2] §§M2&N20 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-odds, perfect-hit solutions, which have the effrontery to contravene current orthodoxy, encourages vulnerability to adopting embarrassingly unlikely alternate theories, and thus (effectively) escaping into the miracle world of the supernatural, palming off — as valid scholarship — notions unworthy of a rational enterprise. For a JHAD-wayhouse of ultra-outre occultisms, see here at: §§C1, D2&D3, E2, G7-G9, G11, H4, I22; fn 12, 13, 33, 44&45, 55, 68&69, 89.

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

4 Wikipedia’s article on the virtually unrefereed 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’ Wikibiography.) Meanwhile, the best-refereed journal in the field, DIO, is repeatedly, aggressively classed by Wikipedia as Unreliable (not deserving an article, with bio-references to Rawlins as “publisher” persistently suppressed), though neither Wikipedia’s 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 having to try) discerned dozens of flagrant examples of prominently-published history-of-astronomy catastrophes that nobody refereed with care (or cared to referee): see fn 50&86 below and at §1’s POSTSCRIPT, also www.dioi.org/jhb.htm, www.dioi.org/fff.htm#xgc. Historians-of-science seeking ready buddy-publishing (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, self-perpetuating cartel: “The Muffia”. (Considering the history laid out in the current paper, who could resist such apt appellation?) A secret of maintaining eternal mufery is JHA’s rigorous refereeing of sufficiently hugely contra-reality super-adherence to 280° old indoor tables, is uncriminal since Greek astronomers were theorists not empiricists, who suppressed — i.e., destroyed — data inconsistent with prevailing models (fn 8&9 below). The Princetinate’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 debied by physicist R.Newton’s 1977 Johns Hopkins University book, The Crime of Claudius Ptolemy, and by D.Rawlins’ scientific history journal, DIO ([www.dioi.org/diomd.htm], which has fitted to attested ancient data scores of new heretical reconstructions (many evaluated below, esp. §3, with selected links), meanwhile asking how ancient astronomers copying predecessors could advance to their surprisingly numerous but heretofore remarkably unappreciated high-accuracy Greek measures. (Below, compare §110 & §11D) to craniolithic cult-insistence on Greek inaccuracy: fn 1, 8 [1], 69, 93, & 584.) Also investigated: ahistorical myth of “theoretical” Greek non-empiricism & data-selection; Occamite resolutions of such problems as ancient Earth-measure by Pharos flame & double-sunsets; the method explaining all 3 Greek-adopted monthlengths becoming undeniably accurate to 1° or better (!); how all hitherto-extinct lunar speeds were based on classical-era use of 13th century BC Babylonian eclipse data ([§34]; pseudo-Aristarchos’ 1970s retrograde Moon & Archimedes’ degree-use (both obvious, yet unnoticed for 2000°: §§11&12 below); Ptolemy’s celestial fakes; Hipparchos’ elaborate and 1°-accurate trig tables, effected by 1°-accurate trig tables; his use of spherical trig: Archimedes-admired pioneer in heliocentricity & spatial-vastness, Aristarchos: P’Tannery’s and DIO’s quadraply-verified (fn 88) reconstruction of his temporally-vast 4868° Great Year, 1°-accurate monthlength, and pre-Hipparchos discovery of precession.

One of R.Newton’s favorite expressions for counter-revolutionary mis-scholarship: A subtraction from the sum of human knowledge.

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

A1 In the contemporary history-of-ancient-astronomy subfield, numerous coherent, mathematically-copper-fastened, expertly refereed, but archon-offending progressive discoveries have appeared for decades, elucidating hitherto-mysterious ancient data. When these offenses cannot be undone frontally, certain careerist serial knowledge-subtractors, — compactly called the 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 JHA against heterodoxy by publishing effectively unrefereed articles that:
impunitively ignore mathematically and logically solid but cult-displeasing findings for decades. This transparent behavior continues even despite 2002 national attention to “unprofessional” acts characterizing the Ptolemy Controversy (ancient astronomy’s hottest). Are debate-averse conservatives justified in unprofessional contempt towards unprofessional-appealing 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 clinging shy of scientific critics. Which suggests several questions that may lurk behind superficial arrogance: [1] Are their deputies like French deans staffed by pros who don’t speak French very well? And pretend it doesn’t matter. (It does: §§7; Rawlins 2017E §K2; Rawlins 2018v end.) [2] Is this why 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 fellow historians-of-science won’t ever ask them to? [4] Does that relate to the inverse: science-historians’ endemic reluctance to acknowledge non-club-members’ condemnations? — a reflex which can reach such extremes (fn 17 & §F5) as to defy any known academic ethical code. (Outside sororities: Rawlins 2005R Epilog.)

B  Hiding Modern Empirical Data: Boomerang Irony & Lawlessness

B1  When today’s Ptolemyists 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 & 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: [DIO 1.2 fn 35] higher expertise?) 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-oscillating contributors. One JHA Advisory Editor privately estimates no real refereeing is occurring, while another (who wanted DIO to stop refereeing at all, realizing it was the only way to compete with other Hist.sci journals!) believes papers by JHA favorites aren’t refereed, but rather preferred: straight-to-press unread (flagrantly, laughably clumsy Centaurus example written by JHA boardmember: fn 50 below), a practice 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, 38F ago: DIO 1.2 fn 66&[B4] whereby “editors” become little more than printers. Note DIO’s typical inhouse pdfs, 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-inevitable JHA-published odd and/or miscomputed scholarship, see: www.dioi.org/JHA fn10-11&86. No like list of botches in DIO has been or can be compiled. Maybe due to scrupulous DIO refereeing?

At least until afternoon tea.


6 O.Gingerich, longtime head of Harvard’s History of science Dep’t, deforms Ptolemy-skepticism, 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-doubting party as just a tiny “paranoic” bunch (§H 5), merely for objecting to communal shunnings he and everyone knew (or would know) are real, fn 5 here; and www.dioi.org/pm1.htm, www.dioi.org/j43f.pdf, “Naked Came the Arrogance”, Rawlins 1994S §§BS-B8; Gingerich, loc cit; and cult-echo slanders sampled at www.dioi.org/§111.pdf, DIO 1.1 §1 [§7; also see fnm 16&20, 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.htm. His refereo reports on skepticism often pretend (between slanders) that it would be ever so good to have the other side heard (¶A2). Were this not so, 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,
In 1987, in order to justify the modern Ptolemaic vision of antiquity, JHA-Editor-in-progress J.Evans published an exceptionally polite, technically pathetic, but politically brilliant Step-One towards becoming a Muñia 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 teamwork-contribution to the pretense that Newton was as crazy as its cult’s unanimous goosestep-slander was insisting: In 34, Which is why the parties soon proven right [14 §B4 and idem §D10] Catalog were all hydrophobic, while those who were impenitently wrong were elevated — the most-impenitent lifted into the field’s politically-ultra Editorship. No surprises.)

Evans’ paper tried alibiing 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 Spica vs the eclipsed Moon (Almajest 3.1) by Hipparchos in −145 and −134. The errors were all ordmag 1°: respectively, −40°, −33°, +33°.

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 §A (emph in original), critiquing Evans 1987, http://journals.sagepub.com/doi/pdf/10.1177/002228628701000401, also Evans 1998 (appropriately reviewed for its considerable merits by Thurston 1998D in DIO 8), 2009. Question-overs anymore — with as for the Moon-star case at hand) the evidence is too obscure for non-specialists to understand, and at least 1200 (§B8) have been newly, clearly, uncomplicatedly, high-schoolishly, arithmetically indoor-computed his four alleged “observations” of the Sun: §B8 below. A textbook case of politics overwhelming reason — as it has, for ordmag a century of history-of-science’s ubiquitous, naked promotion-for-grantproof 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 assuring us that Ptolemy had too much “integrity”.) Further history-of-science contributions to ethical philosophy are announced from the field’s heights by NYU’s A.Jones (who knows Ptolemy faked science [§2 fn 2], but ranks true history vs JHAD status) in canny political order: the Jones-edited 2010 Springer volume Ptolemy in Perspective (Caltech® 2007 conference, arranged by Caltech’s Sوردwoerd), is prefaced by Sوردwoerdian prose, “Among the SCIENTIFIC authors of the Greco-Roman world, none gives us such a strong impression of writing for posternity 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 §A1] 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” (Sوردwoerd 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 Caltech collection, we’re told (Rapef op cit p.126, emph added), in a typically (2 fn 18) condemnationless history-of-science cliche “reply” to the fatal-for-scientists revelation that Ptolemy indoor-faked allegedly outdoor data: “But let us look at this another way. Ptolemy decided not to tamper with the year[length] he had inherited from Hipparchus” (the very datum used for 4 of 4 of his sun data epochally discovering accurate precession thereby: §2 A) —所谓 outrageously tampering with data is the scientific&etrical equivalent of real scientists’ tampering with theory when improving same: just two O.K.-options for resolving theory-vs-data conflicts! Consider the revelation: this CALTECH-SPONSORED expression of outside-and-outside is considered the epitome of DEEP non-judgemental thinking, in the history-of-science commune, where no one in authority seems able even to tell a real ancient scientist from an occultist fraud. And see 1 §H&K on the field’s robobrushof Ptolemy’s connection to astrology, believing in reckless defiance of the awful truth (§111) that only unenlightened, “paradigm”-insensitive scientists could suppose it reflects negatively on him.

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

Among the gymnastic hystorical-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-cient 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 [Ptolemist] lesson (emph added): “No better demonstration could be wished of the uncertainty attached to the method” of fixing stars’ longitudes by eclipses. However, when inductor 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 [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 bellyflop... Contra Evans, neither his own nor Hipparchos’ problems were observational. Both simply miscomputed... valid observational data by using invalid math: the wrong sign for their parallax corrections . . . the [1981 Seattle] longitudinal lunar parallax $p_a$ was virtually 1°/3. . . . the sign mixup would naturally cause an error of . . . [&] the laughably improbable discovery [here in fn 12 & §101 & “observational” link] that Hipparchos did indeed (Evans 1987 loc cit): “too small by about 40”. (Typically, Evans has no comment since, despite [Rawlins ... face-to-faces] & Hugh Thurston [by letter] directly bringing the matter to his attention.) After correcting for this Muff, we see the admirable smallness of the 1981 observational error of Evans (a dedicated student of ancient instruments & possessor of a steady hand, since the cross-staff requires it): merely 1 or 2 arcmin, just the sort of accuracy DR has consistently ascribed to the ancient . . . observations.

field has a word to say in criticism. Parallel Evansiana: despite denial of reading DIO. Evans’ clumsy unannounced sly-try 1998 eclipse switch (unsuitable details: fn 11 below, or fn 47 shows) he read Rawlins 1991W fn 288, DIO’s detection-revelation of Evans’ 1987 parallax-misuse, ever-uncited by him during 3 decades of hiding from publicly facing this central demonstration of his cult-engendered fallibility. Also ever-Evans-uncited: DIO 3, www.dio.org/j301.pdf, the standard critical edition of Tycho’s 1004-Star Catalog. Rawlins 1993D (fruit of DIO’s 7 years of scrupulous investigation & math-reconstruction: 1987-1994): flagrantly deliberate citation-avoidance in U.Press Evans 1998 pp.271-272 & n.28 thereon (p.459), §7 after DIO 3 appeared. (Will scholars not following suit offer this book deeper-hyper-tender sensibilities?) In any case, published because published by a journal correcting an Evans mistake he pretends was never made? (But doesn’t say so. [Indeed, doesn’t say anything.] And no historian of science asks. A field ruled by fear for decades. But note that B.Schaefer has admirably broken ranks on JHA-shunning of DIO 3.) More EvansScience appreciated in Rawlins 1997Z §H[H-H & fn 65, and at Rawlins 1993D §L8, where Evans is shown to unwittingly require Ptolemy saw 12th magnitude stars. (Ptolemy-Flamekeeper Evans in 2013 succeeded Hip in as ‘Hip’ Editor. Utterly apropo listing at www.dio.org/j501.pdf, not listing further examples of deliberately-ignored (recall 1999/4/2 letter, above) revelations of undeniably erroneous but never-retracted Evans scholarship. See also DIO §8 § 4 fn 4 on the unsuitably-absolute & inadver-tently non-empirical—empirical argument at Evans 1998 p.72, ultimately adopting (non-citationally) yet another DR original discovery (§4, DIO 1.1 §7 §C1; Rawlins 1991W §9r, fn 263&272; Rawlins 2008R fn 17): Aristarchos’ 87° half-Moon elongation as not precise but a lower limit. And don’t miss www.dio.org/jb/h.htm#cgjm, on Evans twice copying J.Drewry’s prose without quotation notation. Irony: DIO’s Tycho star catalog & the differently-important Hipparchos parallax-sign discovery, were both triggered by Evans’ own mis-apologia for icon Ptolemy. At Rawlins 1992T §H8 & Rawlins 2009E §A6, find our gratitude to Evans&Hoskin for each of these gifts. (Like thanks to Jones&Toomer at ibid fn 207&292; to Duke, §C14 above & Rawlins 2012V fn 22; to the whole Muffins at Rawlins 1991W §L8.) Evans’ modestly precludes him from citing any of these thank-yous. Compare to DIO’s rule of always correcting its errors — for both integrity and refusal to mislead readers: www.dio.org/err.htm, as well as below at §14 & fn 98k&110, also DIO 1.1 §4 §A2 (1991) & DIO 11.2 (2003) front cover & p.30. JHAD’s perverse reaction to the contrast neon its priorities for all to see. And evermore. 11 Rawlins 2009E fn 4: “Both inquirers were told by now JHA Editor Evans that he would look into [DIO’s correction to his eclipsed-Moon-vs-star experiment]. But he never communicated what he found. Except by implication” when in 1998 (ibid fn 7) he 1984ly suppressed [with notice to readers] his 1987 paper’s 1991-DIO-outdated observed&bungled 1981 eclipse-star measure, then [emburrowing imfamous con man Dr.Cook: www.dio.org/j03.pdf, §C3] nervily subbed, into the same Greek-accuracy-demeaning Maffadocvy, a nonmeasured 1977 eclipse instead. Historians-of-science know of this conscious historical distortion. None objects. None felt it bore on Evans’ 2013 apotheosis to JHA — the #1 political office in the history-of-astronomy field. Kult über alles. With predictably-degenerate Evans history-of-science surfacing yet again in 2015: fn 10&42.

B5 (Do years of not admitting parallax-mismath by now equal a kind of data-alteration?) B6 The above-quoted 2009 article added: [i] detailed proof of DIO’s 1991 contention that the 2 superficially-alike-looking Hipparchos lunar-eclipse-star-plant-measurements cited above were accurate to ordmag 1° if his parallax-corrections were correctly signed; [ii] further, if Hipparchos’ hitherto-unexamined only other eclipse (~140/127) was used to fix nearby Regulus, undoing the very same parallax-sign-error shrinks the very apparent ordmag 1° error (common to all 4 mis-longitudes) down to just a few arcmin (7°, mostly rounding error), yet again.

[Note: All data are subject to discovery from ordmag uncertainty in that era’s ΔT.]

B7 Exact data (Rawlins 2009E fn 22): JHA’s acceptance of the unchallenged record leaves them ±34 & ±1987 errors of, respectively, Δt = –33.4°, Δσ = 1.3°. Idem shows that removing proposed parallax-sign-confusions, as well as accounting for the serious systematic errors of the solar orbit Hipparchos used for his estimation of mid-eclipse time, leaves errors in empirical Moon-vs-star gaps of, respectively, just –2°, +7°, +1°, +2°. B8 The former amounts are obviously less credible than the latter, when set in the context of Hipparchos’ other known observations’ mean single-datum scatter (fn 12): 0°.1 for 3 lunar-limb-vs-Sun measures, Almajest 5.3.5 & 2° for 17 solar equinoxes on Rhodes (mean’s traceable error 0°.7, mostly non-observational: Rawlins 2018U §4B4), Almajest 3.1; 5° for 17 stellar declinations (mean’s error 0°.1±: Table 2 below), Almajest 7.3. B9 The ore-refinement findings by DIO for the three star-vs-eclipsed-Moon data raised by the Ore have shock-vindicated DIO’s longtime position that Greek scientists were empirical and accurate — and the Regulus case constitutes classic coherent theory-fruitfulness. B10 Has Evans produced his 1981 written record, to refute DIO’s 1991-2009 stimuli? No. (fn 127 below); and colleagues cooperative averting gaze (fn 11 above) is consistent with the dreary theory that they prefer their own flubs be granted like silence, in return. Mutual consequence-free lawlessness.

