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Annals of Science (1996 July), reviewing DIO vol.3 (Tycho star catalog): “a thorough work . . . extensive [least-squares] error analysis . . . demonstrates [Tycho star-position] accuracy . . . much better than is generally assumed . . . excellent investigation”.

British Society for the History of Mathematics (Newsletter 1993 Spring): “fearless . . . [on] the operation of structures of [academic] power & influence . . . much recommended to [readers] bored with . . . the more prominent public journals, or open to the possibility of scholars being motivated by other considerations than the pursuit of objective truth.”

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Publisher: Dennis Rawlins (DR), address above.

DIO is primarily a journal of scientific history & principle. However, high scholarship and/or original analytical writing (not necessarily scientific or historical), from any quarter or faction, will be gladly received and considered for publication. Each author has final editorial say over his own article. If non-DR refereeing occurs, the usual handsome-journal anonymity will not, unless in reverse. No page charges.

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

Most unattributed text is DR’s.

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Written contributions are especially encouraged for the columns: Unpublished Letters, Referees Refereed, and regular Correspondence (incl. free errtime for opponents). Contributor-anonymity granted on request. Deftyly or daftly crafted reports, on apt can—


This DIO’s & others’ bluntness has little to do with why historians-of-science flee us.


[2] In 2017, DIO told (e.g., §1) toppe history-of-science journal Isis of [a] its 2015 theft, www.dioi.org/isa.pdf, of an original DIO 2008 discovery (residing for 7+ in ordmag 100 libraries worldwide), & [b] its 2016 laughably-math-bungled (§1 §3) attacks on DR’s 1984 Greenswich Centenary conference paper; pseudo-refereeing of Isis’s 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/qio.doc) of unmild pp.46-85 within, whose history&science “history-of-science”-Isis somehow never commented on in five emails, Isis Editor H.F.Cohen wouldn’t acknowledge receipt of our protests against Isis’ 2015 echo of & 2016 attack upon DIO’s researches, instead emailing (pp.9&45) he wouldn’t answer or read future DIO communications. Our objections were then sent Isis’ Board as Letter-to-the-Editor: article §1 here (pp.3-8). Read it for yourself & see if it is an unreasonable or angry document, esp. under such outrageous circumstances.

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

1Main changes: DIO’s letter to Isis, www.dioi.org/islg.pdf, has minigrated to §1; §18 added to §2; & §3 is a plainly blunter version of www.dioi.org/qio.doc (sent Isis), but has virtually the same content.
References

Gerd Graffito 1990. History of Ptolemy’s Star Catalogue, N.YC.
Gerd Graffito 1990. History of Ptolemy’s Star Catalogue, N.YC.

Διονυσιακή Αστρονομία. Συγεγραμμένη τον περίπου 160 μ.Χ. από Ανδρέα τον Μάνιτιους. Εκδόθηκε το 1912-13.

Διαβάστε την περιοδική έκθεση της δύο άρθρων του 2016 Νοεμβρίου του περιοδικού DIO, σε διαδικτυακή μορφή με τίτλο “The Accuracy of Ancient Cartography Reassessed: The Longitude Error in Ptolemy’s Map,” by D.Shcheglov, Isis 107.4 pp.687-706, is the most recent in a 4-decade succession of post-Robert-Newton attempts at “rehabilitating” thoroughly-exposed (below §items [1]-[4]) 2nd century AD mathematician-fabricator-astrologer Claudius Ptolemy (“The Greatest Astronomer of Antiquity” to influential astronomy-historians O.Neugebauer, Evans, & O.Gingerich), by unwittingly-ironic demeaning of physicists Newton and Rawlins (robbing both of credit for their discoveries), latest of those scientists who’ve since Tycho (1598) revealed fraud in Ptolemy’s corpus.

Rawlins’ contribution to the 1984 Greenwich Centenary concluded that, ere astronomers mangled them, accurate maps existed in antiquity with longitudes based on lunar eclipses. Shcheglov calls such maps a “delusion” since eclipse-use is “impractical”, citing in support “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 p.693, imputes several failings to DR’s and DIO’s work (the less spectacular are reserved for a footnote).1

A. 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, datatting hypothesis: that the 1.4-factor error (40% overestimate) in Ptolemy’s Earth-longitudes was from just multiplicatively stretching the other two by treatting a Pliny solar eclipse as lunar, and by putting Kleomedes’ Spain in the wrong hemisphere (likewise for Xi’an & Luoyang: see POSTSCRIPT below). Whose results can never work as efficiently as plain, raw multiplication.

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 (idem) to follow underlay Ptolemy’s longitudes. (My spare proposal was a simple longitude-multiplication, without any connexion to Shcheglov’s amazing & valuably complete reservoir of centuries of stadelength guesses.)
B Shcheglov (p.705) calls early geography “aquaint illusion” — & his Abstract [catnipped Isis by promising] “Ptolemy’s reputation is rehabilitated in part, and the delusion of high-accuracy ancient cartography is dispelled.” The dispelling is expected by arguing that Greeks couldn’t use eclipses for longitude, skipping all the evidence they did (Rawlins 1984 Greenwich). Shcheglov’s [Muffian 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 §E; JAH7 17 p.326)
0% for star declinations (ditto)
1% for Earth-circumf. precision (ArchiveHistExactSci 26 p.216; DIO 14 §1 §A & eq.28)
1% for Earth’s tilt or “obliquity” (Klio 27 p.266; DIO 16 §3 §3 §A-B eq.2 and Tables 1&2)
1% for lunar mean distance (Almagest 5.13-17; DIO 8 §1 §H4: 59 Earth-radii vs really 60)
10% for lunar-eclipse-prediction (DIO 1.1 §6 eq.32)
1% for time of lunar eclipse (Greenwich 1984 in Vistas in Astronomy 28 pp.258&265)
0% for lunar limbus 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)
1% for ~153/2 equnox on Alexandria Palaestra polestar-set ring (Isis 73.2 p.263 n.17)
10% for sidereal year (DIO 6 §1 fn 46 §G; DIO 11.1 §3 Table 2; DIO 11.1 §1 fn 14-15)
1/century for mean motion of Mars and arguably Venus (DIO 11.3 §6 fn 26)
0% for synodic month (DIO 6 §1 eq.2 & fn 12&18; DIO 11.1 §1 eqs.1-8)
1% for anomalistic month (DIO 6 §1 eq.13 & fn 12; DIO 11.1 §1 §A3 & eq.2)
0% for draconitic month (DIO 6 §1 eqs.2&19 & fn 12; DIO 11.1 §3 eqs.1&3). \[iff \]

Most historians-of-astronomy are, like Shcheglov, unaware of these symptoms of high Greek science, some, e.g., Gingerich&Swerdlow, speculating without attribution: how could scientists keep only theory-accordant data [flatly contradicted by Hipparchos' record: \[iff \]J, thus unwittingly modeling all ancient science on a blundering astrologer, Ptolemy. QUESTION: how could the above-listed measures have ever progressively evolved into accurate achievement by following a tradition of just keeping on confirming prejudice? [Classic projection from own behavior? See below POSTSCRIPT's final line.]

C That ancient geographers’ longitudes were based on eclipses is doubted by Shcheglov p.690 as “too impractical”. I’ve outdoor-eyeball-timed enough lunar eclipses to know their accuracy is ordmag 1% [anciently somewhat vitiated by sundial graduation limitations, ordmagly] agreeing (at 4/91/1) with the well-under-1° accuracy of pre-stretch Geography longitudes, D.Orawlins 1985, “Ancient Geodesy: Achievemen and Corruption”, Vistas in Astronomy 28:255-268: p.265 (1984 Greenwich paper). Though eclipses are common (Ptolemy experienced 3 in 3 years: 133-136 AD, Almagest 4.6), Shcheglov’s n.8 accepts INDOOR (Rawlins op cit §10) astrologer Ptolemy’s giveaway-incredible data [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 3309/20 “Arbela eclipse”. Ptolemy’s reported time (longitude) gap is 4/3 too big, so Shcheglov’s n.8 tries aliasing 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 (4th pair: Pliny’s correct 2° Sicily vs Arbela).

D But Heron didn’t even try to gauge longitude-gap by eclipse (Neugebauer, History of Ancient Mathematical Astronomy, 1975, p.848). Kleomedes’ 4° gap is virtually correct since Cadiz at 25°W longitude and Persepolis at 38°22E are 3°57′ apart. Shcheglov just mis-signed Cadiz and found 3°07′ (comfortingly consistent with Ptolemy’s false 4/3 factor — publication or even citation (3 §A1 item [A]) of expert criticism of this Special Literature (especially DIO’s), revealing defenders’ fatal mismath. No exposure or admission of JHA’s cringing Editor Evans’ 1987 parallax screwup & suspension of his experimental record (§B6 item [1]) will ever appear in the irredeemably dishonest JHA, though undoing this now-conclusive deceit is a required-essential aspect of any counter to Ptoleemism, since it is the Pb-paper-prominent “empirical” centerpiece of the JHD’s fantastic 1987-1998-to-eternity tenet that huge, Ptolemy-sized observational errors were normal in antiquity. Further, no mention is allowed of definitive evidence (fn 3) of JHAD crimes against academic decency (such censorship constituting just one more crime to be henceforth protected by “archons”), evidence-hiding (as just noted), data-fudgery (\[iff \]C §3; \[iff \]C §A, & Rawlins 1992V §C24) keep publishing contrived even-if-laughably-transparent-to-scientists defenses, sapping an ever-befuddling lapdog press-corps, then the prime long-term public perception is secure: Newfoundland obvious illusionism, as delineated at §D4 above, utterly confounds the non-specialist part of audience, as well as the increasingly non-investigative (and even-semiunnumerate) “science” press, and is the key to the endless pretense of Ptolemist historians—of-science — knowingly careless of concomitant hunt to academe’s accurate perception of ancient history — that archons have not—either been proven as Notoriously foolish as Raines: if just a few of puppeteer Gingerich’s claque can forever (\[iff \]A2, & Rawlins 1992V §C24) keep publishing contrived even-if-laughably-transparent-to-scientists defenses, sapping an ever-befuddling lapdog press-corps, then the prime long-term public perception is secure: Not a single Ptolemy-defense archon was ever wrong on his honesty.

(Gerald Toomer the admirable rule-proving exception: §C5.) To normal folk, this may seem a puzzling, feeble, even valueless achievement. But not to those who 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.

D6 Beyond Ptolemites’ lack of science’s attitude is the mundane matter of skills. Virtually every member of their clique, whatever his eminance, has no high scientific expertise[5] relevant to the Ptolemy controversy. Non-specialists — unable to understand the debate’s technical details (or too busy to take the time) — are oft impressed with networking archons’ too-off-network-granted posts, awards, university connections, etc. And are thus so easily diverted from the seemingly obvious point that just because a Ptoleman 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 results: Kleomedes 4° Spain vs Persia; Heron 2° Rome vs Alexandria; Pliny 3° Campania vs Armenia (4th pair: Pliny’s correct 2° Sicily vs Arbela. [5]In mathematical history-of-astronomy, dimbubs + careearists + thesians + poles now constitute a majority. If able, honest scientists ever rejoi &review the present era, it’ll be remembered, with eyes aroll, as the final Dark Ages, when reason was punitably proscribed in favor of Invincible Ignobility. Archons will stoop to ANY tactic, to postpone that day indefinitely. Understandable. For them.
central point here is (as 1° revealed in Rawlins 1982G) that ancient scientists found L to
ordnag 1° accuracy. For that reason, as well as Brandt et al 2014’s p.331 advertising 1°
accuracy, the most precise solutions for x are appropriate. This becomes important (in 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°14/4 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.dioi.org/biv.htm#bndl.