C SphTrig’s Debut: A.Diller’s #1 Discovery Mobbed by Half-Fits C1 Expressing them in stades north of the Equator, Strabo preserved a dozen-plus Hipparchos geographical latitudes $L$ corresponding to what ancients called “klimata”. 13 narrow referee Toomer, Rawlins 2018S passim. [Rawlins 2018S,]° Pro-Greek-competency: DIO 1.1 (1991) §1 fn 24. Hipparchos’ measures of lunar limb-vs-Sun separation (Almajest 5.3.5), mean error 0°.1: Thurston 1998A Ο.1.1. For Rhodes equinoxes, Hipparchos’ scatter (mean single-datum error) was 2°: fn 70 here. With error 0°.1± (Rawlins 1994L, §3), he found his geographical latitude $L$, presumably from poleslars, knowing stellar parallax was negligible. His $L$ is inferable from his star-declinations, which show merely $5°$ scatter (here in Table 2), Regulus-restoration: Rawlins 2009E eq.8. Correcting the four-star-places discussed here, for proposed parallax-sign-slips & for Hipparchos’ PH orbit’s shortcomings (at that era, primarily an error wave of amplitude 0°.4), the above-$\delta$’s exact before-correction-vs-after data are found here at §3Y (or at Rawlins 2009E fn 22). Evans’ refusal to recognize that DIO’s analyses have ordmag-shrunken all 4 of his and Hipparchos’ longitude errors (each from 1° to 0°.1 or 1°: chance odds ridiculous) implies that he suspects scientifically-irrelevant dark magic — behind treasonous dirty-tricks [fn 48 & 54], & unprecedentedly insufferable quadruple-lese-majeste. 13 Familiarity with klimata is vital to understanding the disgraceful ordmag 1° inaccuracy of Ptolemy’s geographical latitudes. For the purely astrological Hipparchos cause, see, e.g., here at §1 §R, or Rawlins 1985G pp.260f. Both sources analyse evidence consistent with the self-evident theory that organized ancient scientists had corresponded for lunar eclipse local-time comparisons (accuracy limited mostly by ruling-finess of sundials&astrolabes used for timing eclipse-start&end), to find longitudes to a mean accuracy of ordmag a degree (§111 below). See Hipparchos’ advice at Strabo 1.1.12. A very recent Isis lead paper, Shcheglov 2016, attacks this old 1790 theory as a “delusion”, with Rawlins 1985G counter-chronologically designated as repeatedly-1°-cited prime hallucinator. Shcheglov tries to prove that ancients were so incompetent that eclipse-based longitude-differences, as reported in Kleomedes&Pliny, were “badly overestimated”. But see DIO’s 2017/20Z (11) Letter-to-the-Editor (published here as article §11, which dishonest Isis refuses to print or even evaluate since it shows that Isis’ editors & referees didn’t notice that Shcheglov achieves his denigrations of ancient competency
Table 1: Diller Sph Trig Problem: Hipparchan Longest-Days in Hours ⇒ Latitudes in Stades

<table>
<thead>
<tr>
<th>Klim</th>
<th>Longest Day M</th>
<th>L Calculd</th>
<th>Sph Trig fn 14 equ</th>
<th>Round Nearest Degr/12</th>
<th>Conv Stads ⇒ C3</th>
<th>Round Nearest 100 St</th>
<th>Strabo Princ Instrtt §NG</th>
<th>NYU JHA</th>
<th>Jones</th>
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<tbody>
<tr>
<td>Eqtr</td>
<td>12°3/4</td>
<td>0°</td>
<td>0°</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>1500</td>
<td></td>
</tr>
<tr>
<td>Cin</td>
<td>12°3/4</td>
<td>12°36′23″</td>
<td>12°7/12</td>
<td>8808</td>
<td>8800</td>
<td>8800</td>
<td>10200</td>
<td>8800</td>
<td></td>
</tr>
<tr>
<td>Mer</td>
<td>13°</td>
<td>16°35′04″</td>
<td>16°7/12</td>
<td>11608</td>
<td>11600</td>
<td>11600</td>
<td>12800</td>
<td>12800</td>
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</tr>
<tr>
<td>Syc</td>
<td>13°1/2</td>
<td>23°59′43″</td>
<td>24°</td>
<td>16800</td>
<td>16800</td>
<td>16800</td>
<td>17600</td>
<td>16800</td>
<td></td>
</tr>
<tr>
<td>EgL</td>
<td>14°</td>
<td>30°33′49″</td>
<td>30°7/12</td>
<td>21408</td>
<td>21400</td>
<td>21400</td>
<td>21800</td>
<td>21800</td>
<td></td>
</tr>
<tr>
<td>Pho</td>
<td>14°1/4</td>
<td>33°31′04″</td>
<td>33°1/2</td>
<td>23450</td>
<td>23400</td>
<td>23400</td>
<td>23700</td>
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<td></td>
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<tr>
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<td>14°1/2</td>
<td>36°15′25″</td>
<td>36°14/2</td>
<td>25375</td>
<td>25400</td>
<td>25400</td>
<td>25500</td>
<td>25300</td>
<td></td>
</tr>
<tr>
<td>Hell</td>
<td>15°</td>
<td>41°07′34″</td>
<td>41°1/4</td>
<td>28817</td>
<td>28800</td>
<td>28800</td>
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<tr>
<td>Mas</td>
<td>15°1/4</td>
<td>43°16′44″</td>
<td>43°14/2</td>
<td>30275</td>
<td>30300</td>
<td>30300</td>
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</tr>
<tr>
<td>Pon</td>
<td>15°1/2</td>
<td>45°15′40″</td>
<td>45°1/4</td>
<td>31675</td>
<td>31700</td>
<td>31700</td>
<td>31600</td>
<td>31600</td>
<td></td>
</tr>
<tr>
<td>Bor</td>
<td>16°</td>
<td>48°45′50″</td>
<td>48°31/4</td>
<td>34125</td>
<td>34100</td>
<td>34100</td>
<td>34100</td>
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</tr>
<tr>
<td>Tan</td>
<td>17°</td>
<td>54°14′53″</td>
<td>54°14/2</td>
<td>37975</td>
<td>38000</td>
<td>38000</td>
<td>37900</td>
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<tr>
<td>SBr</td>
<td>18°</td>
<td>58°12′31″</td>
<td>58°1/4</td>
<td>40775</td>
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</tr>
<tr>
<td>NBr</td>
<td>19°</td>
<td>61°04′56″</td>
<td>61°1/2</td>
<td>42758</td>
<td>42800</td>
<td>42800</td>
<td>42700</td>
<td>42700</td>
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</tr>
</tbody>
</table>

C2 Mafftyspeically Diller as competitor not colleague. Neugebauer attacked these findings by 1934 letter, later publicly branding them “absurd” and not even to be “taken only by his own highly irrational mathematical mis-steps: details in fn 97 below. From H.Buckle History of Civilization in England 1873 (1:318-320). In medieval times “the credulity of men had reached a height which seemed to ensure to the clergy a long and universal dominion. . . . A book . . . sanctioned by [the most eminent] judges” recorded that the Carolingian hero Roland fought the Moors’ towering goliath Fenaciute to no-decision until he “engaged his adversary in a theological discussion. Here the pagan was easily defeated” and, thus confounded, was quickly slain by the sword. When, despite being repeatedly informed of contrary evidence, our era’s equally eminent whistleblower-reforming SCIENCE journals ( fn 11) disarmingly trust for decades Mennonite Jesus-hugger Gingerich’s insistence that his fellow-occultist&court-AlmaFester was The Greatest Astronomer, of an antiquity on whose ingenious empiricism (§) Gingerich remains incalculably clueless, can we regard contemporary academy’s forums as any less deliberately mythmaking-for-the-cause than those of accurate history’s prior Dark Ages?

14 L = arctan[(cos(7.5λ)/tan e)] (L in degrees; M in hours); Almajest 2.3, Neugebauer 1975 p.38; further sample klimata tables, ibid, pp.706-736.
16 Diller 1934.
17 Diller fits’ perfection easily verifiable here at Table 1, or at Table 2 of www.dioi.org/gj03.pdf, Rawlins 2009S. No JHADgist agrees. Yet the truth is plain to all but those controlling discourse, locked away even after 56/ of a century of invincible non-innocence.
19 Rawlins 1994M Table 1, augmented in later printings, as previously unnoticed Strabo klimata surfaced, each unfailingly consistent with Diller’s theory.

Most Neugebauerians received the 14° Diller-Rawlins klimata table (Rawlins 1994M fn 10 and Table 1. Rawlins 2009S, www.dioi.org/gj03.pdf, Tables 1&2.

Diller 1984 Table 0. The 14 klimata: Equator, Cinnamon Country, Meroë, Syene, Lower Egypt, Phoenicia, Rhodes, Hellespont, Massalia, Pontos, Borysthenes, Tanais, South Britain, North Britain. 1986 Jones cited Strabo 25.38 Alexandria gnomon ratio 5:3 is just a common textual alteration: the original Greek is 5:7, which E.Honigmann&Neugebauer realized (uncited by Neugebauer-protégé & eulogist Swerdlow 2010 p.151) wasn’t a gnomon shadow-ratio at all, but the Alexandria klima’s shortest-longest-day ratio, m:M (Neugebauer 1975 p.336); ignored (favoring Neugebauer 1975 p.101 n.1) by Jones op cit n.3, but the H&N idea’s fruitfulness is independently confirmed via Cartilage GD latitude (Rawlins 2009S fn 35): where same m:M mixup with shadow-ratio (in same Strabo 2.3.38) caused 1000’s of North-Africa latitudinal map-distortion (Rawlins op cit §4; similarly durable longitude-disaster for Arbea eclipse: Neugebauer 1975 p.938).
of Hipparchos. Jones does not merely pollute Strabo’s klimata with these alien data from scattered other works of Hipparchos, but uses them as an excuse to shift the entire dozen, suggesting Strabo’s data could have been actually corrupted by addition of a constant, A = 100 stades (meanwhile Jones consistently miscomputes Syracusan latitude by 200 stades), which he argues must be corrected-for — thereby justifying-excusing his replacement of already-perfectly-fitted unmanipulated data, with sub-50%-fitted (Table 1) revised data. Seeing Jones extrapolating from a very exceptional klimata to the whole set, atheist DR is reminded of creationists who generalize from rare, anomalous geological strata to rejecting natural selection entirely. Obvious point against the significance of Jones’ disparate data-injections: he well knows that Hipparchos changed adopted parameters over time.

C6 Further, Jones (ignoring 23°2/3’s confirmations: [H4: Rawlins 2009S fn 23 & 54]) accepts Ptolemy 1.12 testimony that Hipparchos’ obliquity was Eratosthenes’ ε = 23°51’20”, a value which Jones imaginatively attributes to a speculative Hipparchan computation from a conjured-up non-Hipparchan Alexandria L = 31°, without realizing that obliquity ε would already be known since ancients found it concurrently with L — and via solstices, not (as Jones curiously assumes)27 equinoxes.

C7 Jones also-traditionally alters28 the Almajest’s text for Hipparchos’ Marseilles latitude, from L = 43°0’4’’ (consistent with 43°1/12 of the Ptolemy Geographical Directory [GD]) to L = 43°0’1’’; in order to reconstruct (using Eratosthenes’ ε) 2nd century BC Hipparchos’ Jones-suggested indoor calculation-invention (for unstated reasons, and counter-chronologically) of Pytheas’ well-known longago (c. –300) solstitial noon gnomon ratio, 41.425 (whose precision argues it was an outdoor29 datum); and, to force the speculation’s success, Jones begs tolerance of an odd-but-convenient Hipparchan miscalculation,30 yet another ad hoker wrenching of ancients’ data. [While rejecting Table 1’s normal roundings!]

C8 Jones’ promotion31 of such jigsaw juggling seeks at least a half-share of Diller’s discovery. Worse: by fantastically alleging that Diller used invalid data, Jones (Neugebauer-rially: [§C2] lodges his half-fitted theory — unvetted ([§C10] and unbatedulatively-whimsical — as SUPPERIOR to Diller’s ultimately-perfectly-fitting one. Though willing to refer Diller’s 1934 paper (with 2 nonfits of 11 listed klimata, until DIO’s 1994&2009 upgrades: [§C3 [a] & fn 34 end], shunsoldier Jones dutifully, consistently refuses to acknowledge the bare existence of Diller’s 1994 vindication: DIO’s initial near-perfect 12-hits-out-of-13 table32 (though its merit is prominently recognized by H.Thurston [Izis] & by G. van Brummelen’s meticulous standard history of early trigonometry [Princeton University]) — or the final DIO 16 ³3 update distributed in 2009, where the ultimate blinmles-ult-fit perfection (Table 1 here) of Diller’s 14-for-14 victory is too irrefutable to deal with. Except by fleeing.


28 Original-text Hipparchan Marseilles L = 43°0’4’’ (Almajest 2.6), rounded at GD 2.10.8 to 43°1/12, often altered to 43°01’ (defining GD’s consistency with 43°04’). See Rawlins op cit §H. Achronology: Jones 2002e p.17. Pytheas gnomon ratio: Strabo 1.4.5&2.5.41. Rawlins 2009p eqs.2-3.

29 Ibid Summary [1a] and Rawlins 2009S fn 34.

30 Jones loc cit refers Hipparchos rounded 41.713 (41:42:47) to 41 4/5 (41:48), though 41 2/3 (41:40) is rounder & almost twice earlier. F.Ragep 2010 p.128’s Jones-promo sees no problem here.

31E.g., Jones’ foreword to recent (long overdue) reissue of Pedersen 1974.

32 Initial Diller-Rawlins table of Strabo data compared to Neugebauer’s: Rawlins 1994P p.56 [apty augmented in later renderings; as noted at fn 19]. JHA-uncited for 24’ now, but noted by Thurston 2002S p.67, and by van Brummelen 2009 p.65. Final 13-for-13 version of Diller-DIO klimata table: www.dioi.org/vox/wv50.pdf. Diller 1984 Table 0. The table’s subsequent 14-for-14 expansion: here in Table 1 or www.dioi.org/jg03.pdf, Rawlins 2009S Table 2.
his theory’s doubtless-illusory woe: 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 climatal data\(^{35}\) 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 merely\(^{36}\) constitute a paranormal or religious miracle, with no significance or status in his people’s idea of the real world of science, where Occult’s Razor slices an illusion like a 14-hits-out-of-14 table completely out of that special bubble, consigning it to the Orwellian-Googleblames it deserves.

\(^{35}\)Strabo’s numbers being repeatedly vindicated here (universally-accepted restoration noted in Jones 2002E:n9 conclusion), though his interpretations are fertile ground for reconstruction: e.g., above, in fn 25. Trigonometry-table “imprecisions”: Jones \emph{op cit} p.17.

\(^{36}\) This, in a familiar chauvinist tradition we keep encountering here, e.g., in fn 25&68, and even more astonishingly at \url{www.dioi.org/hr/hr.htmfsqv}, and below in §3. Computing odds against Jones’ theory (\& Neugebauer’s):\(\) Rawlins 2009S §§J1, J3, & J6.

\(^{37}\) At the 18\(^{th}\) kima in above Table 1, 58\(^{th}\) 12\(^{th}\) \& 31\(^{st}\) rounds to matching 58\(^{th}\) 1/4; but 58\(^{th}\) 12\(^{th}\) 29\(^{th}\) wouldn’t.


\(^{39}\) ibid §14. Further deep thanks (not necessarily facetious) to our loyal royal cavillers: here at fn 10, 51, 71, 73, & 98; also §2 fn 42, and www.dioi.org/jb12.pdf, “Gratitude to Opposites” p.10.

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

D1 As recounted in \emph{Almajest} 4.11, Hipparchos investigated two lunar eclipse trios, both of which had occurred well before his era, usually called Trio A (−382-381) and Trio B (−200-199). Toomey reported that, for Trio A, Hipparchos had computed lunar orbital elements from the time-interval and longitude-interval between eclipse\#1 & eclipse\#2, and the same intervals between eclipse\#2 & eclipse\#3. And then did the same for Trio B. Hipparchos’ computational findings were, for Trio A (−382-381) orbital radius \(R = 3144\) units, eccentricity \(e = 327.23/3\) units; for Trio B (−200-199), \(R = 3122.12\) units, epicyclic \(r = 247.12\) units. G.Toomer tried an ingenious and daring reconstruction\(^{40}\) to recover these numbers, wrongly assuming (like \emph{Almajest} 4.6&11 and modernly D.Duke) that Hipparchos had used etopolos’ simplistic mathematical procedure (1991W). Toomer 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), presumably\(^{41}\) 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 \emph{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., \emph{Almajest} 3.1) user of Aristarchan data, he could have computed the \(R\) via Aristarchos’ famous 87\(^{th}\) half-Moon elongation and a typically ordmag-rounded\(^{42}\) solar distance of 1000\(^{th}\), as follows:

\(^{40}\) Toomer 1973; Duke 2008W p.286 also assumes Hipparchos used Toomey’s deft trio method.

\(^{41}\) Rawlins 2012V §4. V’s conservative assumption of outset-fixed orbit radius \(E\): Thronson 2002S p.60. For Ptolemy’s & B2’s & \emph{DIO’s} fits are compared for all 4 parameters at Rawlins 1991W eq.12.