D  Watching a Cenental Field Resort (& Slipper) to the Bottom —
How Archons Justify Printing & Printing & Printing Just One Side

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

D2 The truth behind the unprincipled48 — sometimes (e.g., fn 18) even vicious —
stubbornness50 of those determined to protect Ptolemy from public exposure by any means
(www.dioi.org/mot.htm) is that they are not protecting him or their goonoo — 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, Ptolemaic archons are the sort of people whose idea of intellectual
engagement tends (for obvious reasons) not towards weighing scientific arguments but to
[1] slandering (±2 fn 5) their opposites as fools, knaves, and nuts (before discussing evidence —
if ever doing so at all), while [2] pointing55 innocent onlookers to the bemedaled, 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 cyanosura seem so Enormously Stupid
— as they must be or, 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 honest —
no matter how ridiculous (!§ M6 fn 60). Meanwhile, disallow — as too Disrespectful51

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 crucially and frequently — that is, whenever we resort to outrageously untrue extremes like competent scholarship, ethical dealings, and defying Infallible Archons. (To pols, it just doesn’t get any dirtier — or extraterrestrially unfamiliar.)
Don’t forget boldly-untrustworthy DIO’s prime motto (www.dioi.org/mot.htm#gbs): a man who can’t be bribed can’t be trusted.
49 Schafer 2002 rightly deemed the Ptolemy Controversy the hottest in the entire field of history of astronomy. Which is why the decades-long near-hermetic suppression of one side of the debate is so: impressive. And as ethnically repulsive as the tactics employed — by the chiefs of the field — as so unambiguously documented here and in, e.g., §2 fin 1, 3, & 5.
50 Without citing the various powerful evidential proofs that Ptolemy stole the star catalog, Schafer 2013 p.47 instead revealingly resorts to sociology to aver that we can’t KNOW so because herd-loyal Ptolemaists (like BZ2) 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 jolly-jolly Schafer is smiling as he
watches the JHAD show. And one recalls Thurston’s quote from Bishop Berkeley, “I observed how
unaccountable it was, that men so easy to confute should yet be so difficult to convince.”
Another DIO motto (DR), which extends also to many other faiths: “Why does anyone continue believing and/or their goonoo — resorting to any sloppy argument, any curtailment of free discourse necessary

51 Disrespectful, Ya-Disrespected-Me, sound familiar? Seen any mob or blaxpl0 films lately? 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, Gaggara are 1°47’4”, 2°01’14°, 2°23’10°, respectively, all indeed about 2°.]. So Shcheglov concluded that Pliny’s 3° is too high. Yet Pliny doesn’t say 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’t just equate time-difference and longitude-difference. Local Apparent Time for the eclipse differed in Naples from that at the 3 Armenian cities, by 2°29’, 2°48’, 3°14°, respectively, mean 2°50’. So Pliny’s 3° was not “badly over-estimated”. (Neugebauer, op. cit., p.688, had verified Pliny, educationally.
adding, “Solar eclipses are, of course, without value for longitudinal determinations.”) So, ironically, both of Shcheglov’s eclipse-examples for ancient inaccuracy have backfired.2

E Shcheglov’s other Pliny record is the same Arabela lunar eclipse Ptolemy mis-reports
as 8 P.M. at Cartaghe, 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 Arabela-eclipse reports; & neither he nor any other
historian-of-science has noted that “authoritative” (§G) scientist Ptolemy has accidentally
missigned Arabela’s 8 P.M. to Cartaghe! A check of his probable source, Pliny loc.cit.,
reveals by: how 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 Cartaghe time. Since his 4/3-stretched map already had
Arabela 3° east of Cartaghe (real gap 2h1/4), he faked Arbela thusly: 8 P.M. + 3° = 11 P.M.
Then, assuming his geographical astronomical calculations and the Geography’s
Gingerich’s Greatest Astronomer of Antiquity? Ptolemy’s times mega-dissagree with not
just reality but his own tables: 2h3°14’! [Error about as big as quantity sought: like
3° fn 42.]
Shcheglov notes no discords nor Arabela-Carthage-mixup, though all are at fn 45 of the

F On p.705, Shcheglov’s varied attempts at “rehabilitation” include his pure guess that
Ptolemy’s sources were bad (as if The Greatest couldn’t better discriminate): “it would be
unfair to blame Ptolemy for his errors, because the whole tradition he relied on was of a chain
of errors.” Yet we’ve just-above seen how a reliable source, non-astronomer Pliny, was
farcically bungled by “astronomer” Ptolemy, all by himself. If he was this
nonscientifically unreliable on his own, why doubt that, when switching to 500 stades/degree, he was so isolated from scientists (see above §§C&D) of ordinary eclipse data and use thereof) as to believe that his source map’s longitudes were overland-distance-bases, so
that he needed to multiply by 7/5 a traditional 700 stades/degree globe’s longitude-differences?
Any real astronomer knew the degrees were based on eclipses and should be left alone.

G So Shcheglov’s n.16 calling Poseidonio’s dilettante but Pliny an “astronomer” and
(p.694) a geographical authority is Quant at best. Unmentioned in Shcheglov’s attempt
to convince historians-of-science that Ptolemy should be somewhat “rehabilitated” (p.687):
[2] “Astronomer” Ptolemy’s four allegedly outdoor solar observations are
defying Infallible Archons. (To pols, it just doesn’t get any dirtier — or extraterrestrially unfamiliar.)
[p.694] a
49 Without citing the various powerful evidential proofs that Ptolemy stole the star catalog, Schaefer 2013 p.47 instead revealingly resorts to sociology to aver that we can’t KNOW so because herd-loyal Ptolemaists (like BZ2) 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 jolly-jolly Schaefer is smiling as he
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51 Disrespectful, Ya-Disrespected-Me, sound familiar? Seen any mob or blaxpl0 films lately?
lattitudes of real observers Timocharis, Aristyllos, Hipparchos, & Ptolemy’s Anonymous.

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 lattitudes in ancients’ 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, Messalia) . . . . Ptolemy calls such cities . . . ‘foundations’ that should be used as reference points for developing the rest of his map.” No mention that all five “foundations” cities’ Geography latitudes are seriously wrong (rms 26° = ordmag 1°): errors −14°, −30°, −43°, −14°, −14° (mostly quarter-degree negative, from astrologers’ amateurish use of asymmetric gnomon). Meanwhile, statistical stellar analyses by Rawlins (Iris 1982; & DIO 1994, thrice cited in “Secrets”, which Shcheglov read, Y.Maeyama (Centaurus 1984), & J.Brandt (JAHH 2014) show that all 4 real, non-ancient Greek scientists cited above at [GJ 3] knew their latitude to ordmag 1°. This twice-confirmed Rawlins discovery undoes Shcheglov’s entire inaccurate-geography thesis. He doesn’t mention it. Nor does he mention the contradiction it obviously creates versus the Geography’s mean latitude error of ordmag 1°. In response to the disjunct, one JHA Editorial Borehoff has offered that geographers must have ignored astronomers! (falsely). Ptolemy’s Geography was way out of date. DR mathematically comments (“Achievement” pp.260-264) these hitherto-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 consistent with the astronomer-scientist Shcheglov sees Ptolemy as.


K Selling or owning klimata tables for every latitude-degree was impractically voluminous. [Thus, if Almajest’s 1 1/4 klimata-interval was adopted, then each city whose longest-day was closer than 1 7/8 to a klima was grouped under it, its latitude made equal to exactly that klima’s latitude.] Such groupings of cities under ONE latitude is explicitly attested at Geography 1.4.2 (even while justly criticized at ibid 8.1.1; different authors, in all likelihood). Effects of such data-tampering are obvious from errors found [in §3’s sample], while direct ordmag-values for Ptolemy-accurate Hipparchos ignored. DR mathematically comments (“Achievement”) p.260-264 these hitherto-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 consistent with the astronomer-scientist Shcheglov sees Ptolemy as.

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 24000 stades by Kleomedes 1.10; 180000, by Strabo 2.2.2. Was the pre-stretch globe Poseidonios’? How fruitful is the 4/3-stretch theory?

N Dividing 4/3 into §3’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 c.2°. Same 6° appears 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.

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As is their remoteness-independence 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. BZJ will then encounter a sorta-pleasant surprise: all four astronomers’ values of S are correct. All four astronomers’ values, to find an S, the (more reliable) median is 59°, hinting both are skewed high by a few goofs.

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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, extremely determined to undermine acceptance of R.Newtonian skepticism about Ptolemy — by any sleight necessary.

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

C19 During their E-search analyses’ odd-option dependence on crossing-times (instead of obviously-preferable measure by residuals; reminiscent of www.dioi.org/fff.htm#twsa), Brant et al 2014B tries including weights by slopes’ absolute magnitudes (p.331 & Fig.6), the kind of Legendrian primitive that Gauss devised least-squares to obviate. (Again: this requires deletion of stars which Gaussian analysis doesn’t.) But when the paper moves into cluster-analysis, even this precaution vanishes. The paper includes with a long, illustrated section (slightly altering §C17’s L-vs-E regroupings that replaced Ptolemy’s simple split) which tests for clusterings in stars’ crossing-times. This is a patently poor basis for eliciting anything valuable, for the obvious reason that the crossing-times’ 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 deletions [p.332] of Aldebaran, Betelgeux, & Sirius [§C19]), yet in both cases, stars with slopes weaker than some of these are retained. (See, e.g., fn 42 above.) E.g., why does the clustering section of the paper eject Castor but keep Pol lux, whose slope is smaller? — probably because their mutually wan slopes (nearly

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

46 If we re-check the residuals via monovariate test for L at Ptolemy’s alleged observation-year, +137, instead of +159, we find the Clean Dozen more poorly fitting, residuals ranging from −8°1/2 (Zubenelengebi) to +15°1/2 (Betelgeux), the departure from zero of the worst is half again larger. This is one of several recommendations for using the full Clean Dozen and epoch +159 — others being: a lower median error (3° vs 4°), even despite a higher σo (6° vs 5°), an untampered sample (no deletions); and agreement with the Suda’s date for Ptolemy, Marcus Aurelius, +160, not the Ptolemy-claimed epoch: Antoninus, +137. (More exactly, the two epochs are 160/7/14 and 177/7/20 Alexandria App.Noon.) Note: a monovariate e solution for the same stars leads to ±150, a serious difference (see §C13 on similarity). And, since such automatically assumes x = 0, we have L = 31°1’6” (see §C16 above, & Rawlins 1994L §35), which is 3 nautical mi north of Alexandria’s L = 31°12’, whereas the ±159 bivariate solution x = 4° closely reflects the ±3° error in the observer’s overlap assumed L = 31°1/4, and so is effectively right-on: L = 31°11’±2” (ibid Table 3). All of these neatnesses render it doubly strange that Brant et al 2014B persistently refused to recognize DR’s discovery of ±159, misprinting it (over warnings) again&again as +131. See §C21 below.

47 See at www.dioi.org/pg01.pdf, DIO 1#1 §2; & www.dioi.org/pg03.pdf, ibid §3 fn 13, the parallel case of now-neglected metronomical theories that keep getting proposed to weakly explain the already strongly explained ancient Earth-size of Sostratos-Eratosthenes.
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.

[iii] By the correct (now generally-accepted, but still sniped-at) 185 meter stade, 256000 stades is 19% high, near 6/5 of real C; Poseidonios’ & Geography’s C, 180000 stades, is 5/6 low. All the three Rawlins papers which Isis’ authors have profitlessly consulted explicitly stress that air’s bending of horizontal light renders high by 6/5 the C gauged by lighthouse-flame-visibility, while the same air-refraction will make C obtained by timing sunsets (at different terrestrial heights) come out 5/6 low — the double-sunset method.

See D. Rawlins, “Doubling your sunsets or how anyone can measure the earth’s size with wristwatch and meterstick,” Am.J.Physics, 1979, 47: 126-128, p.127. Cited to discover Rawlins for years in the 1990s in the well-known textbook, Hallday, Resnick, & Walker, Fundamentals of Physics, as its kickoff example of applied science, illustrated by diagram (plus frontpiece sunset-photo). See also J.Gerver and Rawlins in Scientific American 1979 May. But uniformly silent Historians-of-science will not so much as admit the existence of the air-ray, or even that its existence is 5/6 low. All the three Rawlins papers which Isis’ authors have profitlessly consulted ‘prove’ said precession from the fabrications: [2] [B2].

Further, DR’s 1994 adopted +159 epoch (Rawlins 1994L, fn 45: all 12 Clean Dozen stars with no deletions) was understood in 2011 for the first time to establish no residuals-overlap (fn 7) in the Clean-Dozen-vs-SickSix split, a finding nowhere cited in Brandt et al 2014B, which keeps mis-rendering DR’s also-uncited +159 date, though it was repeatedly, www.dioi.org/bjr1g.pdf, www.dioi.org/jau8q.pdf, put to BZJ. As is obvious from Brandt et al 2014B’s Figs.7&8 (C-O) 43 star-residuals’ proximity to each other is time-dependent. E.g., the residuals of Aloith and Aldebaran are 18° apart in +128 (Ptolemy group L date of Brandt et al 2014B’s Abstract, Table 2, and pp.332&334) but are within 2° of each other in +159. Indeed, as DR’s 2014/8/26 ref report noted, if we eliminate none of the Clean Dozen and run a bivariate least-squares on them exactly as they stand, the solution is $E = +159\pm 9^0 \pm 4^0 \pm 2' = (L = 31^0 \pm 2': Alexandria) and the extremest residuals are symmetrically within 10° of zero: Betelgeze +10° & Pollux -10°. For contrast, one may examine the results of applying, to the Sick stars, the very same test just done on the actually higher than that of the non-eliminated stars Altair, Castor, Pollux, & Regulus. So the final published version (p.332) expanded the justification for eliminating the Unhelpfuls to include that their zero error occurred later than 200 AD (a criterion eliminating Altair [p.334 vs p.335&Fig.8], though it was widely obtained as a number of additional 800 stades (by a daily grid by L) which barely overlapped (fn 22) — before settlement upon the nal versions of L&E? This further hint of arbitrariness is one of the factors vitiating the paper’s lengthy, impressive-appearing search for groupings of stars different from the skeptics’ simple acceptance of the Conjunctions’ own groupings. In the final paper, no correction occurred for the above-cited ref-noted fact that Aldebaran was eliminated from Brandt et al 2014B, though, again, it was moving faster in declination than non-eliminated stars: the selection of the Unhelpful Threesome was published unaltered at p.332. (The paper’s last version of groupings [in Fig.10] restored Aldebaran while booting Altair & Castor.) More important than these errors is the general misconception that slow declination motion is ground for dismissal (even while Brandt et al 2014B believes it is looking for latitude “accuracy”) — these are the very stars that least-flexibly measure latitude-error. Real bivariate investigation would know that and would know that getting something right cannot occur without simultaneously doing likewise for x, since all the correlations are non-zero, and some are non-trivial.

43 But labelled “O–C”! The various Brandt et al 2014B Figures confuse O–C (Observed-minus-Calculated) with C–O (evidently a routine weakness among historians-of-astronomy [though here fortunately harmless], e.g., [2] [F8], & www.dioi.org/if.htm#bvmn). And Brandt et al 2014B’s eq.1 (p.331) is founded upon a confusion of errors with residuals (possibly just a misunderstanding of the Rawlins 1992 ms’ eq.2), thereby equating Observed-minus-Calculated with what is actually just Calculated. If taken seriously, this makes Observed equal to twice Calculated.
Table 1: Ancient Observers’ Epochs, E, Adopted and Actual Geographical Latitudes L

<table>
<thead>
<tr>
<th>Obsvr</th>
<th>$E \pm \sigma_E$</th>
<th>Adopted L</th>
<th>Its Error x</th>
<th>Actual L $\pm \sigma_L$</th>
<th>$\sigma_0$</th>
<th>$\sigma_1$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Timocharis</td>
<td>302°±0.08°</td>
<td>31°12'</td>
<td>$+1.5^\circ \pm 1.1^\circ$</td>
<td>31°10'.5 $\pm 1.9^\circ$</td>
<td>±6.1'</td>
<td>±5.9'</td>
</tr>
<tr>
<td>Aristyllos</td>
<td>258°±10'</td>
<td>31°15'</td>
<td>$+1.0^\circ \pm 2.7^\circ$</td>
<td>31°14'.0 $\pm 2.7^\circ$</td>
<td>±6.1'</td>
<td>±4.2'</td>
</tr>
<tr>
<td>Hipparchos</td>
<td>-131°±05'</td>
<td>36°08'</td>
<td>$+0.2^\circ \pm 1.2^\circ$</td>
<td>36°07'.8 $\pm 1.2^\circ$</td>
<td>±5.2'</td>
<td>±5.0'</td>
</tr>
<tr>
<td>Anonymus</td>
<td>159°±09'</td>
<td>31°15'</td>
<td>$+4.4^\circ \pm 2.0^\circ$</td>
<td>31°10'.6 $\pm 2.7^\circ$</td>
<td>±6.0'</td>
<td>±5.6'</td>
</tr>
</tbody>
</table>

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

C16 In 1994, 12' later than 1982, DR discerned a new method for finding each observer’s assumed geographical latitude: from nulls in his data’s fractional-endings’ frequency-profiles (as explained in Rawlins 1994L §F) which, by subtraction of $x$, easily produces each observer’s absolute actual latitude $L$. All four least-squares-fitting $E$ and epochs $L$ (Timocharis 11 stars; Aristyllos, 6; Hipparchos, 19; Anonymous, 12), along with their standard deviations ($\sigma_E$ & $\sigma_L$), as well as single-datum standard deviation, raw ($\sigma_0$) and with the effect of rounding $^{39}$ removed ($\sigma_1$). All these desiderata are produced here in Table 1, slightly improved (see fn 38) vs the values of $\{3\}$ Table 2 above or Rawlins 1994L Table 3. Strangely, Brandt et al. 2014A, the refereed version of Brandt et al. 2014B, www.dioi.org/bzj0.pdf, claimed that Rawlins 1994L had latitudes “close to our values” — this, even though BZJ had to have this day solved for any of these latitudes. So www.dioi.org/jau8q.pdf, the DIO 2014/8/26 referee report, at pp.3-4, suggested that this point be clarified and that Rawlins 1994L’s $x$ values and absolute $L$ values for all four observers be printed, since BZJ had brought up the point, and had supplied data $^{41}$ of DR’s other numbers (the majority correctly) — preferably along with a sentence on the novel though-tricky method which DIO had invented while pioneering this entire line of inquiry. But, probably because DIO’s $x$ values especially & hugely disagreed with JAHF’s “accuracy” values, the published article did none of these things.

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

$^{38}$ The sole non-fit for the dozens of data in the nulls experiment was Timocharis’ Aldebaran. Rawlins 1994L fn 39 suggested that the original North Polar Distance may have been 81°15'/15, recorded (conventionally for unit-fractions) as 81°15', but later misrecognized (like $\{3\}$ fn 44) as 81° & 15 arcmin, thus $\delta = 8^\circ 33'4$, as at Almagest. 7.5. Thus, reconstructed true $\delta = 8^\circ 14'15$ or 8°56', which also shows a poor residual. And Arcturus obviously bears a $1^\circ$ scribal error; restoring the original and eliminating outsized-residual for Zubeneschamali (loppily-rounded $\delta = -5^\circ$), we have the Timocharis entry in Table 1 here. (For Timocharis’ results based on non-reconstructed data, see $\{3\}$ Table 2.)

$^{39}$Timocharis & Hipparchos used a precision of $p = 12$ intervals/degree; for Anonymous, $p = 8$; Aristyllos, $p = 4$. The inverse of $p \cdot \sqrt{2}$ is the rms of the effect of average rounding, in degrees.

$^{40}$ We thank Jack Brandt for rightly urging use of modern satellite-based star-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.

$^{41}$E.g., at Brandt et al. 2014B p.331, for all 3 observers, our 1982 ms’ epochs $E$ & $\sigma_E$ are relayed, conspicuously omitting our $x$ & $\sigma_x$.

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

38 Cohen email to DIO 2016/9/27: “Never ever is Isis going to publish a paper which already in its very first sentence . . . contains the phrase ‘smothered by a chauvinist battery of destructive, data-disregarding — even data-fudging — papers’.” (See [3] p.46 below.)

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

40 History-of-science archons’ principledness glimpsed: www.dioi.org/j139.pdf; DIO 1.2 fn 172.
2 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 Ptolemaist O. Gingerich’s various past slanderous referee reports upon our work, naturally requesting removal of anything embarrassing to his clique, even offering to take another later look at the paper (fn 35: “If . . . DR revises . . . I would be happy to look it over.”) to confirm that the censorship he was ordering had been satisfactorily carried out. So our 2015/9/30 resubmission added extensive notes, responding to such typical intrusion by reviewing — at least for JAHH’s info — the long, revolting history of such stifling of open discourse, but giving JAHH permission to delete these or anything else it thought inappropriate, with our encouragement at the prospect of such helpful assistance. In reaction, JAHH has followed the Journal for the History of Astronomy in permanently severing communication with DIO: suggesting, as later confirmed, www.dioi.org/ow3l.pdf, that JAHH’s initial request for cuts was made in vain hopes (encouraged by years of Gingerich-circle slander of DR) that DIO would adamantly refuse revision or edits, thus killing the paper without JAHH being indictable for censorship. DIO instead agreeably refused to fall into that trap; thus, fleeing was JAHH’s only escape-option to effect pre-ordained rejection. The 2015/9/30 version follows, very slightly enhanced.

ABSTRACT

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

A INCURABLE DENIAL OF THE UNDENIABLE

Claudius Ptolemy’s Almajest is the central document (§1 below) of our valued heritage from classical antiquity’s mathematical astronomy. Though Ptolemaic-deponent’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 Serapic state religion at Canopus (near Alexandria) where stood its major temple, which specialized in curing illness through astrology&dreams. Ptolemy also authored astronomy’s bible, the Tetra-biblos. His employers no doubt appreciated his consistent demonstrations that reality was in perfect accord (§M2) with divine celestial theories. However, for centuries, historically savvy astronomers have known that this famous 2nd century AD astrologer-geocentrist mathematician accomplished said illusion by indoor-faking or plagiarizing all his allegedly-outdoor “observations” of celestial phenomena, to force precise accord with indoor mathematical models (some of which had already existed for centuries) and tables computed therefrom. Ptolemy’s deprivations even included stealing and mis-precis-ing 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 then threw out Ptolemy’s fakes & was thus able for the 1st time in history to predict star-positions — to ordmagm 1° accuracy. — 100° in advance (ibid Table 23: 100 select stars for 1701.03). But a few invincibly innocent & deeply committed (§B2; fn 11) archetypical-historians-of-astronomy keep intermittently trying to breathe life back into their longstanding tradition — e.g., Neugebauer 1975 p.284 & Pedersen 37Note problems at fn 42 & esp. fn 45 if done otherwise. Monovariate tests might successively minimize the squares of the residuals by finding the e that does so, then the x that does, etc, whittling S into ever-smaller remoteness from minimum, at each cycle. This would be the cumbersome, tediously-iterative serial-monovariate approach. But why not just elegantly solve x&c for a-swoop (with trivial iterativity from non-linearity), with total exactitude, through true simultaneous bivariate least-squares — as was done back in 1982&1994, and here in Table 1.
C9 The erroneous figures for “accuracy” \( r_s \) in Brandt et al. 2011 and Brandt et al. 2014B were an ordnary too small, presumably because they were mistakenly found (as hinted at in Brandt et al. 2011) by [a] searching monovariously for the \( E \) that minimizes the sum \( S \) of the squares of the residuals, [b] subtracting the subsequent mean 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 is easily seen: when Brandt et al. 2014B p.331 puts the “accuracy” of Aristyllos & Hipparchos at 0.3338, it signifies the 14\&11 ARCSCONDS resp — obviously a fantasy (as BZJ were warned of this on p.4 of www.dio.org/gauk.pdf, DR’s invited referee report), considering that the data’s rms is admittedly 0.1 on the same page: Brandt et al. 2014B Table 2. (Equally incredible: idem lists rms values for methods of Maeyama, Rawlins, & BZJ — that agree with each other to a 1000\(^{th}\) 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

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\(^{34}\) 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.l, the coefficients of the unknowns are 1 and 0.3338\( \cos \alpha \). The rms value of the latter in these investigations is about 1/4, so the standard deviations for \( x \) and \( E \) should exhibit a ratio of about 1 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. 2013A (abstract) or Brandt et al. 2013B. (Note that the BZJ referee [2013 admiringly takes no part in evaluating anyone’s \( x \) — or anything at all about Ptolemy.)