\(^{42}\) Ancient ordmag-rounded Sun-distance estimates: Rawlins 2008R §§D-F. Sun-distance as historical order of origin: Rawlins 2012V §D3. Reconstructed distances: \(a\) \(S_1 = 1000\) Eratosthenes (Rawlins 2008Q eqs.6-13; nearly same at Carman & Evans 2015); \(b\) \(S_1 = 1000\) Hipparchos (\emph{DIO} here; Rawlins 1991W eqs.22-24; Rawlins 2008R eq.12); \(c\) \(S_1 = 10000\) Aristarchos & Archimedes (\emph{Almajest} D2 here; Rawlins 1991W eqs.12-20).\(\) In order to match a "mathematically rounded" solar distance, obviously-orthogonal Poseidonios also proposed at least 1000\(^{th}\): ibid [12], 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: \(a\) Hipparchos tried a variety of solar distances, at some point halving his 1000\(^{th}\) solar distance to 500\(^{th}\), thus solar parallax 7\(^{th}\) (low the inverted basis of 3438/375 \(\propto 491 = .4909\), Swerdlow’s valuable & original discovery, ibid fn 39. \(b\) Arab astronomer Al-Battani foolishly used 1146\(^{th}\) [180-603\(^{th}\)], plainly overexact computed [ibid] via round 3\(^{rd}\) parallax. [3]Almajest 5.15 has non-empirical 1210\(^{th}\).\(\)\(\) 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 aspect of Greek empiricism that it could publish as its Pb paper for 2015 an analysis co- authored by Evans (fn 10 above), Editor of the JHA (“premier” history-of-astronomy journal according to Science 2002 p.40), deliberately-narrow-focus-arguing that Eratosthenes had a solar distance of 102\(^{th}\) (fn 106) — a transparently overexact value — rather than 100\(^{th}\), as realized years earlier at Rawlins 2008Q (eqs.9-12) through common sense, antiquity-sense, & consistency with Eratosthenes’ long-existing actual values (Rawlins 1982W Earth-circumference, 256000 stades. What does it say about the expiatory open-mindedness of history-of-astronomy’s current #1 archon when he finally accepted Eratosthenian solar distance \(2\times 4080000\) stades/252000 stades = 102\(^{th}\), he allegedly never thought to explore-test by dividing (into same numerator) the 2 alternate C-candidates: Kleomedes’ famous 250000 stades (yielding solar distance 103\(^{th}\); Rawlins 2008Q eq.8), or DR’s provably-known-to-Evans-but-uncitable 256000 stades, which yielded 100\(^{th}\) within 100/100\(^{th}\) of \% (ibid eq.11). To obscure his 1987 mismatch \(\%\) above, Evans has also for 27\(^{th}\) now refused to withdraw his claim that he [\& Hipparchos] prefer — MOST-ATYPICAL (\$88+) — to see the Moon outdoors by more than its own diameter, & will keep on (fn 11) ducking (like Isis: fnn 13&100), trusting that academe lacks the integrity to reveal
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D. Rawlins

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R = 1000° cot 87° = 52°24', or, in 60° units, 3144'' — thus matching Trio A’s R (above). Now, a common slip (ancient & modern) is confusion of unit-fraction (inverse integer) & arcmin, since each is signified by a prime-number; so if we test the hypothesis that a Hipparchan-school computer later misread 52°24' as 52°1/2, we find, in 60° units, 3122°1/2 — exactly matching Trio B’s R (above). Unable to counter the math, centrist pols (faces externally, irrevocably invested into shun: fn 125&127) have, during the decades since 1991, had no reaction to this minimal-premises double-match of both 3144'' & 3122°1/2, besides implicitly contending (by pushing incompatible fudge) that this is all PURELY COINCIDENTIAL. Any wonder the above Summary was forced to confront the supernatural? — see fn 2 here.

D3  DIO also found double-consistency with its theory that Hipparchos’ calculations [1] had used eclipse-pairs not trio (the trio-approach has never yielded unmanipulated data-matches) & [2] had sought only one unknown, eccentricity ε (or epicycle-radius r), not apogee-at-eclipse Α0 or mean-longitude-at-eclipse ω0. Thusly computing ε and r produced 327°39' & 247°30'', respectively, each a neat 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 convenient calculations. DR, recently expanding his 1991 orbit-reclamations, compares DIO-vs-Duke simplicities:

that History-of-astronomy’s present Ultimo Archaon mis-signed his 1981 parallax-correction (βB4), but won’t admit so since silence helps keep alive the JHAD sacred cow of Greek inaccuracy. So far, his garish & pretentious acolyte’s honesty has proved perceptive. Ability-consistency of JHA Editor Evans record here (faithfully carrying-on the exemplary tradition of 1970-2013 Editor Hoskin’s grasp of mathematical astronomy: www.doi.org/10.1111/j.6977-1994.2005.470204, especially fn 72. The 151 failure to sign parallax correctly, & [ii] 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 βB4 above), since Eutrophonios’ parallax is given as an ordm 1° (0° 56' 56'' at Carman & Evans 2015 p.14), solar parallax’s uncertainty was ordmag 100% of its size [like ± 1%]. (Rawlins 2008R βC5 argues that Aristarchos suspected an even higher error. So only non-scientist pols like Poltemy or JHA-Dists wouldn’t realize that the JHA-Isis 102° distance’s ordmag-1% (!) precision was way-larflly too exact — when uncertainty is 2 ordmag’s looser. All this reveals an even looser grasp of Greek astronomy & (elementary) mathematical astronomy) by two of the world’s most deliberately-eminent history-of-science journals. Among famous classical Greek astronomers, only faker Poltemy insisted on solar-distance; numbers of such naive wayoverprecision, e.g., Almajest 5.14-16, which Hartner 1980 p.26 justly deemed a “fairly-tale” (like R.Newton 1977 p.198); yet another case where seeing Poltemy as typical of his era, or even its dominant Greatest, has warped history-of-science’s perception of actual science in antiquity.

Rawlins 1991W eq.23.


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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 elsewhere) 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, excepting that Brigham’s no resemblance of era & kind, or anything sufficiently justifiable of fudgers so shamelessly explicit, besides rigorous issuance of The Right Answer. All to smother DIO’s natural-flow-multiflt coherent solution under a pillow fluffy with special assumptions &andelmenting, resurrecting the spirit of co-subtractor Jones, above (in [C] & below (fn 85). Like Jones (&C10), Duke has refused DR’s request to withdraw the paper.

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/prt.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,\(^\text{50}\)). Contra Duke’s attraction to committing fudgery: throughout Rawlins’ researches, it proved unnecessary \(^{51}\) to “correct” any of Hipparchos’ calculations in order to draw coherent results from his data, so a historically new conclusion \(^{52}\) 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 “PF”, in turn replaced by his — 134 Ultimate orbit “U1”. He adopted EH-PAF when there’s no lunar elements from long-earlier “Ptoleosics” Tria A&B (&C13&22, & Rawlins 1991W fn 55). In 1977, physicist Rob.Newton detected a hitherto-unnoticed 1° error in Trio A’s 3rd eclipse, claiming data-restoration here is demanded (fn 91 below) but (very) incredible “consequences will flow from analysis. The orbital elements derived by warning-rejectors Jones and Duke inadvertently double-confirm Newton’s “incredible” prediction. Like Jones, Duke does not notice Newton’s 1° warning, & deliberately (§D6) acts as if the carefully-refereed (§D3) DIO paper that did heed it does not exist, though it was unsuccessfully challenged by DIO’s §6 by Duke to_thrust and (fn 32 above) precisely in Isis in 2002. Duke’s paper independently computes best-fit eA of the Trio B-accordant EH orbit, & of the inevitably weird unrestored-Trio-A-accordant orbit, as if original, though Jones 1991H likewise treats Tria A&B quite inconsistently: Rawlins 1991W fn 209. Poltemy rigged numbers to “verify” parameters: Thurston 1998A pp.3, 13, eq.1, perhaps (§D5) inspiring the approach found in §D4 item [C], among other ploys hereabout.

Rawlins 2012V fn 17, and especially fn 22, where we find that no referee ever read the Duke paper’s TEXT. For practices & business enterprises that lead to similar disasters, see fn 4 & fn 27. Below fn 12. The 1° shift, discussed below in §E turns out not to be a mistake but (worse) a deliberate Hipparchos-school fudge, as shown in Rawlins 2012V §G & fn 11.

ibid §A3.


\(^{50}\) ibid fn.293. The Ake for Tria 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-k&half discovery (§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 1° correction, Trio A is satisfied by unzany elements, which turned out — as discovered 14° later — to be half from the EH Sun orbit (already known from Trio B. §E2 below) and half from 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, while immediately adopting PH’s zeno-point & apogee, neither needing tabulation). Subtractors must see as further mere-coincidences both the correctly-paired split (between tabulated and table-relevant 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 Centaurus-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 Ptolemy orbits: his —157 Early EH orbit (§E1) & —134 Ultimate EH orbit (idem), each seriously differing from PH & previously unknown. (But Hipparchos’ use of a late non-PH orbit was presumably suspected before anyone by Britton a half-century ago.)

F2 Discovery of Hipparchos’ final UH orbit arose from calculations,57 referred and supported by Thurston58 and Curtis Wilson,59 based on realization60 that cuneiform text (eccentricity & speed) that had to wait for adoption ‘til tabulated were naturally those that temporarily stayed EH, while the 2 swiftly-adopted PH elements (zero-point & apogee) were constants thus needing no tables. Perfect manifold correlation-confirmation? Or more DIO witchery? 61


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

58 Thurston 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 precisely Thurston’s followup, to again avoid crediting the zoned discoverer, indeed entirely deleting his name from Thurston’s note: see ibid §H. Alex Jones’ retraction late but exemplary.

59 C.Wilson on Thurston 1995 (in fn 58 above): “I am glad both that the meanderings of Jones’s 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 reverberations 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±357 (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: 1° 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-longing mysteries simultaneously (resulting DIO papers never cited by JHA):

[1] Why the Aristarchos —279 and Hipparchos —134 Summer Solstices are the only62 two among the twenty-eight solar records63 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 positions65 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 Almajest recomputes those true longitudes (via the PH orbit he adopted throughout the Almajest), he twice finds disagreement with Hipparchos’ reported values (all computed

61 Below, §F9; or ibid §§B3&B4. BM55555’s revelation: ibid eqs.6&8. I will ever be grateful to the late Willy Hartner, who was 1° to suggest (letter to DR 1980/8/15: ibid §A5) that scholars (including DR) were ignoring Ptolemy’s hour-omission for two of the Almajest 3.1&7 solar data.

62 See, e.g., the bizarre attempt at Neugebauer 1975 p.284 (followed by Evans 1998 pp.273-274 & n.32, etc., contra ibid p.209, as noted below, at fn 127), to claim that Ptolemy was a BETTER observer than Hipparchos, oblivious to their relative errors, random & systematic (Rawlins 1999 §E — the section of this paper which was suppressed by JHA Editor Hoskin, without showing error of any 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 §A3, condensing the most unexceedable of JHAD fantasies (e.g., centrist-pioneer Aristarchos’ orbit was a non-observing fabricator while go-along-geocentrist & data-faker Ptolemy was antiquity’s ABLE observer. . . . If some oddities are more unique than others, then this one is uniquely unique.” Bringing the foregoing up to 2018: JHAD perception is that 1° known heliocentrist Aristarchos — who discovered precession and the scale & mechanism of the Solar System, knew the stars were at least thousands of times more remote than Ptolemy did — fixed the monthlength and sidereal year to high precision — was a minor, confused figure. But a fabricated, handbook-generating astrologer was the Greatest Astronomer of Antiquity. Or, as our counter to S&T’s gratuitous 2002 Feb smear noted, www.dioi.org/sti56.htm, “Aristarchos (was among other credits) a heliocentric pioneer in promoting realization of the Earth’s place in a huge universe. (Also, he evidently was aware of precession well before Hipparclos: www.dioi.org/jb24.pdf, DIO 11:2 [2003] §4 Appendix 2 §8. He is not known to have been into astrology or theft. He bucked the establishment of his day, which threatened him for his new findings — an ancient prototype for 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 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 scally exploit. Isn’t the history-of-science profession inspirational?

63 In 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 Ptolemy); however, the 2nd alone agrees (by chance, as it happens), though the underlying mean longitude he lists for it is discrepant by 5' vs PH — even while tellingingly 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); xes’ average Moon-Sun elongation was roughly 30°.

[6] UH orbit epoch, the 1st -127 Autumnal Equinox, follows Meton’s sacred -431 Summer Solstice by exactly 304°/4, or 16 or 2° such intervals just equal the 4868° “Great Year” of Aristarchos.

[7] Lastly, the 1st -157 Autumnal Equinox, follows Meton’s sacred -431 Summer Solstice by exactly 304°/4, or 16 or 2° such intervals just equal the 4868° “Great Year” of Aristarchos.

[8] It’s therefore the inexcusably (esp. for the 20th) 157/6/26 (correct) seriously differs from Hipparchos’ 304°/4 below)

[9] Hipparchos’ search for a "Great Year": this is the most conspicuously odd building-block of the lopsided EH orbit, accounting for most of why EH’s e2k were so flagrantly awful: e = 3°/1/4 (vs 2°/1/2 PH, & 2°/1/10 actual), A = 44° (vs 65° PH, & 66°/1/2 actual).

[10] In 1991, Duke, in yet another unrefereed Pb paper for JHA (whose board he had earlier silently joined while DIO Editor), scales new pinnacles of ancient-empiricism-denial, as he tries raising the entire basis of Greeks finding accurate orbits (such as PH&UH), claiming that their equinoctial solar declinations’ error averaged c.15°, nearly equal to the 2°/1/2 PH, & 2°/1/10 actual.

[11] Error-amplitudes compared: Rawlins 1991H at eq.28. Proposed Hipparchan calendar. 365°/4 (Hipparchan calendar) 365°/4 (integral number of days) — every one successively featuring a fresh characteristic cyclic-return, where each of these includes (like the unrefreshed song, “The 1st Day of Zmas”) all the features of the smaller cycles preceding it in the quint-succession. Check it out: you’ll be fascinated at Hipparchos’ hypothesized cleverness. And (idem) successively halving this, yevers, very nearly, sidereal (152°), Kalendric (144°), etc.

[12] Once 2018D.Rawlins,65 this is but a jawdropper-case of confusing systematic error with random. The Duke paper’s Table 1 displays admirably well-computed times of Hipparchos’ equinoxes, whose errors are clearly sprinkled ordmag 0°.1 positively & negatively on either side of zero. Undoing Duke’s historian-usual (4°/43 fn 43) listing of C—O as “error” (C—O), we see he more crucially overlooked that the Vernal Equinox O—C errors are all negative, while the Autumnal O—C are all positive. Since the Rhodes equinoxes were subject to 7° systematic error (found independent by 4 different scholars)56 which corrupted all these equinox

discovery (equally well-referenced: 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 outlandish resenting mob (3.2) — I’m still a successful kill. Because, besides DIO, no known reader — no matter how much JHA referee — of the paper in question (Duke 2008W) has yet read beyond its bald claim (that the Hipparchan 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 10&19, for the three main errors of Duke 2008W.) So, since the defamation of heresy is uncertainly 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 116) if they’re in the JHA, they must be true.


[65] Quintuple succession of doublings (Rawlins 2002A at fn 14’s conclusion): 304°/4 (1st difference between PH and Trios A and B). Hipparchan calendar. 365°/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).

[66] Tihon op cit. The papyrus’ Solstice-day — 157/6/26 (correct) seriously differs from Hipparchos’ original false indoor-computed S.Solstice (—157/6/28), as reconstructed at Rawlins 1991W §K8, a point precisely resolved when Rawlins 2018U §K5 discovered both [1] the solstice’s hitherto-unknown hour, 18° (missing on the papyrus), and [2] the exact origin of the previously-unaccounted-for remainder of the papyrus’ Tihon-discovered novel tropical-Metonic yearlength, 365°/4 — 19°309. On 2015/4/8, the community was alerted to all this by email to a participant — and to the posting of DIO volume 20, containing the 2015 version of Rawlins 2018U presenting these solutions, plus the 1st formula ever developed for finding solstice observations’ small ordmag-1 systematic errors (from Earth-orbit eccentricity). ibid: eqs.10-13. Not to mention DIO 20’s lead paper, with its important fresh discovery (§31 below) of Archimedes’ 3rd century BC use of degrees. There has been no engagement on any point as yet, except for a somnambulist-referenced JHA paper, Duke 2008W, which (at its pp.293-294) doubts Greek observational accuracy by centrally confusing systematic error with random error, causing misuse by a factor of ordmag 10 (see §F3 here, or Rawlins 2018U §B4). The paper’s author, though unable as usual to find mathematical error in the shunned proposal, nonetheless earns his place on JHA’s board in traditional (fn 116 below) fashion by attacking it, albeit frustratingly reduced to merely non-quantitatively implying that [F]’s three hits (upon the right year, and twice on the right solar motion) must have been just another trio of [D]’s like big & big & big coincidences! (Now do you understand the advantages of dispensing with real refereeing? Another fn 43.) This Duke claim appeared soon after Tihon 2010 was presented at CaTech in 2007, timing which suggests the possibility that the JHAD perceived danger right away: the nightmare of general recognition of the foregoing triple-miraculous papyrus-vindication, of a banishee’s paper which had also satirically-advocated such typically-referenced JHA discoveries as the Velikovskian 366° yearlength by Duke’s JHA co-boardmember Jones (Rawlins 1991W §G9,a DIO 4.1/4, 1994) — so the usual bungled lead-paper discoveries [a][b]. (Tihon has further shown that, c.—c. 150, Hipparchos experimented with previously unknown versions of solar motion, epoch, & precession.)

[67] Reaction to DIO’s vindications has been less than inspirational — but valuably revealing, nonetheless. While still under the influence (fn 59 above) of the late Curtis Wilson, Jones graciously assented to UH; but nowhere has it been acknowledged that (repeating for emphasis) Rawlins 1991W and (fin 278/96) Rawlins 1985H revealed, years in advance of P.Fouda 267A’s surfacing in 2005: [a] Hipparchos’ search for a — 157 solstice; [b] his tables’ use of Kalippos’ way-out-of-date solar motion; [c] ancient solstices were accurate to ordmag 1°.