\(^{35}\) BZJ’s initial abstract, www.dio.org/bzj11.htm, Brandt et al. 2011, gave figures for “accuracies” (where we flip BZJ’s unconventional C—O sign): 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.4, show different \( S \)s. Timocharis: \( E = 0.012 \), Aristyllos: \( 0.003 \), Hipparchos \( 0.004 \), Ptolemy \( 0.009 \). Our speculative reconstructions (via \( \alpha \)'s [c]-[d]) alter the experiments but (in a delicate problem) get agreements with some of 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 \( \Sigma m_r \), to show that most BZJ solutions do not approximate it.]

Timocharis 12 stars: \( E = 295 \), \( r_s = 0.022 \), \( S = 2745 \). \( E = 0.277 \pm 0.18 \), \( S = 0.076 \pm 0.07 \), \( \Sigma m_r = 2442 \). \( A \)ristyllos 6 stars: \( E = 258 \), \( r_s = 0.003 \), \( S = 147 \). \( E = 0.258 \pm 0.10 \), \( S = 0.016 \pm 0.045 \), \( \Sigma m_r = 147.4 \). Hipparchos 17 stars (Aliothe \( \delta = 67 \)°35′): \( E = 128 \), \( r_s = 0.004 \), \( S = 446 \). \( E = 0.133 \pm 0.013 \), \( x = -0.001 \pm 0.021 \), \( S_m = 392.5 \). Ptolemy 18 stars \( E = 111 \) (Brandt et al. 2014B Fig.5 no-prop-mot), \( r_s = 0.005 \), \( S = 2539 \). \( E = 1.115 \pm 0.13 \), \( x = -0.004 \pm 0.052 \), \( S_m = 2521 \).

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 monovariable least-squares — and not even via calculus: 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-monovariable 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 monovariable 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 \( S_x \), Maeyama 1984 Table 1 column \( x \) lists \( r_s \), the mean error of a single observation. The resultant errors range as high a factor of nearly 7 (the Hipparchan 44-star sample).
had they faked them by adding $2\frac{2}{3}$ centuries worth of his false 1°/cy precession, namely, ticking 2°40′ onto all Hipparchos’ stellar longitudes. (Closely agreeable date, assuming the star-declinations were faked similarly.) No mention of this Coincidence in Brandt et al 2014B — though urged by referee DR. It has been explicitly well-known for over a century that virtually the same date matches the date for which Ptolemy’s 1025-star Catalogue’s fakes would seem correct, were they real, which few scholars believe anymore. E.g., Peters & Knobel 1915 p.15 noticed that +58 is the date when Ptolemy’s misprocessed 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 Newton’s 2011 recommendation. This positively belies the paper’s p.333, where we instead hear that besides his star-declinations (empth added): “Ptolemy offers additional evidence for his [false] precession” value elsewhere in the Almajest (e.g., [Toomer 1984 p.338]) — innocent of the A-Jones-witnessed fact that upon viewing Grafhoff 1990’s evidence decades ago, Toomer agreed that the Catalog stars came from Hipparchos — which renders irrelevant the faked “Ptolemy evidence” cited to Toomer 1984 loc cit. The DIO ref-report asked: “So are we also to ascribe the Catalog to the same secret observer, who thus must have created a catalog of over 1000 stars though no one ever mentioned his or its existence?” [D] For the δ data Almajest 7.3 gives for Ptolemy’s era, there is (in residuals) a fully clean split (perhaps unknown before DR’s 2011 letter and 2014 ref report to BZJ), between the suspect 6 star-declinations δ which Ptolemy analyses, and the unsuspect 12 data (which he doesn’t analyse in 7.3) — if one adopts the independently-arrived-at epoch Ε (+159) and geographical latitude-error $x$ (+4′) already found through bivariate analysis (Rawlins 1994A) upon the unsuspect data, without any selection among or tampering with these 12 data, long-separately-recognized and separately treated by Ptolemy. Significance of this for Brandt et al 2014B’s peculiar new split is emphasized below at §C21.

C6 JAHH readers have a right to know §C6’s four central considerations, but Editor Wayne Orchiston (WO) has not felt the need to inform30 them. So much for the integrity of the paper’s Ptolemist conclusion. And of the Journal of Astronomical History & Heritage. C7 Brandt et al 2014B p.331 claims that its analysis is a bivariate repeat of DR’s 1982 analysis but (as warned in www.dioi.org/jau8q.pdf, the 2014/8/26 DIO referee report on the paper) it really was just a try (like Maeyama 1984) at solving a bivariate problem monovariately. Except for Ptolemy (where different samplings53 obviated a comparison), the resulting epochs $E$ and their standard deviations are mostly about the same as those of DR 1982, after the standard deviation of Aristyllos’ epoch $E$ was fortunately brought into near-agreement with DR’s recommendation, www.dioi.org/jau8q.pdf, before Publication (compare www.dioi.org/bzj0.pdf, vs www.dioi.org/bzj.pdf, for Aristyllos). A peculiarity of Brandt et al 2014B is that values for $x$ & its standard deviation $\sigma_x$ are not given anywhere: not even when BZJ attempts recounting and repeating Rawlins’ analysis (which explicitly supplies and tabulates both $x$ & $\sigma_x$). Instead, BZJ present miscalculated ($\sigma_C$) values for a single entity, “accuracy,” which they confusedly seem to regard as sufficiently equivalent.

C8 The DIO referee report warned53 that the ‘accuracy’ values given at Brandt et al 2014B p.331 are “astonishingly low”.

30The debate’s existence is mentioned, but without the key indicting details. Perhaps the authors & editor feel that their admirably full and neutral bibliography suffices to provide representation for dissent. BZJ’s 2014B excuse for silence in the text (which is all most readers see) on the undeniably indicated items listed above at §C5.

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

32Slightly true also of Hipparchos, where Rawlins 1982G & Rawlins 1994L included two stellar declinations from non-Almajest sources, a supplement 14 suggested by H.Vogt.

33BZJ were helpfully provided sufficient advice to inspire proper caution: [1] were given all the right answers for $E, x, \sigma_x$ and both’s standard deviations, [2] were told that their own values for “accuracy” looked remarkably too small, & [3] were repeatedly warned not to do the problem monovariately.
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 & Arystyllus (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\(^5\) 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 JAHJ (through the referee-report, which is a 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 Almagest 7.3, Ptolemy ignores the reliable data of his own time and instead uses an unknown’s data from a century past! — without mentioning it.

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

\[
\delta = L \pm Z
\]

(minus-sign for southern transit, plus-sign for northern upper transit, where \( Z \) complements altitude \( h \): \( h + Z = 90^\circ \)), so \( L \)'s error carries directly, additively, fully into the \( \delta \) data, the systematic error of whose mean is therefore the error of \( L \). This would seem to be obvious, but the 1st researcher ever to perform the test upon star data, to show contraven-tionally the admirably small error in ancient star-observers' \( L \)-error, was DR, for the History of science: Rawlins 1982G. From the \( \delta \) data contemporary with Ptolemy, all analysts since (including Brandt et al 2014B) have concluded that there is but tiny error (ordinarily \( L \) in the observer’s assumed geographical latitude \( L \)). So it should not be hidden from the reader (as it is, throughout Brandt et al 2014B) that when Ptolemy reduces transit data (via eq 1), he uses an Alexandria \( L = 30^\circ58' \) (Almagest 5.12-13), which rules him out as the declinations’ observer since this \( L \) is in error by \(-14'\) (Alexandria being at \( L = 31^\circ12' \)).

[C] Some of the star-declinations allegedly observed by Ptolemy (c.+160) are so bad that Brandt et al 2014B p.332 invents a hitherto-unknown observer for them at 57 AD.\(^{29}\) But that date for ibid's Lone Mystery Observer (\( \pm 2 \) fn 37) just-so-happens to be within \( 1^\circ \) (!) of the shortfall-date that the “Ptolemy” Catalog’s stars would end up at (§B6 item [2]), else say that DR was (ibid) merely “interested in checking” the latitude-errors? — as if the discovery of these had been around for years. Why, throughout, is Maeyama 1984 usually cited ahead of DR’s earlier 1982 works, when both are mentioned? It seems especially strange to find DR’s unambiguous priority, in computing separate dates for Timocharis & Aristyllus reported thusly at Brandt et al 2014B p.334, www.dioi.org/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 comes even harder to explain when we find that the earlier, refereed (otherwise nearly identical) version of the paper, www.dioi.org/bzj2.pdf, has the verbartim-same writing except for the citations, which are simply chronological back then: “Rawlins (1982, c.1983, 1994); Maeyama, 1984.”


29 Were there a case for a \( +57 \) observer, the most tempting identification would be Heron, who recorded an Alexandria-midnight \(+62/3/13-14\) 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: §B6.

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#okz), this advice was indisputably a “dumbed lie.” (So claimed patient, sedate, judicious [Schafer 2002 p.40]) Hoskin in his 1983/3 letter to hothead-horribilis [idem] Rawlins, announcing his exile from JHA, while suit-threateningly rejecting Rawlins’ gentle mathematical criticism of “impossible” (fn 35) to the head of the American Astronomical Society, urging supervision of its shamelessly stunning Historical Astronomy Division (H.A.D.), but the unprofessionalism of 2002 has only worsened since. History-of-ancient-astronomy’s lengthy communal monopolistic and craniolicth insistance upon defensively maintaining — by character-assassination (fn 5) & the threat of exile (idem and fn 1) for dissenters — an evidence-defying, perception-inverting ([N]), logic-loutting ([M]), hopeless, honest-Ptolemy-myth as its ultimate herd-sacred tenet, can only weaken the eld’s cred. Same referee-report, which point in a direction other than its inexplicable Ptolemaic conclusion.

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refereeing (fn 3) but is alert as can be in not permitting any author to argue in its pages that Ptolemy was a liar, even while allowing the defense clique to contend he was not (Evans 1993 p.145; Rawlins 1999 §§F7-F8). That the controversy has been ripe with “unprofessional” acts, such as shunning, has been nationally published (Schafer 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 stunning Historical Astronomy Division (H.A.D.), but the unprofessionalism of 2002 has only worsened since. History-of-ancient-astronomy’s lengthy communal monopolistic and craniolicth insistance upon defensively maintaining — by character-assassination (fn 5) & the threat of exile (idem and fn 1) for dissenters — an evidence-defying, perception-inverting ([N]), logic-loutting ([M]), hopeless, honest-Ptolemy-myth as its ultimate herd-sacred tenet, can only weaken the eld’s cred. Same referee-report, which point in a direction other than its inexplicable Ptolemaic conclusion.

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will merely (!) require the opening of discourse\(^2\) and minds.