[68] NB: These DIO-induction-predictions aren’t side-issues. They are central to understanding the 157/6/26 years of Hipparchos’ evolution from amateur-observer-astrologer into an immortal empirical scientist. And subtractors have been uniformly oblivious to a central steel connexion, Revealing his original resort to calculating not observing his earliest, astounding quintuple of geometrically expanding cycles (Rawlins 2002A at fn 14’s conclusion): 304°/4 for Trios A and B, 304°/2 (1st return) for Trios C, 304°/1 (return of Sun); 243°36’ (1st return of Moon); 4868° (integral number of days) — every one successively featuring a fresh characteristic cyclic-return, where each of these includes (like the unrefreshed song, “The 1st Day of Zmas”) all the features of the smaller cycles preceding it in the quint-succession. Check it out: you’ll be fascinated at Hipparchos’ hypothesized cleverness. And (idem) successively halving this, yevers, very nearly, sidereal (152°), Kalendric (144°), etc.

[69] From fitting EH to eclipse-Trio B, DIO mathematically induced (Rawlins 1991W §§K4-K9) in 1991 that Hipparchos’ earliest Sun orbit, PH, [a] a used — 157 summer solstice, [b] adopted Kalippal solar motion, 360°/365°/4 for Trios A and B. Findings [a][b] were both previously unsuspected. But, 14° later, papyrus P.Fouda 267A was examined by A.Tihon (paper 1st presented: Peking 2005) & was found to explicitly verify 1991
observations by that amount on average (while not affecting his solstices), an ordmag higher than his actual 2' random solar-declination single-date rms error (or scatter).

F9 For Almagest 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 Almagest entry. (See fn 61 & 70 here.)

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

F11 He calls the EH orbit “either conclusive nor satisfying” since (emphasis added) “parameters deemed from trio analyses [fn 46 above], are very sensitive to small changes in the input data.” CHANGES?! It appears that orbit-challenged Duke explored resorting input again (as at §D5 above, item [a]) for data-alteration, but STILL couldn’t find alternate orbits [i] which fit all the relevant data of Almagest 4.11 and 5.3&5 — which Rawlins 2012V calls successively Trios A, B, and C — as do the EH—PH (“Frankenstein”), EH, and UH orbits, respectively; AND [ii] whose underlying cardinal points (Vernal & Autumnal Equinoxes and Summer Solstice) uniformly hit upon Hipparchos’ standard 1°4/precision — dawn, noon, evening, midnight — as all 9 cardinal points for EH&PH&UH (not to mention P.Fouad 267A: fn 66 here) conspicuously do. Duke has been publicly challenged (fn 70) to produce his alternate orbits. Nothing has come forth.


G1 The mystery of the superficially-nonsense ancient yearlengths71 found on Vat. gr. 191 fol. 170v and Vat. gr. 381 fol. 163v lays unswolved through decades of fruitless disagreements (fn 87 here). The name of Aristarchos of Samos is written beside two of these yearlengths: \(\tau \xi \varepsilon \delta \kappa' \kappa' \xi' \beta'\) and \(\tau \xi \varepsilon \delta' \varepsilon' \delta', or 365.4' 20' 60' 2' and 365.4' 10' 4'.

G2 Taking the numbers exactly as they stand and allowing signage-flexibility,72 Rawlins in 1980 treated both Aristarchos expressions as continued fractions, and swiftly sent the results to the Journal for the History of Astronomy.73 Listening to Neugebauer’s perceptive perception that6572, Rawlins saw that the 13' expression could be viewed as 365\(\frac{1}{4} + 1'(20 + 2')(260)\) = 365 \(\frac{1}{152}\) of a day, yielding \(\frac{1}{152}\), differing but close to the known (also seriously false) tropical yearlengths of Hipparchos & Ptolemy. The 2nd expression suggested 365\(\frac{1}{4} - 1'(10 + 1')(4)\) = 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 (Great Year) & 152 (2 Kallippic 76’ cycles) between iconic Meton’s and Aristarchos’ S.Solstices, –431 & –279, respectively, are among the EXTREMELY74 few numbers long known to be reliable to Aristarchos, and the difference between the 2 induced yearlengths IS precession, the very discovery traditionally mis-ascribed to Hipparchos. Said difference is close75 to 1720, which presumably later influenced Hipparchos to treat 1’/60 as a lower limit, though Ptolemy eventually adopted 1’/60 exactly (Almagest 7.2-4). Note that Aristarchos is the only astronomer on the Vatican mss’ long-overlooked listing of Aristarchos prior (fn 79 below, & Rawlins 2002A fn 15 & eq.12). Both expressions suggest precession. As the 1st 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 (Almagest 4.2) the 4267 month eclipse cycle for its 126007 place of the 2 induced yearlengths, obviously suggesting precession. As the astronomer we know was a public geomobilist, he is an apt candidate for true discoverer of Earth’s precessional wobble. For two different yearlengths, obviously suggesting precession. As the astronomer we know was a public geomobilist, he is an apt candidate for true discoverer of Earth’s precessional wobble.

G5 We know that Almagest 4.2’s saros of 223M agrees76 to 1 part in 24 million with idev’s deceptively-round-looking saros expression, 18° + 10°/23 or 18° + 45°/60 (where superscript K signifies Kallippic Years of 365\(\frac{1}{4}\) each) — which, times twice 135, so

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71 Metonic “tropical” year: Almagest 3.1; Rawlins 1999; Tthon op cit; origin 1’/4’ 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 §4 & Rawlins 2018U §37.

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

73 The 4267 cycle’s crucially&conveniently trivial inconstancy: 1’/60 quantified by Rawlins 1996C fnn 18&56, on route to verifying it’s the empirical source (Almagest 4.2) of ancient’s discovery of the key relation 251° = 269°. Note revealing Muflitch inversion at Tooher 1984 p.176 n.10.

74 Ibid fn 14 shows that, whatever one’s “sign-choice” for the latter digits of the Vatican mss’ Aristarchos expressions, implied precession will still be near 1°/century, Hipparchos’ lower limit (Rawlins 2018U §55), verified by Tthon op cit.

75 The 4267 cycle’s crucially&conveniently trivial inconstancy: 1’/60 quantified by Rawlins 1996C fnn 18&56, on route to verifying it’s the empirical source (Almagest 4.2) of ancient’s discovery of the key relation 251° = 269°. Note revealing Muflitch inversion at Tooher 1984 p.176 n.10.

76 Sidereal 18.2°&21.01 connects Aristarchos to 1263’ & 2434’, which are 1/3 & 1/2 of 4868’, resp; see fn 79 below, & Rawlins 2002A fn 14-15 & eq.7.

77 Note happy Muflitch inversion at Tooher 1984 p.176 n.10.

78 Ibid fn 14 shows that, whatever one’s “sign-choice” for the latter digits of the Vatican mss’ Aristarchos expressions, implied precession will still be near 1°/century, Hipparchos’ lower limit (Rawlins 2018U §55), verified by Tthon op cit.

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precisely produces day-interval 1778037, Aristarchos’ Kallippic Great Year of 4868—
results which yield:
Discovery #1: The Vatican ms’ Aristarchos-marked year, 365½/4 — 15/4868, is certified
as Aristarchos’ by its denominator’s match to his 4868 Great Year, as already identified
by P.Tannery.
Discovery #2: Merging [A] Aristarchos’ Great Year, [B] his M (jf4 above), & [C] Meton’s
famous lunisolar relation 235° = 19, we next arrive at a vindication for the kind of
exploratory hypothesizing (§31 [f]) that can occasionally move knowledge ahead: 4868
years of 235/M/19 each equals Aristarchos’ Metonic Great Year, 1778022 (15½ less than
his Kallippic 4868½ Great Year). Dividing by 4868 to find the cycle’s yearlength (§38)
produces 365⅓/4 — 15/4868, perfectly matching the figure (Discovery #1) independently found
§(G2) two decades earlier from Vat. gr. 191 fol. 170v: classic predictive success. Nonetheless,
Muffiosi typically refuse to cite (e.g., §G11 below) the confirming evidence. Jones even goes
out of his way to destroy (§G8 below) evidence for Aristarchos’ multi-obvious possession
c.(280 BC) of a monthlength accurate to a fraction of a time-sec, the better part of a century
before Greek-conquered Babylon is known to possess such (c.200 BC) — possibly, just
possibly, because pan-Babylonianists have made a living contending or pretending that the
origin of such wisdom and precision is to be found only in the cuneiform tablets of a plainly
inferior, scientifically-unsophisticated civilization (fn 12; §2 [N13]), which their
cult has become permanently, undeterably in-love with?
G6 We can also merge both cycles found from the Vatican mss (§G2) by noting that
15½ is virtually 1/32 of 4868, which allows us to see [2017/6/6] that Aristarchos’ Sidereal
Great Year is 32ⁿ longer than his Kallippic Great Year, thus 1778069. (Heath 1913
p.315’s reconstructed year, showing Aristarchos toyed with a day-rounded exeligmos, is an
admirably clever revelation, but not sidereal.) The sidereal year must therefore be 365½/4
+ 32/4868 = 365½/4 + 1/(152½+1/8), or about 365½/4 + 1/152 (which of course matches
§(G2)’s Vat. gr. 381 fol. 163v’s yearlength). [Since 15½ + 32 = 47, we see (2017/12/27):
by Aristarchos’ Great Year scheme, precession is 47½/4868, 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-vindications 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
undercut a paper that doesn’t, and how to trash into chaos coherently-knotted emendations
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 nimblly sidewinds to a sly Gordian tactic: erase all accents on the mss (the cripple—
triple ploy of fn 88), which automatically, deliberately wrecks the data-basis for Rawlins 1999’s
refined, precise inductive journey from Vatican ms to [1] ancient science’s ingeniously
constructed (Rawlins 2002A §A) & modern inductive’s 4-way (fn 88) reconstructed
4868 Great Year [itself from superaccurate M; §§G4-G5, www.dioi.org/jb11.pdf, eqs.5-7],
& [2] Aristarchos’ sidereal year 365½/4 + 1/152. How does such holy warfare differ from
the kites? [Query: en-passant: would ancient astronomy historians accept NYU-In
StudAncWorld Director Jones’ deleting all accents from the mss of the Almagest?] Like-Talibanish is his authoritative justification for across-the-board wipeout: decreeing
accents on Greek mathematical data are destroyably worthless in HIS expert judgement.

83 See Moesgaard 1983 or Neugebauer 1975 pp.354f.
85 Dozens of similar DIO vindications are collected at www.dioi.org/vin.htm.
86 DIO’s exact confirmatory hits here are akin to R.Newton’s also-years-later fruitful success: §E3.
II Pliny's Circuli: Deft Ancient-Trig Approximation-Inventiveness

II1 Now for a case of ancient data-tampering that actually happened! — the very sort which modern-chauvinist tamperers have, with dedicated persistence & Creativity, tried misprojecting\(^89\) onto valid data (above §§C-G) that commit the crime of being incompetent to prevailing Mufa preconception.

II2 Neugebauer classified the seven “circuli” of Pliny 6.39.211-218 as a primitive “arithmetical\(^96\)” scheme, deeming their superficial inaccuracy to be supportive of his contra-reality (fn 8 above) insistence on “the absence of any scientific organization in antiquity”, a conviction which he thought helped (along with his reasoning as covered in fn 1 here) to exonerate accuracy-challenged Ptolemy. But the 1\(^{st}\) — and still only available — coherent solution (fn 91 below) ever devised for the circuli indicated they are no more arithmetical than Hipparchos-Strabo’s klimata (which Neugebauer deprecated similarly: fn 18 above), proposing that Pliny’s circuli are instead trigonometric (as are Hipparchos’ klimata) and a case where it is reasonable to test\(^95\) for possible ancient data-alteration, since all seven circuli are clearly a version of the traditional Seven Klimata.

II3 Fit-probes upon them initially produce ridiculous, yet trending obliquities. Experimentally shifting all circuli \(M\) by the same simple round constant,\(^1\) (or \(4\))\(^9\), finds gratifying consistency with the same accurate Hipparchian 23\(^{\circ}\)/2/3 obliquity discovered by Diller and insubstantially & cementally rejected by Neugebauer\&Jones (fn 18&25, resp).

II4 Rawlins’ 1984 paper (invited for the Greenwich Meridian centenary Longitude Zero Symposium), featuring its 1\(^{st}\)-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 (§§5-10 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 even tabulate them (fn 34 above). Jones mentions Pliny’s circuli but simply calls them “crude”\(^96\) (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 tell readers that these restored Pliny data are consistent with (fn 92 here) the very same accurate Diller-discovered 23\(^{\circ}\)/2/3 Hipparchian obliquity Jones is busy trying to replace (§C6) elsewhere in the same paper, a paper which qualifies as a pinnacle of JHAD-subtractivity, simultaneously managing to deny Greek accuracy on all available fronts — obliquity & klimata & calculations & trig-tables & circuli (\& Diller & DIO) — thereby obediently according with predecessors’ orthodoxy (just-as-reliably as did just-as-careerist Ptolemy’s “observations”: fn 8), ensuring Jones’ rapid elevation to JHA boardship.

II5 Our next chapter presents dozens of new perceptions, all rigorously JHAD-uncited, possibly due to the history-of-astronomy cartel’s relative ranking (§J1) of cult-discipline vs knowledge-advancement.

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

I1 Though numerous scholars have doubted 3\(^{rd}\) century BC Greek scientists’ adoption of Babylon’s degree-division of the circle, it was unexpectedly revealed\(^94\) in 2012 that Archimedes’ original unprocessed measure of the Sun expressed its diameter sexagesimally as in the range between 27° 33’ 0” and 27° 33’ 10”, later conventionally published in his Sandreckoner as rightangle200 and rightangle164. Hard to believe (given the ultra-simplicity of the math): no one has noticed this for the last 2000\(^2\).

I2 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 farcical\(^5\) fundamental empirical data (2\(^{rd}\) solar&lunar diameters!), & in spite of its ludicrously contra-reality requirements that: [a] total lunar eclipses last 12\(^{th}\) (Neugebauer 1975 p.642, \& ibid) 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 Bizarrty-Hall-of-Infame, by the astonishing fact that, again: no one has noticed this for the last 2000\(^1\).

I3 In the 27\(^{th}\) since publication (Rawlins 1991P fn 6), no modern scholar has yet cited DIO’s discovery of the elementary cause of §J2’s 4-factor-oversize: pseudo-Aristarchos misinterpreted Aristarchos’ lunisolar diameter of 1/15\(^{th}\) of a “part” or \(\mu\rho\varsigma\) of the zodiac as: 1/15\(^{th}\) of a zodiacal sign. But \(\mu\rho\varsigma\) (meros) was just an ancient unit \(=\) circle/48 = 7° 1/2, of which 1/15\(^{th}\) exactly agrees with Archimedes’ correct half-degree testimony.

I4 Aristarchos’ famous 87\(^{rd}\) half-Moon elongation wasn’t empirically a precise figure, in moderns (and seemingly Hipparchos: §D2 above) assume, but a lower-limit.

I5 From tiny solar declination-motion near solstices, unusually sneering,\(^96\) amateur-astronomer-Pтолемists (incl. MacGenius Swerdlow: \(\dagger\) in 8) doubt ancient solstices could


\(^94\) Sizex’s 5 farces & Aristarchos’ 1/2-Moon-elongation 87° lower limit: www.dioi.org/jne02.pdf, Rawlins 2008FR §§C1-C3 & fn 17k&29. [By proportions, Aristarchos-Archimedes’ shift (Rawlins 2008FR fn 37 vs eq.13) from Sun-distance 1000 to 10000 altered limit to 87°’7 (near actual 89°.85).]

\(^95\) For “part” (\(\mu\rho\varsigma\) or meros) = circle/48; see Neugebauer 1975 pp.652&671, or Rawlins 1991P fn 6.