\textbf{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.” \textit{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 ancient who 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 shrunkanalysed as paranoid by one who thinks the whole universe is run by an invisible mind and who publicly calls on “our Lord Jesus Christ.” A world where sanity is insanity and vice-versa. With respect to its obvious duty to return the Ptolemy controversy to a scientific basis — above politics, smearing, slanging, religion, 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 is and isn’t reliable, \textit{Skeptic Inquirer} Editor K.Frazier asked him to debate Rawlins on Ptolemy in \textit{Skho}; Gingerich (1978/2/2) explained his refusal by calling Rawlings “exceedingly paranoiac” 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 Rawlings, 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 sandlerae — accuse someone of insidiousness when he himself was even WISELY (Gingerich, p.258) to 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’ secret silence on his secret actions, some even going so far as to supplant his personal permission, before publishing any data he has received!) Gingerich in a 2000 referee report to \textit{Isis} on an article (Thurston 20005) appreciative of Rawling’s inductive successes, typically promoted freespeech-in-theory while inserting an element aimed at ensuring that heretics’ Irresponsible abuse of freedom would be properly recognized as what only could issue from a disordered mind: “To say that the paper should not be published will only fuel the paranoic claims of the small [fn 1] group fighting Ptolemy, who believe that a cabal of [Neugebauer] ‘muta’ [sic] are present, and has been taken from being aired in the standard journals.” \textit{Hesperia} 5, Rawlings received another paranoid’s letter regarding said zany’s own recent book-review (emphasis added): “So far the Neugebauer camp has not been heard from. Perhaps my merely mentioning [R.]Newton in a review of Neugebauer’s has placed me beyond speaking terms.” (See www.dioi.org/pm1.htm.) And who was this fellow-paranoid, who imagined a closed Neugebauer “cabal”? No other than O.Gingerich! In the midst of decades of documented — ah — “inconsistent” character-assassination fantasy, Gingerich adds one more fantasy, soberly describing himself as “a practicing Christian”: 1978/2/2 to Frazier. Obviously, being religious doesn’t mend vendettas. We recall gullible or cynical saint-mystilosopher Cardinal John Henry Newman’s politically dept holy war on straightforwardly ethical independent idolatrous Cl. Kingsley; at Newman’s 1890 death, after the standard public eulogy, Cardinal Henry Edward Manning privately mourned his colleague thusly: “Poor Newman. Poor Newman. He was a great man.” \textit{Eminent Victorians}, 1918, Manning chapter, end of part 9.)

\(6\) \textit{DIO} has long had a standing offer to publish debates, with quite novel rules, www.dioi.org/deb.htm, scrupulously designed to ensure fairness. [Except for inspiring a contemptuous joke from A.Jones, these rules — and \textit{DIO’s} generosity in offering debate-space — have proven of no interest to Mufosis.] 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.

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

\textbf{C Latest Into the Lists}

\textbf{C1} As to the ancient star-declinations issue: what is history-of-science journals’ record? Well, both \textit{Centaurus} (in 1982) and the \textit{Journal of Astronomical History & Heritage} (in 2014) received competing solutions to the \textit{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.

\textbf{C2} Brandt et al 2014B is the most recent attempt to exonerate Ptolemy, arguing that the fact that among \textit{Almajest} 7.3’s star-declinations (§C5) Ptolemy faked data. Further, both journals refused to acknowledge the content of subsequent communications demonstrating their folly.

\textbf{C3} Of \textit{Almajest} 7.3’s 54 star-declinations (§C3) reported by 4 ancient observers, BZJ’s 2014 project examined 53: Timocharis 11 stars, Aristyllos 6, Hipparchos & Ptolemy 18 each. These data had already been studied by Pannekoek 1955 (1° to appreciate the accuracy), R.Newton 1977; also Rawlings’ 1982 bivariate least-squares study, which received another paranoid’s letter regarding said zany’s own recent book-review (emphasis added): “So far the Neugebauer camp has not been heard from. Perhaps my merely mentioning [R.]Newton in a review of Neugebauer’s has placed me beyond speaking terms.” (See www.dioi.org/pm1.htm.) And who was this fellow-paranoid, who imagined a closed Neugebauer “cabal”? No other than O.Gingerich! In the midst of decades of documented — ah — “inconsistent” character-assassination fantasy, Gingerich adds one more fantasy, soberly describing himself as “a practicing Christian”: 1978/2/2 to Frazier. Obviously, being religious doesn’t mend vendettas. We recall gullible or cynical saint-mystilosopher Cardinal John Henry Newman’s politically dept holy war on straightforwardly ethical independent idolatrous Cl. Kingsley; at Newman’s 1890 death, after the standard public eulogy, Cardinal Henry Edward Manning privately mourned his colleague thusly: “Poor Newman. Poor Newman. He was a great man.” \textit{Eminent Victorians}, 1918, Manning chapter, end of part 9.)

\(27\) Maeyama 1984 is graphical by trial\&error and is monovariate (§3 fn 100), finding nearly accurate values for \(E\), though with estimated (nonmathematically guessed) standard deviations \(\sigma_E\).

\(28\) 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 Rawlings 1982G was just based on guessed work not statistics), as suggested by their ultimate omission to state in Brandt et al 2014B that DR was specifically the establisher of the Timocharis-Aristyllos split (earlier presciently guessed by Neugebauer 1975 p.34), as well as of the whole concept of finding the 4 observers’ latitudes & latitude-errors from the data. Why else say (Brandt et al 2014B p.331) that DR “quoted” the Timocharis-Aristyllos dichotomy? (In fact, Rawlings 1982G split Aristyllos off from Timocharis and gave both astronomers’ dates, explicitly on the basis of star declination studies [calculated \& tabulated in the unpublished 1982 ms], adding that the novel finding that all five precise ancient Greek star-catalog stars showed that their observers knew their geographical latitude \(L\) to ordmag 1°. See \(\text{C14 below.}\) Or why-
to $\Delta \lambda = -29' \sin \lambda \tan \beta$, which gets substantial in the north. In the star catalog no such $1''/2$-amplitude wave, so we neither did an outdoor star-collecting Ptolemy. Of course, ever-openminded Ptolemites robo-countered this new shocker with their usual standard-weapon: scientific inability so truly embarrassing as to raise the question of whether impenetrable Ptolemitism has become a medical problem. Exhibiting the science-grasp of Ptolemy’s fellow-crank-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] longitudinal conversions to great-circle measure entails multiplication by $\cos \beta$, thus gt-cire $\Delta \lambda$ can’t exceed 29’. Swerdlow’s response: he hides (DIO 5 §[B4]). Meanwhile, Evans’ attack on the (§B5) absent-error-waves argument confused sine waves with cosine waves ($\pm \sin \theta$) blowing off a 65° phase-difference with: “the phase is not exactly right” (Rawlins 1991W fn 152). All this to obscure that he can’t find a cosine wave in the Catalog’s zodiac stars, with amplitude even 1/3 as large as the 1.5/2 he sought.

B6

In the 1987 JHA, Evans took its 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 found 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 1º 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 1º. 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. Conman 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 (near-verbatim: Evans 1998 p.259), Evans conveniently forgets to discuss that 1981 eclipse at all (switching instead to adding a previously unmentioned 1777 eclipse he hadn’t outdoor-measured with) — and continues inedueably concentrating for ancient errors of ordmag 1º. [2] Ptolemy claimed (Almajest 7.4) to have outdoor-observed all 1025 stars of the Almajest’s Ancient Star Catalog. R.Newton 1977 proved that Ptolemy had instead stolen the...

\[\ldots\]

8 Demonstrating how one earns a MacArthur in certain sub-fields, the principled Dr.Swerdlow has not only called R.Newton dishonest and kook (§B2), he has additionally published a Joe-McCarthyesque incompetency-accusation against him (quoted verbatim at R.Newton 1991 §E2), claiming lots of thesis-gutting mathematical errors by Newton here-in-his-hand without giving a single valid example. (Check the revealing cavil-count context data carefully detailed at ibid fn 6.) And the Journal for the History of Astronomy sister-scape applying its proud name-branding at R.Newton as an honest and reliable scholar at issue is the ideal choice to carry on the JHA tradition of spotless integrity. Given such a heavy longterm 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 shrunkin’ chauvinist...

9 Ptolemites’ bunker is here described as not shrinking but shrunken, since for years now it’s been composed of only the hardest cases, by now auto-rejecting all evidence showing they were extensively, viciously wrong. (Like those who’ll be annually fantasizing forever in Ferguson.) And all will die 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-rereeeealy — for either 1974 Pedersen or JHA Editor Evans. Each could always be counted upon to create (check the revealing cavil-count context data carefully detailed at ibid fn 6) an error-wave-ridden imbroglio: he hides (DIO 5 §[B4]). Meanwhile, Evans’ attack on the (§B5) absent-error-waves argument confused sine waves with cosine waves ($\pm \sin \theta$) blowing off a 65° phase-difference with: “the phase is not exactly right” (Rawlins 1991W fn 152). All this to obscure that he can’t find a cosine wave in the Catalog’s zodiac stars, with amplitude even 1/3 as large as the 1.5/2 he sought.
null
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 (C-I & K-L) of our proofs of Ptolemy’s dishonesty hereabouts (a display —

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 Swerdrow, rejected the all-too-obvious explanation for Ptolemy’s rigged 140 AD solstice with two imaginative excuses:

The 1st was misconceived at a juniorhighschool level. The 2nd was a clumsy fantasy.

Near a solstice, NS alleges it’s impossible to measure accurately the time of maximum height of the noon Sun, since from day-to-day it’s virtually not changing 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)19 has approved an argument implying that if we toss a ball upward and catch it 4 seconds later, a 9th-grader (or younger) can’t tell that it peaked at 2 seconds?

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

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

B1 Does it tell us something about the state of soft academe that the Journal for the History of Astronomy, the “premier”15 journal of its field, has for decades used pseudo-science and deception in the shall-we-just-say extrareeeeeeeemely peculiar cause of protecting the reputation of a fellow pseudo-scientist (and grant-cow), the ancient Greek astrologer Claudius Ptolemy? — known for centuries to informed scientists as the most notorious liar in astronomical history. JHA’s longtime #2 official, Harvard’s Astronomy 101 teacher & dept Disraeliesque trowel-wielder Owen Gingerich, has repeatedly billed16 this ancient faker and superstition-peddler17 as “The Greatest Astronomer of Antiquity”, claiming that all who question this eminently-sensible-to-him proposition are the insanes’ 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 (Almajest 3.1) typically disagreed with the real Sun by 1°1/2! — or a degree and a half, which is over FIVE TIMES the angular distance from the Sun’s center to its limb (edge) — thus, his report agreed with Hipparchos’ indoor tables to within 1° or a fraction of an

16 Gingerich 1976, Gingerich 2002. Discraeli (L.Strachey Queen Victoria 8.3, 1921 [Harbrace pbk p.244]): “when you come to royalty you should lay it on with a trowel.” Gingerich summed-up succinctly: “DIO 2, 16 to 2.” To enjoy Rob’t Peary’s like supplications, see www.dioi.org/pm1.htm#ibdv.
17 Ptolemy authored astrology’s bible, the Tetrabiblos, the tripe in which he has to be (but usually isn’t) read if one wants a measure of how seriously to take Ptolemy as a scientist. See Rawlins 1977 pp.70-71&79 for analysis of the book’s escape-hatchery, plus a fundamental Ptolemy gender-mis-calculation (ibid p.69) which has escaped detection & thus survived for millennia, persisting even in today’s horoscopes, e.g., those on sale in our grocery-store-checkout-counter literature-departments. Psychologist Gingerich already intended-to-be-anonymous 2000 referee report to Isis (outed in Rawlins 2003X) called Ptolemy-skeptics just a tiny bunch of paranoids — thereby inadvertently and delusionally smearing most of the scholars in the field, even WHILE he is echoed in the 2015 JAH! referee report’s complaint that DR doesn’t respect which to claim. Can it get any weirder? Well, actually, yes. As we see from www.dioi.org/pm2.htm, a secret Gingerich referee-report slander of DR’s character (so relevant to the subject paper’s mathematical development!) on a basis which Gingerich had forgotten he’d already revealed, at www.dioi.org/pm1.htm, applied to himself.

(On the reality of cohesive shunning of Ptolemy-skepticism: see §3 fn 6.)

(errors displayed at Thurston 1998A Table 1): the mean of the error-sinusoid that best fits the tables, and thus very closely fits the “observations,” exceeds a full degree: −65°. (See formula at ibid §1.) This, though naked-eye solar means can be made to ordmag 1° (fn 47; Rawlins 2018U §B4). Either Ptolemy’s genius was so preternaturally refined that he could look up into the sky and (fn 18) see objects’ theoretical positions instead of their actual positions16 or (§B2) he has broken the law of empirical science by pretending to base theory on outdoor celestial “observations” actually computed, indoors on the sky, from or to fit the very same theory (Thurston 1994P §D & Rawlins 2002V §§B3-B5), such fakes then used to “prove” said theory, a (literally) preposterous sham-process as Delambre saw: §B2.