\(^96\) Those wondering whether Swerdlow&Gingerich are primarily scientists or careerists, cannot miss their amusingly indiscriminate alibis for Ptolemy’s fakes, at www.dioi.org/jk02.pdf. Rawlins 2018U §§B2&B3 and fn 2k&3. (And Duke at fn 69 above, vs Rawlins op cit eqs.5&21 Table 3, \& §EJ: Alleged solstice-inferiority: fn 27 here, Evans 1998 p.206, \& Swerdlow 1979 [Phi Beta Kappa] p.527. Noel Swerdlow, though occasional valued discoverer [e.g., fn 42], is a prime contributor to DINO-J.HA “anomaly luck” (see §4J1B 1 on Swerdlow’s \& Evans’ innocence of equal-altitudes, the obvious ancient solstice-finding method, there are unambiguous historical points they’re equally (and, as purported historians, less excusably) unaware of (unlike non-politicians such as Toomer 1984 p.12 \& Britton): all known ancient scientists found yearlengths via solstices not equinoxes: Meton, Euktemon, Callippos, Dionysios, Aristarchos, Hipparchos, BM55555. And these doubters of ancients’ solstice-accuracy (plus fn 27: Jones) are now confronted by recently recovered papyrus F.Poul 267A, testifying to a Hipparchos solstice accurate to ordm 1\(^{\dagger}\) (\(\dagger\) fn 20[C]). Prefererring solstices over equinoxes for year-length-determination becomes...
be good to ordmag 1, which invites equinox-vs-solstice accuracy-comparison (§27).  
I6 Rawlins 1991H found the Babylonian yearlength on famous Astronomical Cuneiform Text 210 was 365.25 days based on known Greek solstices (~365.2527 & ~134.626), the 1st datable transmission of an orbit-element between Babylonian & Greek astronomy, going Greece→Babylon, thereby cutting the Mufa’s holiest tenet (§373&fn 120).  
I7 Aristarchos & Archimedes put minimum solar & stellar distances at 10,000 & 10,000 AU, resp, because humans can see (as no historian-of-science seems aware) to c.1/10,000 raw. Rawlins 2017E §2: “For advancing history of science, knowing science matters.”  
I8 Ptolemy’s allegedly-outdoor 4 solar “observations” (132–140 AD: Almajest 3.1&7) averaged over 1 error (not even 1 shot encroached anywhere on the solar disk) and were fifty times nearer Hipparchos’ 3-century-old indoor tables than to outdoor reality (as known for 1200 BC: F.Ragen 2010 p. 121): undeniable but also (for over 30000 J.HA-unpublishable 56) despite null ancient attestation of their speculation. Ptolemites insist (fn & 127) this is because ancient scientists kept only observations agreeing with theory. (I.e., they cheated.)  
A1 How could Ptolemy’s solar “data”, all off by a degree happen in the 1st place, outdoors?  
B How did it happen that Hipparchos’ Sun observations’ average disagreement with reality were merely two or three times their disagreement with his tables, vs Ptolemy’s fifty?  
C How could Greeks just copying predecessors achieve their many accurate discoveries? (E.g., Greek vs real ancient monthlengths, Rawlins 2017E §§B2-B4: startling proximity.) However, most scholars (A.Pannekoek, R.Newton, Y.Maeyama, J.Brandt, & P.Zimmer among the welcome exceptions) reject ancient high scientific accuracy (fn 1&35 here), undeterred 2016 scholar D.Shcheglov even (fn 13 here) calling it a “delusion.”  
I9 Aristarchos discovered precession 1500 before Hipparchos: §G3.  
I10 Rawlins’ order-of-magnitude estimates of two-unheard-of Greek accuracy (§1: §F): 1’ for lunisolar diameter; 1” for moon distance; 1” for solstices; 1’ for equinax taken on Alexandria’s ring (fn 70 here: Almajest 3.1); 10’ for sidereal year (fn 114 here); 1’ for observer latitude (see fnn 39&100 here and §4’s Table 1 & fn 38): 1’ for 500-mile North-South arcs on earth (fn 111); 0’.1 for star declinations (fn 100): 0’.1 for lunar-limb-vs-Sun gap (fn 12 here); 0’.1, even 1’ for star-vs-eclipsed-Moon gap (§B7); ocular error 1’±1’ (fn 97 here) for Hipparchos’ mean equinox, with 2’ scatter (fn 12); 1” for Earth-circumference-measure precision (fn 108); 10” for lunar eclipse-prediction (fn 97 here); 1” for lunar eclipse observation (Rawlins 1985G pp.258&265); 1’/century for mean motion of Mars (and maybe Venus); 1” for all three adopted months (synodic, anomalistic, eclipse), each correct to better than one part in a million. Historians-of-science like Scheglov show no sign of awareness of any of these Greek achievements.

obvious from, for instance, Britton 1967 p.29. More generally, §8’s key Obvious Question C jokifies fn 8’s Swerdlov-Gingerich-Scientific-American’s Ptolemy-exculpation theory.

57 On 1982/7/17, ever-Ptolemy-worshipping J.HA, unable to argue facts or math, nonetheless belatedly excised Ptolemy’s sensational 50-to-1 indoor-vs-outdoor ratio (f8) from a projected Rawlins paper (fn 74 here; precensored text at Rawlins 1999[E], along with nonselector-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 imperferct solar data. His UH tables’ eclipse-prediction accuracy was ordmag 10m: Rawlins 1991H eq.32.) The 7’ mean error of Hipparchos’ equinoxes is mostly not due to eyeball inaccuracy: when one removes the effects of his presumed solar parallax and (like fn 70 above) the effects from refraction in the Sun’s zenith distance, and his instrument’s setting-tilt from polestar-refraction, there’s only 1’ (±1) unaccountable-for: see Rawlins 2018U §B4. Mars (and Venus) mean motion accuracy c.1”/century: Rawlins 2002V fn 26. Contrast to 1 here, and to the umpteenth fruitless metrological analysis of Eratosthenes’ Earth-circumference, Scheglov 2016, www.dio.org/sch.pdf, which massively contends that high-accuracy Greek longitudes are mythic, his entire assault upfront-promoted by History of science Society (and post-protected by HSs stillwell-nonnunciation), ultimately undone by his crudely confusing a solar eclipse 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 dufus-science: details of these latest Isis disasters can be found here at §6, DIO’s Letter-to-Isis’ hiding-since-receipt Editor H.F.Cohen.

11 At the 1984 Greenwich Centenary, DR presented evidence countering Neugebauer’s denial (above: fn 90, vs fn 13) of the existence of organized science in antiquity, by 3/4-unstretching the longitudes of the naïve 4/3-stretched map of Ptolemy’s 160 AD GD, the Geographical Directory (often called just Geography or Geography), finding that Greek mean longitude error was well under 1”, thus indicating that ancient scientists had cooperated in competing (as recommended by Hipparchos: Scheglov 2016 n.7) local times of lunar eclipses at even far-distant sites (fn 105). E.g., the unstretched 42” longitude-difference between Carthage and Persepolis is correct to ordmag 1%. NB: The distorted remains of accurate ancient geography in Ptolemy’s GD were ruined in two widely separated stages: (but sharing an astrologer-source: in each case):
[1] Latitudes were semi-randomly wrecked by Hipparchos (contra Rawlins 2009S fn 18): his discrete tabulation of them (GD 1.2.4) for professional reasons. (See sources here at fn 13 for details; Hipparchos was publishing tables [Tihon 2010’s valued direct revelation] which served 3 astrological traditions simultaneously, sidereal (or anomalistic), Kalippic, & Mentonic, looking as professionally gain-oriented as today’s astrology, whose practitioners yet cater to 2 of the 3.) [2] Longitudes were systematically ruined by isolated astrologer Ptolemy’s ignorant stretch of correct eclipse-based longitudes on 7/5 or 4/3 (§1 §F&K&M).


13 For decades, historians-of-science argued (e.g., fn 18) against Hipparchos’ possession of spherical trigonometry, a position now indefensibly obsolete: for a pioneering session of spherical trigonometry, a position now indefensibly obsolete: for a pioneering

14 Rawlins long insisted that the Almajest Mars mean motion was based on ratio 152145/329621. Inspired by Duke’s skepticism, Jones in 2002 discovered it was based on a different cyclic ratio that Rawlins had carelessly neglected, residing, ironically, in the Almajest itself. In the same article, Rawlins gave a similar explanation of Almajest Jupiter mean motion, equally false historically, reflecting even less-excusable overconfidence. The correct cyclic Jupiter solution was simultaneously realized by Jones and Duke. Rawlins faxed double-congratulations to Jones the hour he heard of the nds, announcing them in DIO’s Letter-to-Isis’ Ptolemy-exculpation-thesis. 

15 Generalizing from the bases of mean motions of the planets, the Moon, (partially) the Sun, DIO created a General Theory of Ancients’ Cyclicity, proposing it was preferred ancient method to found mean celestial motions upon empirical integral-return rates. 

16 It’s long been assumed that Hipparchos rounded the time of dawn or evening to the 1/4-day, even near solstices. But, in 2015, DIO showed that Tihon’s papyrus is consistent with Hipparchos using exact time for each when gauging yearlength, thus revealing (fn 66) the origin of his hitherto-unexplained 365½/14 – 1½/309, which Tihon was 1st to reveal.

17 In 2002 K.Pickering discovered that original locations of dozens of erroneously placed stars in the Almajest 7.5-8.1 catalogue can be reconstructed (occasionally via spherical trigonometry), but it was repeatedly found that repairs could only work with coordinates from Hipparchos’ era, not Ptolemy’s; sixteen years later, Ptolemites have yet to produce a collection of such reconstructions based upon the opposite theory, leaving the independent implication that Hipparchos’ catalogue was observed by Hipparchos, later plagiarized by Ptolemy.


95 Pickering 2002C.
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, Aristyllus, Hipparchos, Ptolemy’s anonymous observer — through bivariate least-squares testing upon these observers’ star-declination data.100 In 1994, ex-

100 Rawlins 1994L §§F3–F8, 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. Mayerma 1984 finds thusly, 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, noncalculus trial-and-error, and graphs — gauging $E$’s standard-deviation by eyeballing, and $x$’s by confusing it with that for a single datum. More recently, in the mathematically-challenged Journal of Astronomical History & Heritage (JAHH), Brandt et al 2014B, www.dio.org/bjpdf, says most of Rawlins’ geographical latitudes of $E$ (within 0.5° of our values). But their $L$ are merely assumed, so DIO’s JAHH-requested 2014/8/26 referee report, www.dioio/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. to the book’s standard-deviation (computed as in www.dio.org/bjpdf, p.5) to find $L$ a more accurate $L$. Also and more importantly unmet was the DIO referee’s urging the Brandt et al 2014B paper to note that Ptolemy arithmetically reduced (Almajest 5.12-13) his transit “observations” to declinations using a seriously false Alexandria latitude $L$, from Vitruvius 9.7.1 (plagiarism unmentioned at Swerdlow 2010 p.151), $L = 30°58'\! (\sec \tan \! 3.5; \! above, \! in \! fn \! 25)$, erroneous by $-14'$, which obviously is much-too-large for a regular outdoor observer, and also is in $17'$ conflict with latitude $L = 31°15'\!$ which is indicated (by nulls) to be the virtually-correct value that was adopted by the stars’ actual observer. Brandt et al 2014B’s standard deviations $\sigma$ were allegedly calculated bivariately; but, actually, after each $E$’s was found monovariately, $L$’s “accuracy” was found by averaging the remaining residuals, original but invalid procedure, making $L$’s uncertainty too small by an ordmage, as referee-specified. NB: Had Brandt calculated $E$’s first, finding $L$’s $\sigma$ first, and then, finding $L$’s $\sigma$ first, leading him to get the other variable’s ($E$’s) “accuracy” similarly, the result would have been informationless zero! (See §§C11.) There are further oddities. Ibid eq.1 sets $O-C$ equal to $C$. The paper’s $O-C$ graphs are rendered C–O. Two strange JAHH attempts to undercut skepticism by splitting Ptolemy’s 18 stars differently from R.Newton (and, by-the-way: Ptolemy and Pannekoek and DR — 2015 8° from Table 3) — only those low-southern declination-stars (with $\delta < L$) to test the declination data thus recovered, to learn which $L$ produces (in implicit zenith-distances) the dramatic. R.Newton excessive of 00° endings expected104 for raw ancient instrumental observations. That $L (35°50')$ suggested the southern stars’ observer worked on Rhodes Island’s south tip, Cape Prassonesi (altitude over 200m), which rules out frequently assumed $L = 36°4.4$ (Rhodos City) and barely conflicts with commonly rounded $36°$, while consistent with above $36°8'$, indicating Hipparchos’ permanently fixed instruments were near Rhodos Island’s Lindsos ($36°05'$).
ordmag 1° accuracy, before Ptolemy's fateful stades/degree scale-shift\textsuperscript{(109) (§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 (variably rounded) stades-values, 25000 or 25000, but instead was 256000. In 2008, 26 later, all 3 were checked\textsuperscript{(106) against Eusebius' long-neglected Eratosthenian Earth-radius \( r = 40800 \) stades, and 256000 was the only one of the 3 that fit this \( r \). (How will Carman & Evans 2015's authors explain not mentioning this match, to \( I \) part in a thousand, when their own cited sources show they knew of 256000? See fn 10 above.)

I23 It was simultaneously found that Earth-radius \( r \) was the empirically primary datum, consistent with being based on Sostratos'-non-spherical Phaenon method (§24), which directly\textsuperscript{(107) finds \( r \) from his Pharsal Island lighthouse (in Alexandria harbor), built near Sostratos-Eratosthenes' time and place. Again, our deliberately silent ([§22] & [126] JHAD-center) -- which accepts no discovery if not from its own network -- must regard the temporal/spatial coincidences as completely without significance. (Like unrecognized confluence in another sphere: www.dioi.org/shg.pdf, "Kit Marlowe's Perfect Crime" §[G.3.)

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

I25 Taking the stade's length to be the generally-accepted value, 185 meters, Sostratos-Eratosthenes' \( r = 40800 \) stades is 19% or about 6/5 too high, while Poseidonios' \( C \) = 180000 stades, the other anciently standard Earth-size, is exactly 5/6 too low. It is an Occam-DIO dream-come-true to perceive that since horizontal light rays' curvature = 1/6 Earth's, the atmospheric refraction would cause observed errors in \( C \) of 6/5 and 5/6, respectively, for two simple, clever, low-physical-law never-leave-home methods\textsuperscript{(108) of measuring the Earth: the Pharaoh-flame method (6/5) virtually attested by Pliny 2.65.164; and the also-physisy-physical & (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

\textsuperscript{109} Rawlins 1985G p.265, taking an idea due to (ibid n.22) Gosselin 1790, suggests an ancient, adjusting for the 700 stades/10° - 500 stades/5° switchover, stretched longitudes by 7/5, mis-assuming they were based on land-surveys ([1 §F]). Or by 4/3 via Poseidonios' \( C = 240000 \) stades—180000 stades. Proposing Greeks organized to compare eclipse observations (fn 13): Rawlins loc cit, vs fn 90 above & cons. in C. Marlowe's spy novel, "The Virgin's Men", westernmost known land, chosen as Marinos' \( C \) longitude (www.dioi.org/[501].pdf, §[A5]), to kill longitude sign-muffs like those (fn 97) cursing Scheglov 2016, [Speculation 2018/7/15. Marinos = pseudonym, like "Ptolemy? Or map-title from maritime Tyre?]

\textsuperscript{110} C = 256000 stades \( C \) induced from a Nile map's latitude intervals: Rawlins 1982n pp.212, 214, 216-217; Rawlins 1985G p.229; Thurston 2002s p.66. For which \( C \) fits Eusebius' \( r = 40800 \) stades: just multiply by \( r \) twice: \( C \) = Rawlins 2008Q eqs.8-11&18, esp. eq.11's solar distance = 100.1 AU. FN 10's \( 112 \) AU is overexct (fn 42), even without the 252000 less than 100 AU fits 256000 fits.

\textsuperscript{111} Rawlins 2008Q eqs.12&21&28, eqs.14-15&17-18 for \( r \) as Sostratos' direct empirical measure. See \textit{ibid} §[A4] [a] for his Pharaoh-flame method and Pliny's semi-attestation of it. Double-sunset method: Rawlins 1979 \textit{or Scientific American} 1979 May. Interval between times of sunsets seen from Pharaoh's top-bottom exceeded a time-minute, unmissably-enormous alert and gauge of \( C \) size. (Elementary illustration-by-extensions that different results ensue for flame vs sunsets: Rawlins 1979 V §[A5]). To show needing direct atmospheric refraction matching the sun's (even if not a invisible) fit here, the foregoing utterly original & successful atmospheric-refraction theory -- tri-neatly solving the INTERMINABLY-contended ancient Earthsize mystery — cannot legitimately be ignored. But it is: fn 111 below. Unbelievably worse: fn 101! One recalls not only JHAD shunning of Diller (fn 25), but the case of L. Boltzmann's kinetic theory of gases, which E.Mach & others spurned because (though theory neatly fits evidence) \textit{no one had ever seen a molecule}. Did this trigger Boltzmann's 1906 suicide (just ere vindication by Wilson-cloudchamber) We don't know. What we know is: certain pods cited hereabouts wouldn't care. Past perhaps praying for history to repeat. Ever so vainly.

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Possessive&Geographical Directory's \( C \) (5/6 low), AND the 185 meter stade: triple-vindication for this neat-fit & totally fresh DIO idea. I.e., shockingly, Greeks' 40%-disparate \( C \) is EACH solved to 1% by the same airbend theory & the same unfudged stade.

I26 Though the atmospheric-refraction solution, explaining erroneous \( C \), has been repeatedly published (fn 108 in the scientific literature — and featured with generous credit to the author) for years in the 1990s as opening-page demonstration of using physics, in the long-standard physics textbook Halliday, Resnick, & Walker — historians-of-science either [a] cannot follow the math or [b] regard it as mere scientists' intrusion onto grounds best adjudicated by the real experts. Thus, the reaction, decade after decade is: no reaction at all in JHA and fellow captive journals, which doesn't help communal awareness\textsuperscript{(111) of the theory's existence, to repeat) produces only the 185m stade. F.Ragep 2010 (p.124) and two recent (2015&2016) Isis lead articles — all on closely related ancient geographical subjects — cited Rawlins papers which explained the atmospheric-refraction theory, yet each of the three [Isis] authors, in concert with all their colleagues for decades, refused (see also fn 111 here) to mention the theory's very existence, even when it was right before their noses (details here at §[W]&X). Do ordinary readers of history-of-astronomy journals deserve the opportunity to make up their own minds on the airbend theory's plausibility? HsS archons exhibit zero concern at the spectacle of assertively-totalitarian shutdown of their own people's access to a theory so solid that (to emphasize by some repetition) it has appeared in the American Journal of


\textsuperscript{111} Possessive&Geographical Directory's \( C \) (5/6 low), AND the 185 meter stade: triple-vindication for this neat-fit & totally fresh DIO idea. I.e., shockingly, Greeks' 40%-disparate \( C \) is EACH solved to 1% by the same airbend theory & the same unfudged stade.
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 archonial cynosurae realize that the 6/5 factor has been standard among selectively, anachronistically used by Eratosthenes’ mod-groupies to rig right-on correctness for his too-big C? In 2014, it was seen for the 1st time that the much-attested (fn 111) early 3rd century BC Greek rule of dividing terrestrial meridians into 60 parts (not 360) — step-one of C’s potential *sexagesimalization*, *conventional Greek fraction-practice* — could’ve led naturally to the Ptolemaic empire’s regularization of the “stade” by defining it, parallel to our definitions of meter and nautical mile (fn 111), as C/60/60/60 = 400000000m/216000 = 185m. This is the best — the only — available scientific theory explaining modern-consensus-185m’s Greek origin & durable adoption, which survived even influential Eratosthenes’ soon-after insistence on a 19%-larger C.