To appreciate the grossness of the illegality here, consider its sheer enormity (as emphasized, e.g., and planetary contexts throughout R.Newton 1977): for his 3 equinoxes, Ptolemy is alleging 1°-hand visual sightings of the Sun’s center on the celestial Equator at times agreeing to ordmag 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°/cy. (A rate consistently bi-mis-copulated at CalTech’s Swerdlow 2010 p.152, item 3.) So: when is Ptolemy’s Sun correct? Obviously that epoch must be 137.547 + (100°/cy)65°/−23°(cy) = −145, which (as seen at Rawlins 1991W §M6; similarly below at §D5) is Hipparchos’ era, and is indeed the regnal epoch (Ptolemy VII Physkon Year 1) of — and the time of creation (Rawlins 2018U §O) and launch of — his Prime solar orbit (dubbed “PH” at idem). How can Ptolemy’s defenders expect to credibly deny that he plagiarized from Hipparchos, when Ptolemy’s Sun and thus his entire longitudinally contingent celestial system (not just the Sun but the Moon, planets, and stars) is correct only for the time of Hipparchos?

D4 The Hipparchos-Ptolemy tables’ −65° mean solar longitude error at 137 AD is well known, but the following revealing point isn’t: the errors of Ptolemy’s “observations” also mimic the Hipparchan PH solar model’s big 0°-4 amplitude annual periodic error. (Again: see error formula for Ptolemy’s Sun at Thurston 1998A §C.) For Ptolemy’s mimicry of not just systematic but even random Hipparchan error, see the glaring case of Arcturus: fn 37. D5 This unsubtle echo connects to the irony that Ptolemy didn’t fake the solar data via tables but by even cruder means. (He fabricated similarly elsewhere as well: Venus [§C1; Rawlins 1991W fn 166], the stars [fn 37], and the Arbelra eclipse [§G3]). As early as 1639 — the year the Ptolemy controversy should have ended (or been doomed to never begin?) — Christian Severin said Ptolemy had merely computed the alleged solar observations from Hipparchan data, and Delambre 1819 (pp.lxiv-lxxix) explained in detail how Ptolemy had fabricated his solstice and equinoxes by merely adding integral numbers of Hipparchos years to Ptolemy’s observations of solstice and equinoxes. (Discussion: Thurston 1994P p.58; and Thurston 1998A §§A.K.S.) For example (by method of ibid §2, reconstructing independent 139 AD Autumn Equinox) to recover Ptolemy’s 132 AD Autumn Equinox (Almajest 3.7), just add 278 Hipparchan years (365°1/4 − 1/300 each, 6° longer than has been claimed, e.g., in The Acquittal (§M). And in Hipparchos’ case, a ratio something above unity is to be expected since the tables were, after all, based upon his slightly flawed observations, of which he [contra some delusional Acquittal] reports several discordant with theory & each other: [§3 fn 8.

18 Ptolemy didn’t have to look skyward to see theoretical data instead of real: his Optics takes perfectly false-theory-accordant refraction angles that are erroneous by up to 2°1/2 or 150 arcmin! (See, e.g., Neugebauer 1975 pp.895-896.)
tantrum of the other non-US journal in this strange field: the Journal for the History of Astronomy, whose Editor Michael Hoskin, upon receipt of DR’s constructive criticism of JHA refereeing of one of its papers, responded with threat and correspondence-cutoff. DR’s criticism was supplemented by a polite, admittedly valid DR referee report: see the paper’s recomputation at JHA 1984 June, which happened only because the scientist author preferred honest accurate results, and cast seed upon stone by (1983/4/27) recommending DR’s refereeing skills to JHA. Likewise, when Curtis Wilson and Hugh Thurston insisted (e.g., Thurston 1995) on correcting a flock of JHA-unrefereed errors in Jones 1991H. In these and dozens of other instances (www.dioi.org/jha.htm/#hsbk) of serious DR-apprehended JHA-errors, that journal has never taken the initiative in correcting the situation, to produce accurate information — as if it really didn’t care at all about such trifling considerations.

A strict rule at JHA (at least vis-à-vis DR): if the erring author is no more honest than the JHA, then — no correction is made. (Prototype for JAHH 2015 and [11] Isis 2017.) EmbODYING a third of a century of the field’s proud progress:

[a] JAHH in 2015 exiles a referee who expended extensive time and labor to respond scientifically to JAHH’s S.O.S. for assistance in cleaning-up a paper which was beyond that journal’s technical capabilities, as thoroughly demonstrated below.

Also: some among the authors may have found themselves rushed or only partially consulted, and out-of-available space for their paper, as a deadline approached — and out of time, having waited too long to call in expert advice. (But this doesn’t excuse the central omission: Ptolemy’s – 14° error in latitude $L$, emphasized in www.dioi.org/jahh39g.pdf, DR’s 2011 letter to Brandt, for its fatal contrast with the trivial $L$-error shown by the data.)

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

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

A4 During the silent months that followed submission to JAHH of the paper that ultimately became §2 above, ever-cheerily-optimistic DR had entertained the possibility that WO was seeking a 2nd, more relevant referee. Finally, DR emailed a friendly 2016/2/29 letter, www.dioi.org/oww2t.pdf, to WO and learned of an arbitrary act which at last nakedly unveiled a proud new addition to JHAD covering-archonduum (§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-along intent to suppress DR’s paper for heresy, not style. I.e., he can’t in 2014 blame the JAHH’s gross failures of editing (& math!) on DR’s not-yet-written 2015 paper (expanded to §2 here), www.dioi.org/pm.pdf, which ultimately granted total freedom to WO. (See www.dioi.org/issa.pdf for Isis’ like achievement in time-disjunction.)

A6 We will shortly move on to putting JAHH’s 2014&2015 sellouts into the context of the disgrace of worshippers’ ever-more bullet-hole-ventilated half-century pretense that actual 365°.2425 year then) to Hipparchos’ — 1469/27 (00 Autumn Equinox (Alm 3.1), and one finds 132/9/25 13°46′. Ptolemy reports (Alm 3.7) 132/9/25 14°. All 4 of his solar data agree with such arithmetic, to the 5th precision he displays for each of the 4 — the precision itself a revealing farce, since in all cases based upon Hipparchian cardinal-point times 5-fold rounder: each expressed to the nearest 1/4 day. (For historically valuable detection of yet another Ptolemy solar longitude fake see Thurston 2002S pp.65-66 & fn 14.)

D6 We have now encountered the following telltale Ptolemy solar curiosities: [1] mean positional error exceeding a degree (§D1) for epoch 137 AD; [2] and null only (§3D) for Hipparchos’ epoch; [3] Ptolemy “observations” tightly (§D4) and overprecisely (§D5) theoretical not empirical. No matter how obvious the implications of items [1]-[3], each has received no 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 single simple theory that simultaneously, coherently, and fruitfully explains them: Ptolemy faked.

E PTOLEMY’S GEOGRAPHICAL LATITUDES:
MORE CONTRADICTION 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 $F$, to be obviously the Cape Verde Islands), uniformly rounded to the nearest twelfth of a degree. At GD 4.5.9 he gives 31°05′ for his religious home, the Serapic temple at Canopus. The Alexandria and Canopus values are each too low by 14°. No regular celestial observer — Ptolemy’s pretense (at, e.g., Alm 7.4) — can be this far off and not know it.

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

18 From chats with Ptolemistologists over many years, the following alibis are recalled: [1] Ptolemy’s ~65° solar error has been speculated as due to his having constructed his system before most or all of his “observations” (pretty dumb, since the system depended on the Sun: §D3), so he was stuck with the error and decided [see Ragep at §3 fn 9] not to 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 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 only for Hipparchos’ time has been explained by assuming that Ptolemy thought Hipparchos’ observations were better than his own putative outdoor observations, so (§D1) he instead reported Hipparchos-accordant data. (Was it honest to commit this appropriation? — and without saying so, which makes it a theft.) [4] See also rocks and asymmetrically unclear air at [L1] and fn 42, respectively. Would that the energy and ingenuity expended upon these 4 joke-fantasies had instead been applied to open-minded, undirected, exploratory research.

19 Rawlins noticed this unsubtle point (that no regular celestial observer could be 1/4 off in his adopted geographical latitude $L$ and not realize so) immediately upon entering the controversy. Thurston 1994P noticed something just as glaring back in the 1940s. How could Ptolemy-specialists,
Moreover, such an error (see math of 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 error 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.

Confirmation is achieved via statistical induction (Rawlins 1994L §F8) of the exact latitude L = 31°1’1/4 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 nothing to do with the Clean Dozen stars. Besides stealing them, Ptolemy-contemporary star data from Ptolemy’s era offered no scientific guidance at all (unless one delusionally regards shrinkoanalysis of the assumed era). These, however, show (Rawlins 1994L §E9) that the observer’s error 002, about 9 standard deviations distant from Ptolemy’s geographical latitude was 4.5.76 rounded 31° value — insisting on removal of anything embarrassing including accusing DR of an . (Similar disconnect for the 1025 celestial latitudes Alm 7.5-8.1 star catalog.)

Con rmation is achieved via statistical induction (Rawlins 1994L §E3) of the exact value (Rawlins 1994L §F3-F9) to the observer’s latitude L = 31°1’1/2 (ibid Table 3) — consistent with Alm 7.3’s dating of Ptolemy to epoch Marcus Aurelius 1 (160/161). Previously unnoticed vastness of Alm 7.3’s leapfrog anachronism: the Clean Dozen δ were observed c.160 AD (as just shown), and then merged with the Sickδ3,0 which were precess-faked so inadequately for stated epoch 137 AD that their coherent 5 stars’ mean is (fn 37) instead correct for over 100 years before 160 AD! There are two unknowns when analysing ancient star- declination lists: the observer’s epoch E and the error x in his assumed latitude. For the four observers whose star declinations are discussed in Alm 7.3, the curious paper, Brandt et al 2014B (discussed also in fn 37), gets mostly non-out results for epochs E yet for all 4 cases messes up the other unknown, x, the error in the observer’s assumed latitude. For the 4 observers, the figures given (op cit p.331) for the latitudes’ “accuracy” (which the authors compute instead of x) are: Timocharis 0°.72, Aristylos 0°.18, Hipparchos 0°.24, Ptolemy (?) 0°.3 — values which are oversmall by an ordmag. Likely-wasted-at-present wakeup 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 x, and thus matter in gauging the accuracy of the 4 men’s adopted latitudes for their observatories.

Along with §§C, D, and F, the star- declinations analysis proves that Ptolemy’s observship (or his authorship of the models he reports) is not established by the mere fact that some of his purported observations dated back to his time. Yes, Ptolemy clumsily double-lists L values for sites other than Alexandria (e.g., Helopolis-vs-On and Syene-vs-Elephantine: details at Rawlins 1985G p.260 and n.6). But none were the world’s cultural center he is supposed (by his defenders) to have been familiar with and from where he reports 1°-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, Aristylos, and (above, §§E2-E3) Anonymous. In light of such sloppy-copy, one can only admire Dennis Duke’s witty new translation of the Almajest’s Greek text, Syntaxis (§3 in fn 15), as: Cut&Paste. Not in Liddell-Scott-Jones. Yet but to a fellow politician, who despite “careful” reading could come up with no errors of science or history — or anything else — and thus (in stark contrast to DR’s ref report), offered no scientific guidance at all (unless one delusionally regards shrinkoanalysis as science), instead — even while acknowledging that DR is “clearly quite knowledgeable in the astronomical history involved” — insisting on removal of anything embarrassing to his clique, adding gratuitous psychological evaluations including accusing DR of an “apparent need to disparage those with different views”. This from a cult which has for a half-century repeatedly (and reliably-always behind-the-back) smeared, as insane, anyone differing from its own reality-detached view of Ptolemy — a genuine, mentally-disabling insanity from The Leader now at last the last almost alone in the usher with an (publicly) loyal J.Evans, J.Brandt, & possibly B.Schafer. (Ptolemy’s more well-known doubters — their consensus not at all well-known — are extensively listed here at §2 fn 1, though JAHH’s guardian [“referee”] is still stuck dreaming-on of a 1/2 century ago, in calling skepticism an extreme position: “worth hearing” he pseudo-tolerantly offers, even while continuing its suppression for a 4th straight decade at his JHA.) The ref added a death sentence to the paper, telling an editor who obviously wishes to stay on the good side of History-of-science’s Archbishop of TruthBury: “If this were my journal, I would not like to see this paper in it.” When JAHH supinely granted him full veto power over the paper, it had been determined that it was not going to appear in any form in JAHH. But censoring editors (& refs) are ever pretending not to, so JAHH’s initial tentative approach to exploring for an excuse for nonpublication was to find out if the durable myth, that DR would not accept editorial revisions, would suffice to dodge publishing archon-loathed heresy.

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A3. DR’s reply, www.dioi.org/owu8q.pdf, tried [A] to test whether demanding the paper’s softening was in hopes of making DR go away; and [B] to check out JAHH’s biased chummishness with its mentor (who has loathed and labelled DR for decades). So [A] DR unexpectedly refused to enter into any argument over content, granting full veto power to JAHH, instead of its 1st referee (as if there were a difference, as we learned). [B] The paper was expanded to provide information about the referee’s claque — vainly asking, www.dioi.org/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
A The Shy Archon Triggering the Present Paper: Politics vs Science

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

A2 Brandt et al 2014B p.332 claim that the 2nd century AD star-data of Almajest 7.3 “could have been taken by Ptolemy himself.” The evidence for this politically-convenient falsehood? Ptolemy was alive when they were recorded! — a fact which did not require a new publication for broaching, 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-alibiing pure-careerist like Brandt’s Puget Sound neighbor J.Evans as quotable Neutral Expert. (Brandt et al 2014B p.333: “The situation has been nicely summarized by [Evans 1998 p.2621].”) So DR responsibly submitted a paper, “Ptolemy’s Fraudulence” (§2 above), to the JAHH, whose chief, W.Orchiston (formerly established in Oztrollia, like JAHH, but lately transplanted to Thailand) turned it over not to a specialist in the relevant science

8 When asked to send Brandt his 3-decade-old star-declinations ms (later slimmed, revised, augmented with newly discovered absolute latitudes, and published as Rawlins 1994AL), DR took the time to profitably review his 1982-1994 conclusions, sending his further-revised 2011 thoughts in a letter, www.dioi.org/bij3g.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 Deepen & SickSix stars was overlappedly clean: §C17. (But Brandt et al 2014B didn’t cite any of this.)

As we mourn the passing of technically able contributors to 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.

9 See below at, e.g., §B4.

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 diaster of §C — the distinction being only that the Venus clashing-data-pair were faked while the Alexandria clashing-data-pair were plagiarized. Unwarily copying or mimicking others’ errors (e.g., §§D4-D5 and E1, fn 37; Bryce 2017A §§D2-D3) is the ever-lurking but ever-just pit that all plagiarists risk falling into.

E6 Also revealing of Ptolemy’s degree of empiricism is his astonishing listing of the Pharos (§E4) at exactly the same L (§E1) — 31°05’ — as for his home Serapic temple at Canopus from where he had only to look down the Mediterranean coast after dark to see those nautical mile (nmi) 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 159/5/17 (allegedly observed outdoors with Ptolemy’s putative armillary astrolabe). Using several Alm orbital elements for Mercury, Ptolemy mathematically derives the planet’s synodic longitude for each date. The mean synodic motion is then found by dividing the number of synodic degrees traversed during the interval, by that interval’s number of days. F2 But the difficulty for Ptolemy’s loyalists is this: his Canobic Inscription 1987), listed precisely the same Mercury mean motion, but most of the other elements differed. So: how could the same mean motion have been empirically and mathematically based for both works, if the respective derivations involved 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 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-alibiing pure-careerist like Brandt’s Puget Sound neighbor J.Evans as quotable Neutral Expert. (Brandt et al 2014B p.333: “The situation has been nicely summarized by [Evans 1998 p.2621].”) So DR responsibly submitted a paper, “Ptolemy’s Fraudulence” (§2 above), to the JAHH, whose chief, W.Orchiston (formerly established in Oztrollia, like JAHH, but lately transplanted to Thailand) turned it over not to a specialist in the relevant science

24 Check Rawlins 2008Q (§C1 & eq.23) for the Pharos’ sea-level visibility-distance, about 20 nmi, or 1/2 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-eyeballs of course assumes that our Greatest Astronomer even knew (or cared) which way is north. (Canopus today is Abu 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.R.Hess.)
foreign eclipse times couldn’t have supported the longitudinally-stretched geography ([G4] he borrowed (with credit)\textsuperscript{25} from Marinus of Tyre. Instead, Ptolemy’s vast opus provides (GD 1.4.2) but one\textsuperscript{26} example: two longitudinally much-separated reports — 500\textsuperscript{2} old! — of the famous Arbela — 330/9/20 lunar eclipse’s start, saying it was seen there at 23\textsuperscript{h} and in Carthage at 20\textsuperscript{h}, thus proving that the 2 places are 3\textsuperscript{h} or 45\textsuperscript{m} apart in longitude.

G3 However, Pliny earlier reported the same data very differently: 20\textsuperscript{h} (8 PM) for Arbela (modern embattled oil-city Irbil) and 18\textsuperscript{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\textsuperscript{3} show that non-astronomer Pliny was quite accurate, while The Greatest Astronomer of Antiquity was amazingly wrong, over 23\textsuperscript{h} off for Carthage, 3\textsuperscript{h} off for Arbela. The former error nearly equals the actual 21\textsuperscript{h}1/4 longitude gap between the sites, and the latter error far exceeds said query. But the weakest 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\textsuperscript{h} Carthage and 3\textsuperscript{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 appearance in Sicily, merely: moonrise (“exorini”). By contrast, the Arbeia time is given as the “22\textsuperscript{h}” hour after sunset, or about 20\textsuperscript{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, “secunda hora” appears nearer in the sense to “Sicilia” than to “Arbelam”. This obviously suggests that Ptolemy used Pliny’s or his source but (evidently unable to read Latin well) took Pliny’s 20\textsuperscript{h} time to be Carthage’s.

G4 But how did Ptolemy arrive at 23\textsuperscript{h} for Arbela? Since Gessellin 1790, it has been obvious that multiplication by an expansion factor (Diller 1984 §C5) had been applied by Marinus or Ptolemy or their source to a prior map’s accurate longitudes, creating the oversized longitude intervals of the GD. Rawlins 1985G eq.15 showed that the expansion factors were either 7/5 or 4/3 in the region under consideration. Assuming that the earlier accurate map correctly put Arbela 21\textsuperscript{h}1/4 east of Carthage, then expansion by 4/3 would produce 3\textsuperscript{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\textsuperscript{h} to 20\textsuperscript{h}, thus arriving at his fantastic 23\textsuperscript{h} time for Arbela.

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

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

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

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

\textsuperscript{26} Due to modern astronomical 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 eclipsicals as having to mean that (www.dioi.org/cot.htm#cknh) there then existed no empirical scientific community to be isolated from!

\textsuperscript{27} The Battle of Arbela was fought at nearby Gauamela and 11\textdegree after the eclipse. We find actual Local Apparent Times of the —330/9/20 eclipse’s umbral start: Carthage 17:43, Lilybaeum 17:52, Gauamela 19:56, Arbela 19:58. So the Gauamela—Lilybaeum difference in geographical longitude \(E = \Delta E = 2^{60^\circ}4^\prime\); Gauamela—Carthage, \(2^{13}13^\prime\).
4 Ptolemaic Enmity

Ptolemy-Defense Cult Lays Yet ANOTHER Egg On Own Already-Unwepably-Eggregious Faces ArchonBishop of TruthBury’s Trowel & Slander

We now analyse the latest installment, this time from the Journal of Astronomical History & Heritage 2014, in a half-century serial display of unfailingly invalid archonally apology-defenses of Claudius Ptolemy, sacrosant masoct-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 ([5A below]) just a mathematician who faked science. Badly. Among Ptolemy’s numerous clumsy Almajesta ([the silliest on scintillating display here in [2] were, e.g., [a] 2 different dates (37 days apart!) for the same Venus maximum-elongation, [b] plagiarizing Hipparchos’ star catalog, [c] impossible-for-regular-observer ignorance of his own city’s latitude, [d] 4 alleged solar “observations” which were (as no historian-of-science denies; or admits) many times nearer Hipparchos’ old indoor tables than to the outdoor sky’s actual Sun. The JAHH paper in question, J.Brandt, P.Zimmer, & P.Jones (below known as Brandt et al 2014B), attempted mathematical analyses of the Almajest’s stellar declinations, observed by four successive ancient Greek astronomers over nearly half a millennium. The journal and authors contend that the dozen Almajest-contemporary declinations could be Ptolemy’s observations, never warning the reader that zero evidence is provided to establish that claim, while simple, definitive, long-published, referee-urged proof to the contrary is below shown ([§C5) to have been deliberately omitted. The paper’s “bivariate least-squares” statistical analyses were not bivariate and thus didn’t exactly find any least-squares $S_0$ of residual-squares, as is also demonstrated below ([§C23). While observers’ epochs $E$ are nearly right (but not), attempts to find their geographical latitude-errors $x$ are revealed as grossly misguided, at a primitive level ([§C9&C12], though referee DR provided, ahead of publication, accurate $x$ (& standard deviations) for all four of the ancient astronomers being analysed, solutions which could’ve been (but weren’t) crudely verified by elementary arithmetic, as will be shown here (fn 34 or §C23). Our discussion’s bluntness derives from the fact that, though Brandt et al 2014B is politely written, its knowing evidential omissions cooperate in trying to grant eternal life to an establishment myth — Ptolemy as Great Outer 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 IHAH archons who long ago mistakenly decreed Ptolemy “The Greatest Astronomer of Antiquity” and thus have faces so at risk of megga-eggitudinal disgrace that they must forever encourage pseudo-science-for-The-Cause of forever-pseudoscience-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 mobagaba (though those interested only in 2014’s mishmash may skip straight to §C3), 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.

1 See Gingerich 1976 for 2 prominent examples of Believers (O.Neugebauer & himself) who got way too deep into worshipping Ptolemy as “the greatest astronomer of antiquity” ever to reverse and escape their own self-created trap of constitutional inability to admit error, and who consider their image of Authoritative Wisdom to be a more important consideration than [1] the field’s sanity or [2] ever doing justice to pioneer genius Ptolemy-exposer R.Newton, upon whom they are proud to have done their own pioneering, in smear-creating Newton as the field’s cohering hate-object (Gingerich 1990 p.364; Schaefer 2002 p.40) — before, since his death, honoring DR by elevating him onto the same pedestal.

rotates about the equatorial not ecliptic pole: as we can see from, e.g., the educational paper model Evans has helpfully disseminated, so (Rawlins 1982Ec p.361 & Fig.2) we’d find error waves of amplitude 1° in the Catalog’s latitudes $\beta$ (cosine waves: $\text{ibid eq}.4$) and northern longitudes $\lambda$ (sine waves: $\text{ibid eq}.3$). We don’t. (Amusing details at Rawlins 1992V §§13-C15 & fn 31). See also the inspired findings of Graßhoff 1990 — which instantly converted dedicated and scholarly Ptolemaic 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 discernable in the $\text{Alm}$’s, including a few ultra-giveaway cases where a star with an error of several degrees is found to have the same sized error, with the same sign, for both Hipparchos and Ptolemy. I2 It thus became obvious c.1990, even to the most religious, that many Ptolemy stars were Hipparchos. However, no archon was ever going to admit in print the plain truth: the establishment had been blindsided by proof that its challengers had been right all along — that Graßhoff’s test had now unexpectedly surprise-vindicated the long-loathed Tycho­ Board et al 2014B) is put into the context of decades of like uniformly baseless mobagaba (though those interested only in 2014’s mishmash may skip straight to §C3), 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.