**Question #3:** Would it matter?

Hypothesis-discoverer Rawlins’ own 1996 case for re-evaluation (emphasises in original) follows. *DIO*’s new PHYSICAL — not standard kneejerk-meterological — theory (ascribing both ancient [Earth-C] values’ error to [atmospheric] refractive) simultaneously solves . . . both the (very discrepant) Eratosthenes & Poseidonius donoms values . . . through a single value for the stade: the same . . . 185m value . . . found even in most dictionaries.) No other simple, coherent theory does so. [Classic Eratosthenian stade-scruncher J.Dutka][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 ever be rational discussion here when the only theory that fits all 3 data (both C, as well as the standard 185m stade) is not even understood by those who keep prominently churning out forced metrological retreats (as recently as late 2016! — in 9792), none of which can fit more than one of the 3 desiderata; and even that simple 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.

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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 [J2]! The *ad hoc* nature of the digitally mythic runty “Eratosthan” stade is obvious to most specialists, e.g., P.Gosselin, E.Bunbury, D.Dicks, O.Neugebauer, H.Bocher, J.Berggren, A.Jones (more at [J2]). (Who creditably did not jump indiscreetly at a poor solution, but waited for a valid one to come along. So far so good. But now that *DIO*’s airbed theory is here, no historian-of-science is claiming the math doesn’t work. Or that anything works better. Has the-catagonia got the JHAD-tongue?)

The dwarf-stade myth is efficiently, consistently, bluntly, and utterly evaporated by Engels 1985 p.309. Sexagesimally-defined stade: Rawlins 2012T fn 2; self-contradicting the titillating contention of Rawlins 2008Q & note ibid [J4][a] that early-Ptolemaic survey-based Earth-circumference determination was just legend. This can be seen as showing DR’s poor judgement. Or desire to learn. Or both.

111 Dutka 1993 p.64 cites Rawlins 1982N — whose App.A explicitly links 6/5 to lighthouse and 5/6 to sunsets — without ([J2] 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-curve is apt to a meridian circle of circumference 398700000m) 5000 stades Alexandria-Aswan & Aswan-Meroé each good to ordmag 1/4 for 700 stades/degree; 7°/18 + 7°/18 = 14°/14. (Rawlins 2009S C notes Philo’s solar work at Meroé, presumably for an imperial survey.) Testimony for early-Ptolemaic meridians in 60ths: Strabo 2.5.7 (Eratosthenes); also Geminos, etc.: Neugebauer 1975 pp.590 (n.2), 735, & 1364 (Fig.43). Is a Ptolemy I survey’s memory embedded in Kleomedes 1.10’s famous legend? (Rawlins 2009Q §A4[a].) Dismoor 1950 pp.230-251, cites 5 ordmag-10% discrepant Greek stadion’s stadion-long Greek race-courses. (Shchedrovina 1996 p.436 notes too more — even more.) The only post-Ptolemy-I course is obviously also the 185m one. The ancient stade was 1/8 of a Roman mile (1480m): Engels 1985 p.308. Updated compendium of ancient Earth C-values, in stades: Aristotle 400000, Dikaearchos? 300000, Timocharis? 216000, Sostratos- Eratosthenes 256000, and Poseidonios-GD 180000. Correct circumference C = 216000. (Meter = C/10/10/10/10/10/10/10/10/10/10/10/10.) Nautical mile = 1852m = C/72/60/60 = C/72/60/60 = 5/8 of the Roman mile. Further speculation on the pharmonic stade’s history is found at www.dioi.org/40.htm#kchg. Relating attested meridian-60ths to 185m is another JHAD-uncertified completely original *DIO* revelation.

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113 Yearlength 365 1/4: Rawlins 1985H. Kallippos solstice-hour-epoch: *ibid.*, Rawlins 2018U eq.2. [Wikiopedia’s Callippus entry falsely implies Meton-Kallippos’ 1st difference relates to precession.]
Sourcing Ptolemy’s final lunisolar ratio, \(105416^\circ = 8523\circ\), occurred 2 decades ago (all 10[!] digits exactly elicited) by test-exploring Greek awareness of the 800° sidereal
eclipse-cycle (1/5 of 800° cycle attested: Geminus 8.40–41): solution, awareness, &
rest not suspected ere Rawlins 1996E eq.31. (Sidereal year accuracy: \(\text{ibid Fn 110}\).)
Royal Muftia Cavilliers have produced no math error or alternate solution since. Predictable result

More muteness greeted DIO’s 2002-2003 discovery that all 3 previously unsolved,
anciently adopted mean motions of the Moon (1. System A; 2. draconitic; 3. Ptolemy’s
last lunar equation)^114 were exactly consistent with discovery by ancient scientists who
merely divided an eclipse cycle ratio by whatever integer or half-integer was common to
both the ratio’s terms.

\[\frac{\text{Solve: Rawlins 1996C eqs.20–31. Thanks to K.Moesgaard for a perceptive correction.}}\]

No observed solstices. No vertical instruments. No knowledge of Babylon’s latitude (\(\text{ibid Fn 73}\)). Hmmm. Do pan-Babylonians never-wonder just why; Babylon had no Aristarchos?
No Archimedes? No Apollonios? No even a Seleukid Euclid? No trigonometry. No transit data.
No observed solstices. No vertical instruments. No knowledge of Babylon’s latitude (\(\text{ibid Fn 73}\)).
No serious astronomical work until after his death. No Autolycus (\(\text{ibid Fn 73}\)).

\[\text{No other method than eclipse-period ratios method, clearly attested (§134) for finding Greek monthlengths from eclipse-pairs separated by 3 1/2 or 6 centuries, or presumably and fittingly (§136) 10, 11, even 13 centuries.}\]

\[\text{See www.dioi.org/thr.htm#cpvc, and Britton 1999, for his theory of System A-year origin, and www.dioi.org/j129.pdf, for his clique-puzzle for Rawlin}^{2002B}'s simple exact halfing an integral eclipse period-relation! P.Huber dreams that ancient merged non-integral short-period relations to create ordmag 1000 integral ones (contrary to obvious common-sense, as well as ancient sidereal-vs-synodic records: \(\frac{12^\circ}{115}\), like Neugebauer 1975 reading into 3rd century AD AD containment 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 1000th rational number within a supposed accuracy of 0.001 of dividing advantage of (from dividing advantage of a very huge integer), known to every astronomer who ever gauged celestial periods (from Mars to pulsars) in the real scientific world. 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.


No serious astronomical work until after his death. No Autolycus (\(\text{ibid §§113}\)).

\[\text{For this recent shock, see www.dioi.org/thr.htm#rbkv. Re Hipparchos’ 600° lunisolar tables, see Pliny 2.9.3.}\]

114 PianHyp 1.1.6 (Heiberg 1907 pp.78-79 or Neugebauer 1975 p.901 eq.3); 105416° = 8523°. Solved: Rawlins 1996E eqs.20-31. Thanks to K.Moesgaard for a perceptive correction.

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

116 Moesgaard 1992 p.474. Initial Muftia tactic vs R.Newton & DIO was non-citation. But Isis Editor Margaret Rossiter’s publishing DIO-respecting Thurston 2002S defied the 30° shun, inspiring (what else from pathologically unregenerates?) DOUBLEBLOshun: [a] Thurston’s swift ever-exile from JHA (www.dioi.org/pm3.htm); [b] DIO-citations’ end in AAS-HAD’s Newsletter & [c] Isis’ Cumulative Bibliography; [d] during Thurston 2002S’s refereeing (2000), the usual indiscriminate (fn 66) unreferenced anti-bodies prepared for launch. Schafer 2001 (Pb), Schafer 2002, Jones 2002E (2nd to Pb), Duke 2005T; Duke 2005W (Pb), Jones 2010B (2nd to Pb), as pols outdid each other (to squush #1 blackballee no matter how: §§R-B), all now on JHA’s certified-Premier (fn 42) board. Re JHA’s prior villain, we quote from fn 28: “Rewards handed out to those who attacked the R.Newton satan include JHA boardship (R.Newton 1991 fn 2) & a MacArthur for miss-man [fn 96 here] Swedrow. (It’s hard to find good help anymore.) . . . . maid-men Evans & Schafer were elevated at JHA not long after their massive bungled 1998&2001-2002 attacks on Rawlins. (The unsubtly here may actually be deliberate.) Selecting boardmembers [thushly] will damage mean-IQ atop JHA for decades to come.”

116 For this recent shock, see www.dioi.org/thr.htm#rbkv. Re Hipparchos’ 600° tables, see Pliny 2.9.3.
among almost-as-accurate proximate ratios (www.dioi.org/thr.htm#epcc), or [d] explain why each solution emerges as a ratio, a glaring feature of ALL pre-Ptolemy adopted lunar motions, which by contrast to orthodoxy is accounted for perfectly inevitably by the eclipse-period-ratio solution. Jones’ blinded private rejection225 (by subsequently-undercut226 reasoning) of the DIO, aucton sollicitus, flies all of the overrun-numerous, solid, unambiguous evidences consistent with said theory, particularly its PRECISE match to Hipparchos’ draticonic ratio 5458s = 5923s ratio (Almajest 4.2) by pairing an early apogee eclipse, −1244/11/13, with his Rhodos-observed −140/11/27 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 draticonic purpose, with inferior result—inevitably, due to shorter timeframe. Comments: In all history, no astronomer before Hipparchos ever used an apogee-perigee eclipse-pair. Scoffins at the theory’s outrageousness-vs-orthodox-preconception inadvertently compliment it by reflecting its potential advance’s enormity.

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-mindedness & tolerance (Hoskin, Evans, Jones, and Toomer have filed contact with Rawlins for years, e.g., Thurston 1998D 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 outweigh (?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 maximize minimally preferably 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., fn 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 Ockam prefer?227

225 Half-dozen evidences for Hipparchos as author of 5458s = 5923s: Rawlins 2002H §C.
226 Neugebauerians long taught that 6 cuneiform-tablets’ lunar calculations for c. 200 proved chronologically Hipparchos (c. 130) took his draticonic 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 123’s flock of coherent evidences, but also rejects an unexpected key openning spinoff-benefit: recommending responsively increased caution when time-placing non-clay-dated astronomical cuneiform tablets: ibid §D1.

weighing of competing theories by such criteria as simplicity (§31 above), probability’s relation to confirmation (§2 [N15], minimal hypotheses (§D & fn 122), fruitfulness (§C3b), E, & F), predictivity (§F & G). Instead of by herd-grantmanship. J2 What simple, Occamly-coherent theory explains the serpentine communal behavior detailed here? — targeted non-citation, desperately indiscriminate “alternate” solutions to demeand solid achievement, dishonest smearing, data fudgery and even destruction? Answer: shunning228 (aimed at anyone upsetting archons’ tenets or pretensions) — the single spare hypothesis that ties together all of the foregoing superficially mysterious, deeply inexcusable offenses against ethical scholarship. No use denying it: jihadd-shunning of Diller, Newton, Rawlins, and DIO is common knowledge throughout the JHA circle. (Can’t blame on Rawlins’ acknowledged shortcomings, for sedate gentlemen Diller, Newton, & Thurston [with fn 66] were shunned from 1934, 1948, & 1978, resp. most of the years before Rawlins barged into The Ptolemy Controversy in 1976. Even highly expert Britton felt he’d been somewhat exiled, as he imparted to Duke, for honestly owning that Ptolemy’s solar errors were not innocent: fn 122.) But, given the above consistently weird incidents, one needs no inside dope to smell heresy-banishment, along with the temptation that attacking the most archon-resented heretics (no matter how shoddily: fn 66) will bring advancement to any careerist willing to stoop that low.229 What scholar ever lost immediate status by adding to an establishment cult’s sacred crank literature? Concluding: [1] Outside the clique here examined, can historians-of-science recall any cases like the above-cataloged rear-guard mass-vandalism of data and thus of potential historical progress constructable thereon? (But, then, have there previously been unanswerability-panics of the magnitude that R. Newton & DIO represented?)[2] For the ancient astronomy field, has Curtis Wilson’s cleansing hope (fn 59) been snuffed? [3] Greek astronomy will ever rank uniquely as man’s 1st foray in precise predictive science. Its lovely place in history need not be desecrated by archons’ mundane limitations.230

DIO-J.H.A 22 ¶3 Data-Fudgery for Myth&Turf 2018 D.Rawlins

J2 228 Jihad-shunning (longstanding: above, fn 109, & Rawlins 1991W fn 171&173) of Diller, Newton, & DIO is known to all in the JHA-circle, resorted-to from careerists’ fear that honest critics are simply bad-for-business. Organizing such disgusting cultist behavior (which works by influence [e.g., fn 116], not ESP) is a disgrace to academia. Like marriage, shunning is easy to commit, hard to end (12 fn 34): usually originating in archonal rage at intellectual rebellion, in fields so degenerate that Disrespecting archons and exposing pretensions are worse crimes than the counter-crimes of smearing, shunning, deceiving, stealing, doctoral censoring, en route to effecting decades of knowledge-subtraction. The enlying entity commits to the banned’s worthlessness without anticipation of concomitant risk: what if the shunner then produces valuable knowledge? The shunner can never admit banishing valid scholarship. So either [a] he loses faces by unshunningly owning to a mistake or [b] omerta-fakes (e.g., fn 66) the exile’s vindicated work as being just as worthless as Infalibly decreed at the outset & bars non-denigrating citation. How many image-protective archons ever chose option [a]?
229 Whenever weighing cultists’ attacks on DIO’s frustratingly ridiculous induction and computational achievements, it may seem difficult to distinguish between [i] those meant to impress archons with toadily-awful lowly-slavery to the shun, from [ii] just innocuously misguided truth-seeking efforts, in a field with a limited number of puzzles, where endeavors inevitably overlap. Difficulty with [ii]: why would honest research keep resorting to doctoring or trashing data (§G-C above)? Further on the theme of openness/honesty in the history-of-science world: [a] In 1992, complaint from sometime Isis boardperson R.Kargon caused temporary cancellation of Johns Hopkins Univ’s Library subscription: DIO, 2.1 p.2. [b] At the cozy Mufa 1994/5/6 Dibner Inst symposium (M.I.T.), a display stack of DIO display samples’ sole copy of Tycho’s star-catalog (DIO vol.3: Rawlins 1993D) vanished.
230 History-of-astronomy’s present #1 archon Evans joined the unhHoly Trinity ruling the JHA less by quality of research (though that’s not negligible: fn 84&34, and www.dioi.org/cot.htm#gjne) than by ingratiatory loyalty to JHA power-operator-editor & AAS-HAD co-founder O.Gingerich. Due to these less-than-entirely-academic factors, upon Evans’ 2013 elevation to JHA Editorialship (Rawlins 2009E fn 7 had predicted years in advance that Evans was heir-apparent, apparent), no audible historian-of-science thought it mattered that (among other ethical lapses): in 10 he continues decades of ducking explanation of his laughable 1987-launched & 1998-suppressed lunar observation of 1981/7/16 (fn 8) and lets
References

Gerd Graßhoff 1990. History of Ptolemy’s Star Catalogue, NYC.

stand indefinitely-uncorrected his own miscalculated-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 hereabouts: [a] Ptolemy’s honesty (fn 9); [b] Greek data-incompleteness’s unattested-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, pre-post-erously, with Ptolemaic evidential circularity, using that very model to prove his normalcy, a truth-inversion warp that’s ruled the field 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 felicitously often-confirmed inductions of the distant future of the distant past.
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 archonial 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 clumsey Almajestists (the silliest on scintillating display here in [2] were, e.g., [a] a 2 different dates (37 days apart) for the same Venus maximumelongation, [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 actual Sun. The JAHH paper in question, J.Brandt, P.Zimmer, & P.Jones (below known as Brandt et al 2014B), attempted mathematical analyses of the Almajest’s stellar declinations, observed by four successive ancient Greek astronomers over nearly half a millennium. The journal and authors contend that the dozen Almajest-contemporary declinations could be Ptolemy’s observations, never warning the reader that zero evidence is provided to establish that claim, while simple, definitive, long-published, referee-urged proof to the contrary is below shown (¶C5) to have been deliberately omitted. The paper’s “bivariate least-squares” statistical analyses were not bivariate and thus didn’t exactly find any least-squares $S_6$ 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 $x$ (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,” and thus have faces so at risk of mezza-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 indubitable contribution to knowledge. Below, at ¶B, the most recent misfire (Brandt et al 2014B) is put into the context of decades of like uniformly baseless mobaganda (though those interested only in 2014’s mismatch may skip straight to ¶C5), 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 perverted.

Text for the Day:

In the 1946 Alfred Hitchcock film Notorious, German spy Claude Rains suddenly learns he’s oops-unknowingly been cumbubally sleeping with a U.S. spy, and realizes that his fellow German spies would snuff him yesterday, if they discovered his security-breach. So he has a talk with 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 untruthful prophets (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 astrolorger Claudius Ptolemy, who 4-times-out-of-4 reported observations of the Sun that were unendubably but captive-journal-unprintably 50 times closer to Hipparcos’ 280-old indoor tables than to the real Sun. The prime forums perpetrating this fantastic but profitable joke on academia and the public, for consecutive decades, are the Journal for the History of Astronomy and the American Astronomical Society’s 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 againandagain below. But as with oft-crafty Rains, the prime forums’ satirists are protected by the very instrument of the theory that such ultra-eminently 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 oft meticulousely documented in DIO.

2 Prime smear against dissent is Insanity (as with media on polls’ heresy) by magauniformed establishment-poilers & darlings Gingerich (fn 16) & MacGenius Swerdlow (fn 4&18, ¶ fn 35), scientifically-challenged (¶ 2 fn 8) MacG even mirorlessly calling JHU-physicist & JHAD-satan Robert Newton a Velikovskian “crank and a con-man”: www.dioi.org/j113.pdf, DIO 1.13 §§ D-D.

3 Nobody disputes the 50-1 indictment. But no Reputable Forum (including even popnags, newspags, & toob) 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 scientist 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 50-1 evidential crusher from a projected DR article, that the paper was suppressed by JHA, being finally published by DR 6½ later. Our discussion’s bluntness derives from 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.)

4 DR has asked AAS to monitor HAD’s “unprofessional” (Schaefter 2002 p.40) behavior: 2002/10/2, 2015/12/29, & www.dioi.org/jcxbq.pdf, emaili 2017b/26 (no reply) transmitting photographic proof of dishonest archonial smearing: www.dioi.org/pm1.htm, vs www.dioi.org/pm2.htm, tactics long known to DIO recipients, a class which includes the AAS, whose chief in 2017 joined the deaf&dumbers.

5 Some authors may be able, but this breed of apology never quite is. Evidentially countering such feeble and comically self-contradictory (Rawlins 1992V 10) and that is exactly why Ptolemyists eschew (¶ fn 52) risking rational debate with DR, written on www.dioi.org/deb.htm) or spoken (¶ fn 5) preferring character-assassination-stealth’s braver: fn 4.

6 Ptolemyists’ integrity-level (e.g., ¶6 below) generally shows us less in the (perhaps-unintentionally) deficient original paper than in subsequent failure to acknowledge its thesis-gutting flaws.

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1See 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-exposing Newton as the field’s cohering hater-object (Gingerich 1990 p.364; Schaefter 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

In 2011, DR belatedly responded to the 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, Aristylos, Hipparchos, and (allegedly) Ptolemy — reported and analysed in Almajest 7.3. In 2014, much-too-shortly before the resulting paper Brandt et al 2014B went to press in 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 undeterredly determined to promote more cultism.) And, indeed, no changes were made, in response to central points challenged by DR’s scientifically detailed 2014/8/26 referee report, www.dioi.org/anjg4.pdf, though an irregular sprinkling of (non-space-expanding) alterations was affected. The timing suggests that the paper’s case for Ptolemy as outdoor observer was not going to be derailed by mere evidence, much less a full discussion of issues. Some other referees might care enough to regard such treatment as insulting which would only divert from the main point: it’s counter to a journal’s obligation (and own best interests) not to take all pains to provide the most 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 cullicity and non-bravery) less from iniquity than from JHAish inability (increasingly typical of the whole ever-less-scientifically-skilled history-of-astronomy field) even to begin to tell balanced, competent technical research from cultist apologia.

Brandt et al 2014B p.332 claim that the 2nd century AD star-data of Almajest 7.3 “could have been taken by Ptolemy himself.” The evidence for this politically-convenient falsehood? Ptolemy was alive when they were recorded! — a fact which did not require a new article for bringing out, 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. (Too remote from principled stands even to recognize one, OG actually claims [fn 15], “I know my tastes in this regard why DR wants to be at odds with it.”) DR’s response was expanded to provide information about the referee’s claque — vainly asking, www.dioi.org/oww2u, JAHH to point out DR errors — which would have caused a neutral journal to choose a different referee. The JAHH’s brave reaction to this disappointment? Just run away. JAHH went silent, even blocking DIO’s email address. Which is why DIO is distributing the present DIO issue, with the offending paper right here at §2. Nothing new about this: it’s just copying the equally scientific, receptive, & ethical 1983

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15 Gingerich’s private ref-reports on DR’s work can’t resist personal remarks having no place in such. (Too often ref-reports are actually editor’s opinions, even to recognize one. OG actually claimed “I want to be at odds with it.” DR wants to be at odds with it.”) 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’ obsession: bizarre details (§ Cardenal Manning’s perceptiveness at §2 fn 5.

16 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.

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8 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/bjg3.pdf, accompanied by the requested 1982 ms. Brandt certainly deserves credit for updating star-data (fn 40) and for stimulating DR’s 2011 discovery (§C21), which everyone including DR had missed right along (even though Rawlins 1994AL had already concluded that +159 was the Clean Dozen’s epoch): for epoch +159 (unlike for +137) the split between Clean Dozen & Sick Six stars was overlaplessly clean: §C17. (But Brandt et al 2014B didn’t cite any of this.)

40 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.

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82 Below at, e.g., §B4.
Ptolemy observed, playacting which is intended to prevent the public from catching on to the field’s long-running thespian obtuseness 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 transmogrifying a clumsy data-faker into not only an honest scientist but a genius,10 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 adducing planted eggs to prove the Easter Bunny is real.

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

B1 Does it tell us something about the state of soft academe that the Journal for the History of Astronomy, the “premier”11 journal of its field, has for decades used pseudo-science and deception in the shall-who-just-say extreeeemely 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 astronomic history. JHA’s longtime #2 official, Harvard’s Astronomy 101 teacher & deft Disraeliesque trowel-wielder Owen Gingerich, has repeatedly billed12 this ancient faker and superstition-pretenders’13 “The Greatest Astronomer of Antiquity”, claiming that all who question this evidently-sensible-to-him proposition are the insane14 parties to the simmering-if-generally-suppressed dispute inevitably triggered by such superlative saleshype. We will first briefly examine a sample of the succession of careerist soldiers who 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 archbishops, thereby improving odds for future conference-invites, publication, posts, grants — all with confidence that no matter how outrageously unlikely their arguments, they had nothing to fear from contradiction, in any of their captive journals.

B2 Ptolemy claimed to have outdoor-observed a 140 AD solstice, though his report B2 typically disagreed with the real Sun by 1 3/15’ (Almajest 3.1) which has escaped detection & thus survived for millennia, persisting even today.15 Gingerich’s intended-to-be-anonymous 2000 referee report to Isis (outted in Rawlins 2003b) called Ptolemy-skeptics just a tiny bunch of paranoids — thereby inadventently and delusionally skewing most of the scholars in the field, even WHILE he is echoed in the 2015 JAHII referee report’s complaint that DR doesn’t respect those who disagree with him. Can it get any weirder? Well, actually, yes. As we see from www.dioi.org/pms.html, 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.

Ptolemy archon: “you mean she isn’t?”

A4 During the silent months that followed submission to JAHII of the paper that ultimately became §2 above, ever-cheerily-optimistic DR had entertained the possibility that WO was seeking a 2nd, more neutral referee. Finally, DR emailed a friendly 2016/2/29 letter, www.dioi.org/oww2t.pdf, to WO and learned that WO had written an arbitraric act which at last nakedly unveiled a proud new addition to JHAD covering-archondum (§A3). This confirmed that the usual heresy-containment info-control (standard for the last half-century: §2 fn 35) was being governed by the priorities of, in this instance, two colluding politicians. A final 2016/3/21 DIO letter (successfully sent to WO by alternate email address), www.dioi.org/oww3l.pdf, again tried collegially to allay hypothetical shyness (about editing a DR paper) by pointing out that DR could hardly complain of any WO deletions if he’d asked for them! As DR had, in his 2015/9/30 letter. The new letter ended with an easy test (In 13) of the theory that the paper had been dead from the outset. No reply. Which is perfectly consistent with the theory.

A5 On 2016/3/25, DR happened upon the final published version of Brandt et al 2014B and was disappointed that various key required corrections, which DR’s ref report had taken the trouble to point out, had never been made. The paper appeared in 2014, long before WO revealed his all-plain intention to suppress DR’s paper for heresy, not style. I.e., he can’t in 2014 blame the JAHII’s gross failures of editing (§math!) on DR’s not-yet-written 2015 paper (expanded to §2 here), www.dioi.org/pml.pdf, which ultimately granted total freedom to WO. (See www.dioi.org/iss.pdf for Isis’ like achievement in time-disjunction.)

A6 We will shortly move on to putting JAHII’s 2014&2015 sellouts into the context of the disgrace of worshippers’ ever-more-bullet-hole-ventilated half-century pretense that
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 juniorhigh school level. The 2nd was a clumsy fantasy.19 [1] Near a solstice, not only 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 referring by Phi Beta Kappa (fn 20) and by Reverend Gingerich, as usual (one might almost say: as-always, given the reliable brand of sheeple who man or oldboy Hist.sci.’s most prominent forums)21 has approved an argument implying that if we toss a ball upward and catch it 4 seconds later, a 9th-grader (or younger) can’t tell that it peaked at 2 seconds?

2 Swerdlow’s fantasy for explaining why Ptolemy’s four solar “observations” were ($\pm$2) scores of times nearer Hipparchos’ indoor solar tables than to the outdoor Sun: all ancients selected22 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 ordinates better: roughly 1°, and the solar semi-diameter is 16°, so his equinoct-solstice errors average about 4 times the distance from the Sun’s center to its edge: $\text{($\pm$2B).}[b]$ Further, we know that 2nd century BC Greek scientist Hipparchos reported real observations which disagreed with his theories and with each other ($\pm 3$ 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 description has become deliberate because [a] the plain Hipparchan counter-evidence (item[2] at $\text{($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 & Phi Beta Kappa!) is examined at R.Newton 1991 fn 20 and Rawlins 2018U $\text{($B2$-$B3$)}$. He and J.Evans continue (in ignorance of both the observing technique and the historical record: details at $\pm 3$ 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: $\pm 3$ fn 7.
[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 papyrus P.Fouad 267A, with solstice’s time correct to ordmag 1º (actually to a fraction of 1°, by chance) has yet enlightened any cultist.

See Rawlins 2018U for full details of ancient solstice-determination, and DIO’s new form & ref to DIO 2005 and to R.Newton 1991. Probably he & his perps, who have never read any of the relevant ancient and modern works, are still relying on old cultist nonsense that would’ve caused the instrumental & actual ecliptics to tilt-separate from each other.

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, DIO. 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-acceptable expertise, by forever-cutoff of correspondence): JHA and JAHH [& Isis].

22 See ScAm 1979, quoting Swerdlow & Gingerich, but primarily dependent on Swerdlow, as DR learned directly from the piece’s unblinded 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.

known within 2%, all 3 monthlengths accurate to 1-part-in-a-million or better, observatories’ latitudes correct to ordmag 1°), could never have been arrived-at over centuries of investigation, had ancient scientists just unprogressively copied their predecessors. The cited clique’s mass-slander of all ancient scientists’ empiricism and ethics is widely believed among academics, who’ve no notion that they have been protected by skewed journals from learning that it is nothing but a wrench of history directly caused by the continuing pretense that indoor-cheater Ptolemy was the ultimate ancient astronomer.

[d] The purely dreamt-up claim that it was standard practice for ancient Greek astronomers to select outdoor data to fit indoor theory, merely models all ancients after Ptolemy, in with two imaginative excuses:

the very same literally-preposterous logic LEARNED FROM PTOLEMY who faked “observations” agreeing with his theories, in order to then “prove” his theories from these same data. Shame-shame-Shame on DR for accusing JHADsters of ineducability. . . .

B4 Delambre 1817 and noted Rawlins 1982C had investigated the glaringly unique failure of Ptolemy’s 1025-star catalog (Almajest 7.5-8.1) to contain any stars lower than 6° above his horizon, indicating Hipparchos as the catalog’s observer, since his southern Rhodes Island observatory (geographical latitude 35°53’) stood c.5° north of Ptolemy’s Alexandria ($L = 31°12’$). So Schaefer 2001 contended at enormous length, in (yet-another!) JHA-Pb-anti-RRN paper, that the catalog could’ve been observed from Alexandria nonetheless because aerosols (atmospheric crud) blocked23 low stars. Among Pickering 2002A’s unanswerable responses: if this were the problem, the southern limit of the hunting stars wouldn’t explain physically-impossible repeated 1° errors in the 1st place! Especially again & again & again & again.

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, DIO. 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-acceptable expertise, by forever-cutoff of correspondence): JHA and JAHH [& Isis].

23Ever-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 $\pm 2$.

24Don’t miss www.dioi.org/pm3.htm, longtime (1970-2015) “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 itself to the brink of impotence into lapdog ever nearer the ultimate intimacy it aspires to: lapdancing a needy establishment.
to $\Delta \lambda = -29 \sin \lambda \tan \beta$, 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 Ptoleism has become a medical problem. Exhibiting the science-grasp of Ptolemy’s fellow-crane-liar F.Cook, MacArthur-Genius N.Swerdlow attacked 29’ $\tan \beta$ as indefinable near the celestial North Pole, where 29’ $\tan \beta = \infty$; this, merely from his own innocence (2 fn 8) of undergrad math: celestial (DIO 3 §[A2] (d)) or terrestrial (DIO 21 §[C1]) longitudes’ conversion to great-circle measure entails multiplication by $\cos \beta$, but (C2) $\cos \Delta \lambda$ can’t exceed 29’$. Swerdlow’s response: he hides (DIO 3 §[A2] (d)). Meanwhile, Evans’ attack on the (§B5) absent-error-waves argument confuted sine waves with cosine waves ($\pm (\Delta \lambda)$ blowing off a 63° 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 1½2 he sought.

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

[1] In 1981 Evans used a cross-staff to measure the longitudinal distance of a star from the mid-eclipse Moon. “I find on examining my notes from that evening” the longitude’s error was c.–40’ (Evans 1987 p.275), proving Ptolemy-skeptics were wrong to deny that ordmag 1° errors were not unusual for ancients. However, Rawlins 1991W fn 288 and Rawlins 2009E later showed that if Evans’ reduction hadn’t bungled his lunar parallax correction’s sign, the error would’ve been merely ordmag 1°. Evans’ response: he’s hidden “my notes from that evening” ever since, hiding also from questions on the incident put by Rawlins and Thurston. (See 3 fn 11. Comman Frederick Cook ducked inquiry identically [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 1989 p.259), Evans conveniently forgets to discuss that 1981 eclipse at all (switching instead to adducing a previously unmentioned 1977 eclipse he hadn’t outdoor-measured with) — and continues inexcusably contending for ancient errors of ordmag 1°.

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

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25 Seeing his own 1981/7/16 record proved DR right on that observation, Evans 1998 nonetheless repeats his 1987 argument that Hipparchos’ ordmag 1° errors in his 2 observations of Spica (–145 & –134) bolster the case for large ancient observational errors. Evans 1998 doesn’t tell his readers that Rawlins 1991W fn 288 had long since shown these observations had also been reduced by Hipparchos with the same parallactic sign-flip & that when corrected for this, both his hugely erroneous placements of Spica were merely bunglings of raw observational data accurate to ordmag 1°. Later, Rawlins 2009E §E showed Hipparchos had made the very same parallactic reduction-slip when seeking Regulus’ longitude, causing the worst error of all his fundamental stars (~35’). When corrected, the result turned out to be merely ordmag 0°1.1777 eclipse switch renders it undeniable he’s seen DR’s detection of his flub. Has he found fault with it? No. So he & his equally ethical colleagues combine to fake that detection’s non-existence, by 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 academe, 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-tied foundations of all arguments for archons’ precious central myth of ancient science’s fumbling non-empiricism.

26 Maeyama 1984 is graphical by trial & error and is monovariate ($\pm$ 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 1982C was just based on guesswork not statistics), as suggestive (totally unmentioned) 1981–1977 eclipse switch renders it undeniable he’s seen DR’s detection of his flub. Has he found fault with it? No. So he & his equally ethical colleagues combine to fake that detection’s non-existence, by 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 academe, 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-tied foundations of all arguments for archons’ precious central myth of ancient science’s fumbling non-empiricism.

28 Catalog from Hipparchos by (as long suspected among astronomers) just adding 2°2/3 of 1°/century precession onto Hipparchos’ longitudes, a fabrication betrayed by the precession’s falsity (actual precession then: 1°3.38/century) which ensured that, after 2 2/3 centuries of 0°.38/cy slippage, the fakes fell 1°.1 short of mean reality. Rawlins 1982C added that the Catalog would display large error-waves (details above: §B5) had anyone observed its stars with an armillary astrolabe mis-set by –1°.1 of celestial longitude. Evans 1987 tried impressively far-fetched schemes to confuse this desperate situation (the funniest by far was spoofed above at 2 fn 11), repeatedly following classic Ptolemy-apologist robo-attraction to the inherently unlikely and rejection of the likely. But Evans’ voluminous star-catalog appendix flamed out wherein he deduced: he had 1990 brilliantly tested (as neither Newton nor DR had thought to do) for the mass-statistical correlation of Hipparchos’ and Ptolemy’s star-places, proving (as Alex Jones witnessed 1°-hand), even to formerly-pro-Ptolemy-as-cataloger Almajest-editor G.Toomer’s honest satisfaction, that the catalog had indeed been plagiarized — thus vindicating Newton’s & Rawlins’ prior pioneering tests and disproving Evans 1987, Schaefer 2001, & Schaefer 2002.

C Latest Into the Lists

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

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

C3 Of Alm 7.3’s 54 star-declinations $\beta$ reported by 4 ancient observers, BZJ’s 2014 project examined 53: Timocharis 11 stars, Aristylos 6, Hipparchos & Ptolemy 18 each. These data had already been studied by Pannekoek 1955 (1° to appreciate the accuracy), R.Newton 1977; also Rawlins’ 1982 bivariate least-squares study, which Maeyama 1984 is graphical by trial & error and is monovariate (R.Newton 1982), demanding a mathematically unsophisticated & 1984 (received at Centaurus a year later, in 1983 June), whose standard deviations for the four
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 letters 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 are point in a direction other than its inexplicable Ptolemist conclusion. (Though R.Newton is cited as a skeptic on Ptolemy, none of his or DR's damning evidence 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 $d$ via the equation

$$d = L \pm Z$$

(1)

(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 $d$ 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 $d$ data contemporary with Ptolemy, all analysts since (including Brandt et al 2014B) have concluded that there is but tiny error (ordn mag. 1') in the observer’s assumed geographical latitude $L$. So it should not be hidden from the reader (as it is, throughout Brandt et al 2014B) that when Ptolemy reduces transit data (via eq. 1), he uses an Alexandria $L = 30^\circ\,58'\prime$ (Almajest 5.12-13), which rules him out as the declinations’ observer since this is in error by $-14'$ (Alexandria being at $L = 31^\circ\,12'$.)

[C] Some of the star-declinations allegedly observed by Ptolemy (c.+160) are so bad that Brandt et al 2014B p.332 invents a hitherto-unknown observer for them at 57 AD. But that date for ibid’s Lone Mystery Observer ($\pm 2$ fn 37) just-so-happens to be within $1^\circ$ (!) of the shortfall-date that the “Ptolemy” Catalog’s stars would end up at ($\pm 66$ 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/bzj/pdf, has the verbatim-same wording except for the citations, which were poorly and unhelpfully redacted: “Rawlins (1982, c.1983, 1994); Maeyama, 1984.”


28 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 $\pm 2$ fn 11), the case for non-fabrication vanished long ago: $\pm 66$.

had he faked them by adding 2 3/4 centuries worth of his false 1°/cy precession, namely, tacking $2^\circ40'$ 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 mispressed 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 his star-declinations (emph 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 Graßhoff 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 $d$ 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 $6$ star-declinations $d$ 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 1994L) 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 §C2.

C6 JAHH readers have a right to know §C5’s four central considerations, but Editor Wayne Orchiston (WO) has not felt the need to inform them. So much for the integrity of the paper’s Ptolemist 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 really only just a try (like Maeyama 1984) at solving a bivariate problem monovariately. Except for Ptolemy (where different samplings obviated a comparison), the resulting epochs $E$ and their standard deviations are mostly about the same as those of DR 1982, after the standard deviation of Aristyllos’ epoch $E$ was fortunately brought into near-agreement with DR’s recommendation. Instead, BZJ present miscalculated values for a single entity, “accuracy”, which they confusedly seem to regard as sufficiently equivalent. This becomes even harder to explain when we find that the earlier, refereed (otherwise nearly identical) version of the paper, www.dioi.org/bzj/pdf, has the verbatim-same wording except for the citations, which were poorly and unhelpfully redacted: “Rawlins (1982, c.1983, 1994); Maeyama, 1984.”

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 $E, x$, 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” $r_s$ 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 nullivariately for an independently estimated best $E$, [d] computing “accuracy” $r_s$ by meaning the minuscule leftover residuals. (Our reconstructions of data via this procedure are in fn 35.) Perhaps we could dub this the “least-non-squares test”. The impossibility of BZJ’s numbers are easily seen: when Brandt et al 2014B p.331 puts the “accuracy” of Aristyllos & Hipparchos at 0.009, this correlates to 14 & 13" ARCSCONDS resp — obviously a fantasy (and BZJ were warned of this on p.4 of www.dioi.org/gaulpdf. DR’s invited referee reports, 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 Maeyama, 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, Maeyama 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 0.3333333333. The residual 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 in $L$ to 4 in $E$. In Rawlins 1994L Table 3 and below in Table 1, this is roughly true. But no such symmetry appears anywhere in Brandt et al 2014A abstract or Brandt et al 2014B p.331. (Note: of paper Zinn et al 2013 admirably takes no part in evaluating anyone’s $x$ — or anything at all about Ptolemy.)

35 BZJ’s initial abstract, www.dioi.org/bzj11.htm, Brandt et al 2011, gave figures for “accuracies” (where we flip BZJ’s unconventional C–O signs): Timocharis $E = -295$, 11 stars $r_s = -0.022$, Aristyllos $E = -258$, 6 stars $r_s = +0.004$, Hipparchos $E = -128$, 18 stars $r_s = -0.010$, Ptolemy $E = -115$, 18 stars $r_s = -0.005$. Later, Brandt et al 2014A p.6 & Brandt et al 2014B p.331 note different $r_s$: Timocharis 0.012, Aristyllos 0.003, Hipparchos 0.004, Ptolemy 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 above false $r_s$ 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 $S_{min}$ to show that most BZJ solutions do not approximate it.]

Timocharis 12 stars: $E = -295$, $r_s = -0.022$, $S = 2745^{52}$.

$[E = -277^{58} = x - 0.0076 + 0.077, S_m = 2441^{52}].$

Aristyllos 6 stars: $E = -258$, $r_s = +0.003$, $S = 147^{52}$. $[E = -258^{50} = x - 0.0012 + 0.045, S_m = 147^{52}].$

Hipparchos 17 stars (Alioith $\delta = 6^{35}$/5): $E = -128$, $r_s = +0.004$, $S = 446^{52}$. $[E = -133^{58} = x - 0.0001 \pm 0.021, S_m = 39^{52}].$

Ptolemy 18 stars $E = +111$ (Brandt et al 2014B Fig.5 no-prop-mot), $r_s = -0.005$, $S = 2539^{52}$. $[E = +115^{13} = x - 0.004 + 0.052, S_m = 2521^{52}].$

36 In his 1983 Aarhus talk Maeyama did not yet know that “mean systematic error” relates to error in the observer’s assumed latitude. He later disremembered that he learned this from DR’s ms: fn 27 above. The results displayed at Maeyama 1984 p.292 Table 1 are not from bivariate but monovariately least-squares — just by graphing trial&error to find $S$. Nonetheless, the values found for $x$ (though not recognizing it as latitude-error) and $E$ are roughly correct, since Maeyama in-effect was running a double-monovariate 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 monovariate 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 $\sigma_x$, Maeyama 1984 Table 1 column d wrongly lists $\sigma_x$, the mean error of a single observation. The resultant errors range as high a factor of nearly 7 (the Hipparchan 44-star sample).
Table 1: Ancient Observers’ Epochs $E$, Adopted and Actual Geographical Latitudes $L$

<table>
<thead>
<tr>
<th>Obsrvr</th>
<th>$E \pm \sigma_E$</th>
<th>Adop $L$</th>
<th>Its Error $x$</th>
<th>Actual $L \pm \sigma_L$</th>
<th>$\sigma_o$</th>
<th>$\sigma_t$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Timocharis</td>
<td>$-302\pm0^9$</td>
<td>$31^\circ 12'$</td>
<td>$+4.5\pm1.9$</td>
<td>$31^\circ 10.5\pm1.9$</td>
<td>$\pm6.1'$</td>
<td>$\pm5.9'$</td>
</tr>
<tr>
<td>Aristylls</td>
<td>$-258\pm10^9$</td>
<td>$31^\circ 15'$</td>
<td>$+1.0\pm2.7$</td>
<td>$31^\circ 14.0\pm2.7$</td>
<td>$\pm6.1'$</td>
<td>$\pm4.2'$</td>
</tr>
<tr>
<td>Hipparchos</td>
<td>$-131\pm0^5$</td>
<td>$36^\circ 08'$</td>
<td>$+0.2\pm1.2$</td>
<td>$36^\circ 07.8\pm1.2$</td>
<td>$\pm5.2'$</td>
<td>$\pm5.0'$</td>
</tr>
<tr>
<td>Anonymous</td>
<td>$+159\pm0^9$</td>
<td>$31^\circ 15'$</td>
<td>$+4.4\pm2.0$</td>
<td>$31^\circ 10.6\pm2.0$</td>
<td>$\pm6.0'$</td>
<td>$\pm5.6'$</td>
</tr>
</tbody>
</table>

Note: The grouping is E (Early 6 stars) and L (Late 9 stars), which does not follow the grouping into 2 stages, in 1994 and — thanks to Brandt’s restimulation of interest — in 2011. But this was explained in DR’s 2011 letter to Brandt, to no effect.) The ref report also noted a misprint in Brandt et al. 2014B, but 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. Some of the data cited above involves Robertet et al. (1970), as 81° 15' 15", but later misrecognized (like §3 fn 44) as 81° 15' 15". This, as at Almajz 7.5. Thus, reconstructed true delta 8° 14' 15" or 8° 56", which also shows a spin residual and the Andursus entry, thereby equating Observed-minus-Calculated with what is actually just the rms of the effect of average rounding, in degrees. We thank Jack Brandt for urging the importance of modern satellite-based star-places. Versus the Rawlins 1994L results: the maximum effect on epoch $E$ was $1^\circ$; on $L$, just a fraction of $1^\circ$; but the improvements are welcome.

43. The errors&oddities in Brandt et al. 2014B’s sinusous 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 changed slowly in declination. The DIO referee report advised that Aldebaran’s declination-speed was exactly right cannot occur without simultaneously doing likewise for $x$ and $\sigma_x$.

44. But oxidized-Sachs like the various DR et al. 2014B Figures confuse O–C (Observed-minus-Calculated) with O–O (evidently a routine weakness among historians-of-astronomy [though here fortunately harmless], e.g., §2 [FR, & www.dioi.org/ifm.htm#bvmn]. And Brandt et al. 2014B’s eq. (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 Arcturus — leaving a consistent set we might as well call the "SickFive" — the resulting (unweighted) residuals are mostly about 1/2 degree, the smallest being 17'. No overlap at all. A lovely split. So there’s just no need to get fancy over dividing the “Ptolemy” 18 stars. Unless one is extremely determined to undermine acceptance of R.Newtonian skepticism about Ptolemy — by any sleight necessary.

C18 For finding epoch E, Brandt et al 2014B adheres to depending on each star’s “crossing time” (the year when its residual is zero) & “slope” (rate of change of declination/year). Though of some interest and utility as rough checks (on better procedures), these approaches are sub-prime (especially when compared to standard procedures — which are provided by BZJ since they give results in accord with R.Newton’s), 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.

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#wsa), 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.

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 potently bad possibility 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 (0’.3338/yr) to virtually zero — the latter producing nearly valueless crossing-times, which lead to exclusions and inclusions based on virtually random happenstance. The cluster-analysis deletes (p.335) Castor, Altair, Betelgeux, & Sirius (not consistent with earlier deletractions [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 Pollox, whose slope is smaller? — probably because their mutually wan slopes (nearly horizontal) in Fig.7 magnify a tiny difference (just a few arcm: less than σ₀!) 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 accidentally 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, 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 at fn 35 (more than micro-trivial noise) by varying either x or e values, to find an x or e which anyone could’ve foreseen by noticing that both papers inexplicably got deeply involved in graphical solutions by trial&error, the latter paper soberly treating the former as the prime prior research!

C23 As a final quietus to JAHH’s monumentally stubborn 2014 adventure, we now show how easily a scrupulous journal could have checked on whether its or our solutions for x were correct. All JAHH needed to do was: vary the x&c of their solutions to see if their residual-squares-sum S was minimal, i.e., equal to our minimum, Sₒ. (Which is the square of the appropriate σ₀ 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 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 Sₒ. (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-e space where S is a minimum. If BJZ have any doubts that DIO has found THE actual minimum S, they need only conduct the very same test, using our x or 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 e.

Our solutions for x are just 0-4 arcm. 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 1° accuracy. For that reason, as well as Brandt et al. 2014B’s p.331 advertising 1° accuracy, the most precise solutions for x are appropriate. This becomes important (fn 45) for the Clean Dozen, where x = 4’, closely reflecting the error in the observer’s adoption (independently demonstrated in Rawlins 1994L [F8]) of L = 31°1/4 for Alexandria, which is 3° (close to 4°±2’) larger than the reality: L = 31°12’.

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

D Watching a Cemental 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 zoodapt, one may justifiably draw conclusions.

D2 The truth behind the unprincipled 48 — sometimes (e.g., fn 18) even vicious — stubbornness 49 of those determined to protect Ptolemy from public exposure by any means (Ptolemy oridently) is that they are either protecting and/or 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 [2] §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 [2] §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] slandering ([2 fn 5]) their opposites as fools, knaves, and nuts (before discussing evidence — if ever doing so at all), while [2] pointing 40 innocent onlookers to the bemedalled, Reputable people who’ve taken their side: after all (as we ask at above p.87, in the Text-For-The-Day intro to this article), how could such cynoscuresae seem so Enormously Stupid — as they must be or act, if skeptics are right?

D4 Well, here’s exactly how: just [a] keep smearing heretics behind their backs (details & photos at §B1 above) while continuing to [b] publish pseudo-defenses of Ptolemy’s honesty — no matter how ridiculous ([3] fn 66). Meanwhile, disallow — as too Disrespectful 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 outrageous 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.dio.org/mot.htm#gbsc): a man who can’t be bribed can’t be trusted.

49 Schaefer 2002 rightly deemed the Ptolemy Controversy the hottest in the entire field of history of astronomy. Which is why the decades-long near-hermetic suppression of one side of the debate is so: impressive. And as ethically repulsive as the tactics employed — by the chief of the field — as so unambiguously documented here and in, e.g., [2] fn 1, 3, & 5.

50 Without citing the various powerful evidential proofs that Ptolemy stole the star catalog, Schaefer 2013 p.47 instead reassuringly resorts to sociology to aver that we can’t KNOW so because herd-loyal Ptolemists (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 jollypol Schaefer is smiling as he watches the JHAD show. And one recalls Thurston’s quote from Bishop Berkeley, “I observed how

51 The cumulative weight of scientific evidence — which the JHAD show, Schaefer, and influential power brokers (including Brandt et al.) are so determined to prevent from reaching the ears of the public — is so overwhelming (see www.dio.org/mot.htm.)

D5 Such childish obviously illusionism, as delineated at §D4 above, utterly confounds the non-specialist part of academe, as well as the increasingly non-investigative (“science”press, and is the key to the endless pretense of Ptolemist historians — of science — knowingly careless of concomitant hurt to academe’s accurate perception of ancient history — that archons have not-otherwise been proven as Notoriously foolish as Raines: if just a few of puppeteer Gingerich’s claque can forever ([52], & Rawlins 1992V §C24) keep publishing contrived even-if-laughably-transparent-to-scientists defenses, sapping an ever-befuddled logorid press-core, then the prime long-term public perception is secure:

Not a single Ptolemy-defense archon was ever wrong on his honesty.

(Gerald Toomer the admirable rule-proving exception: §C5.) To normal folk, this may seem a puzzling, feeble, even valueless achievement. But not to those who thrive (& fiscally survive) on a vanity of judiciousness or infallibility that’s the antithesis of the scientific attitude of inquiry, and of humility to the rule of evidence.

Beyond Ptolemists’ lack of science’s attitude is the mundane matter of skills. Virtually every member of their clique, whatever his eminence, has no high scientific expertise 52 relevant to the Ptolemy controversy. Non-specialists — unable to understand the debate’s technical details (or too busy to take the time) — are often impressed with networking archons’ too-off-network-generated posts, awards, university connections, etc. And they are thus easily diverted from the seemingly obvious point that just because a Ptolemist is an astronomer doesn’t mean that he knows much about positional astronomy, orbit theory, & statistics, which are the specialties one needs for research into ancient astronomy. (Astrophysics, planetary astronomy, & spectral analysis are worthless for it.) These are the very specialties of such astronomers as R.Newton and DIO’s Myles Standish and DR. Lack of such expertise shows up in the truly historic fumblings of, e.g., Swardlow & Evans, as shown above in §B and the especially comedic do-not-miss topsy-turvyfest at [2] §N — and now in the serial-learning-experience stats (§C8) of JAHF’s 2014 paper.

D7 Concluding: we anticipate that (unless made shy by our 2014 referee report, www.dio.org/jau8q.pdf) Ptolemist archons are already typically ([3] above) pointing to Brandt’s many awards&posts, as if relevant, proud that yet another Reputable Figure has, after reviewing the evidence, decided to help the needy establishment by coming out for Ptolemy, hoping no-one will notice the §A1-obviousness of the fact that eventual discordant-evidence-sterilized Brandt et al. 2014B’s conclusion was all-along set in cement.

Slippers.

52 In mathematical history-of-astronomy, dimbulbs + carreestars + thespians + poles now constitute a majority. If able, honest scientists ever rejoin&review the present era, it’ll be remembered, with eyes atop the fence. As Dark Ages, when reason was but dimly propped in favor of Invincible Ignorance. Archons will stoop to ANY tactic, to postpone that day indefinitely. Understandable. For them.
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