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 Swedlow. (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 bustings (1990 and 2000-2002 attacks on Rawlins. (The unsubtlety 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 disjoint mix of superposed shapes: circular (stars observed afresh) and elliptical (stars copied from Hipparchos). But the diagrams are instead just elliptical. (Even if otherwise, this would prove only that someone other than Hipparchos — not necessarily Ptolemy — observed the stars whose dots mapped circularly.)
that: well—doesn’t everybody withdraw money from banks? The BSC does not claim 1st hand observation, while The Greatest Astronomer of Antiquity explicitly 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 Hiss — 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 Ptolemy’s Catalog authorship (Rawlins 2000A in 177): the 5° gap which should exist between theantarctic circles30 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 $L = 55°.9$, impeded these difficult Sgr stars’ availability. (By recording Fomalhaut, Tycho actually went down to within 2°.6 of the horizon: Rawlins 1993D Table 17.)

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

I1 The Ancient Star Catalog (Alm 7.5-8.1) has an obvious excess of 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-precising them by adding 2°2/3, the false figure which Alm 7.2-3 claims stars precessed during the 223 centuries between the catalog epochs of Hipparchos and Ptolemy, —126.278 (Rawlins 1994L fn 45) and +137.547 ($\xi$D3), respectively. From slying each longitude $\lambda$ by $2^\circ40'$, 00’ endings became 40’; 10’ became 50’s; 15’ became 55’s and were rounded to 00’s; 20’s became 00’s; 30’s became 10’s; 40’s became 20’s; 45’s became 25’s and were rounded to 20’s; 50’s became 30’s. (Note how the odd endings 25’ and 55’ got eliminated.) The frequencies of endings in $\lambda$ and $\beta$ are displayed by Rawlins 1994L 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 observbership (Rawlins 1982C n.3). Schaefer’s dodge (§H2 item [2]) was just the latest in the subsequent tradition of evading facing skeptics’ vindication. Some even accent Ptolemy’s use of the word “we” when describing purported 1°-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 $L$. 

References

O.Gingerich 2002. Isis 93:1.70.
Gerd Graßhoff 1990. History of Ptolemy’s Star Catalogue, NYC.
Alexander Jones 2002E. JHA 33:1.15.

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-domain clique-myths regarding Greek astronomy that we’ve been discussing hereabouts: [a] Ptolemy’s honesty (fn 9); [b] Greek data-ineptitude’s untested-but-allowed 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. [32]

13 Most critiques of Ptolemy’s chicanery point primarily to the excess of 40’s endings (vs 00’s endings) in the Catalog longitudes λ, but (thanks to the 00’ ballot-box being [deliberately?] stuffed with the entire sample of rounded 55’s) the most shocking frequency-contrast is elsewhere (Rawlins 1992V §C22 item [; Rawlins 1994L fn 5]: the spectacularly greater number of 10’s endings than 30’s endings. (Before Ptolemy added 2°40’ to Hipparchos’ λ, these were 30’ and 50’ endings, respectively.) Looking naively at the tabular distribution, the fact that 30’s are the least frequent endings is bizarre, since 30’ is nearly the most frequent latitude ending, as it should be. Indeed, for the latitudes β, 30’s are roughly twice as common as 10’s, but this is reversed for the longitudes λ, by far the strongest confirmation of R.Newton’s hypothesis for explaining the longitudes’ odd fractional-endings distribution.

14 Maintaining the Jekyllian pretense to being a genuine, respectable, outdoor astronomer, required the sneaksiness of hyding the otherwise-glaringly-odd 55’s and 25’s endings (which Ptolemy’s addition-thiev ery had produced from formerly 15’s and 45’s 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 λ are near-benef of 15’s and 45’s (only32 5 in all: five 15’s, no 45’s), though appearing with roughly expected frequency for latitudes β: twenty-eight times more often than for λ.

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’ λ exhibited no particular excess of 40’s. Later, DIO 10 (2000) fn 177 tested Gem-Sqr (roughly Shevchenko’s range) and found that — excepting Sco, whose prime stars’ β reach atypically far south for the zodiac — these stars’ original Hipparchos 00’-excesses were not in ecliptic longitudes but in polar longitudes. The novel and insuperable impediment thus created for Ptolemy’s defenders is found in the footnote cited.

17 The root, of the persistence of the embarrassingly-long (given the evidence’s imbalance) “debate” over the Ancient Star Catalog, is that sneakily (§J4) stealing ordmag 1000 stars is unambiguously, undeniably a scientific crime, verifying the justice of the Newton book’s Neugebauer-klan-hated, JHAD-ennagring title, The Crime of Claudius Ptolemy. Some Ptolemites have improbably dodged Ptolemy’s other (Sun, Moon, planets) fakes by claiming they’re just innocent pedagogical illustrations of his theories (ignoring the inconvenience that he repeatedly calls them real 1st-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 PedaDodgely Ploy cannot excuse his explicit claim (§K1) of 1st-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 (conflating their occasional over-optimism with Ptolemy’s flagrant 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 “mere” Fudger from the naked thief.

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

The Alm is an invaluable resource, our only connexion to much of high ancient astronomy. Given what DIO has induced from it (passim), we are (as distinguished from R.Newton)33 especially grateful for its survival. But it must be used with extreme caution.

The most educational observation we may end with, regarding the tenuous state of the modern history of ancient astronomy community, is this: its fiscal rulership can read all that you have just read and, though finding not a digit out of place in the analyses, conclude or editorial submission to such (fn 1) — we've descended into knowledge-destructive sociopathy. How many image-protective archons ever chose option [a]?

In 1982, complaint from sometime place? Their JHAD is even now hoping to assert [3] Greek astronomy will ever rank uniquely as man's 1's foray in precise predictive science. Its lofty place in human history need not be desecrated by archons' mundane limitations.

125 Jihad-shunning (longstanding: above, fn 109, & Rawlins 1991W fn 171&173 of Diller, Newton, & DIO is known to all in the JHAD-circle, resorted-to from careerists' fear that honest critics are simply bad-for-business. Organizing such disgusting cultist behavior (which works by influence [e.g., fn 116], not ESP) is a disgrace to academics. Like marriage, shunning is easy to commit, hard to end (12 fn 34): usual result of a gullible, "up a pole & 10 ft up" if fields so degenerate. Disrespecting archons and exposing pretensions are worse crimes than the counter-criminals of smearing, shunning, deceiving, stealing, doctoring, censoring, en route to effecting decades of knowledge-subtraction. The exile 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 knowledge. How many image-protective archons ever chose option [a]?

126 Whenever weighing cultists' attacks on DIO's frustratingly reliable induction and computational achievements, it may seem difficult to distinguish between [i] those meant to impress archons with toady-awesomely loyal-slavery to the shun, from [ii] just innocently 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 trashy dating (53-G above) further? Hence 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 Mufla 1994/5-6 Dibner Inst symposium (M.I.T.), a display stack of DIO issues was stolen: DIO 4.1 p.2. c) At the 1997 international History of science conference at Liège, the DIO display samples' sole copy of Tycho's star-catalog (DIO vol.3: Rawlins 1993D) vanished.

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33 R.Newton (pp.365&379) thought Ptolemy’s sham-universal “work displaced almost all of the earlier and valid Greek astronomy. If the Syntaxis had not been written ... much valid Greek astronomy now lost would have been preserved directly. . . . We do not owe Ptolemy our thanks for the small amount of earlier astronomy that he has preserved. Instead, we owe him our condemnation for the large amount of genuine astronomy that he has caused us to lose.” (Note Toomer 1984 p.1’s naïve guess: “the work of Ptolemy’s scientific [1] predecessors . . . being obsolete, . . . ceased to be copied.”) See Neugebauer 1957 p.145 & Rawlins 2008Q fn 223 agree with RRN’s estimate (idem) that data Ptolemy reports from others are faked though (as most loyalists don’t deny) routinely reduced to produce impossibly consistent “confirmations” of his models. Basic confusions of those who see Ptolemy’s derivative astronomy as primary: [M1 [b].

14 It is little understood or anticipated that any decision to shun (usually made in anger or fear, initially) needlessly and precipitously puts the instigator’s integrity at risk. For, the decision is a bet: gambling that the shunner is forever worthless — oblivious to the possibility that his output may prove valid (or later start to be), at which point, how does the bully-invested, no-turning-back shunner then justify continued non-citation? For saving faces (and what else matters to archons?), he has no choice but to start faking the output’s invalidity — not knowing (as he gets progressively deeper-in) when if ever the deceit can stop. As we get to the point where evidences CENTRAL TO THE FIELD (e.g., §N11-N17 and N18; fn 1, 7, & 47 item [1]) cannot be openly discussed without fear of archons — or editorial submission to such (fn 1) — we’ve descended into knowledge-destructive sociopathy.

15 Among JHA-circle herd-talk compliments toward R.Newton and his solid mathematical analyses had been this view (DIO): “incompetent” (A.Aall), “unconstructable” (at intellectually 1984 p.viii), and (all Swerdlow) “silly”, “careless and unreliable,” “Velikovskian,” “absurd,” “crank,” “con-man,” “insults the intelligence of the most naïve reader” (sources for all but Field [Greenwich, 1984] at DIO 1.1 117 [C7 & §3 §§D2-D3). And see Scientific American at fn 52. If taking academia’s pretensions at faces-value, one might’ve thought that the JHA or the American Astronomical Society’s H.A.D. would insist upon its officers’ rational academic discourse. Instead, we’ve had a 4 year long (and optionally inexcusable) non-engagement and/or equally inexcusable and/or editorial submission to such (fn 1) — we’ve descended into knowledge-destructive sociopathy. How many image-protective archons ever chose option [a]?

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129 History-of-astronomy’s present #1 archon Evans joined the unHoly Trinity ruling the History-of-astronomy's present #1 archon Evans joined the unHoly Trinity ruling the
among almost-as-accurate proximate ratios (www.dioi.org/thr.htm#pcpe), or do 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’ blindered private rejection (by subsequently-undercut reasoning) of the DIO draconic solution, flies all of the overkill-numerous, solid, unambiguous evidences consistent with said theory, particularly its PRECISE match to Hipparchos’ draconic 5458° = 5923° ratio (Almagest 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 (Almagest 6.9) paired with a less early apogee eclipse (−719/3/8) for exactly the same draconic purpose, with inferior result—inevitably, due to shorter timebase. Comments: In all history, no astronomer but Hipparchos ever used an apogee-perigee eclipse-pair. Scoffings at the theory’s outrageousness-vs-orthodox-preconception inadvertently compliment it by reflecting its potential advance’s enormity.

I38 §136 presents three perfectly fitting 2002-2003 eclipse-solutions to Greek adopted monthlengths. Why shunners’ 1st-impotence in finding DR-errors? Or alternate solutions? Answer (§134): there IS no other unblamable lunar eclipse-pair whose integral-months ratio precisely, proportionately, directly yields (by Almagest 4.2&6.9 method) any of §136’s three attested, never-before-solved Greek integer-ratio motions, with: both eclipses visible in Greek-Babylonian region, latter eclipse within 50′ of discovery-date (c. −262, −130, resp), earlier eclipse not ere Almagest 4.6’s −720/3/19 (oldest eclipse-data historians-of-science accept that Greeks possessed: Toomer 1984 p.166 n.59). To pioneers who undo the above negative assertion by finding, before 2020/1/1, real umbral lunar eclipse-pairs directly solving the ratio (question in above specs, incl. hist.sci’s own 721 BC bound), DIO will gratefully grant: $10000 for 3277/25121 $20000 for 6247/6695 $30000 for 5458/5923; $40000 extra for all 3 relations. [To certify the bet, DIO will deposit $50000 with the National Academy of Sciences, if it will hold same for winners until time’s up.]

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

J1 The foregoing suggests shortcomings in the modern ancient astronomy subfield re: [a] Scrupulous & neutral refereeing. [b] Turfless generosity & citational integrity. [c] Open-minded curiosity and tolerance (Hoskin, Evans, Jones, and Toomer have fled contact with Rawlins for years, e.g., Thurston 1998D fn2) without a professional-survival priority — nay, necessity — of treating archons with an attitude of nondissent, even supplication, to allay (non-imaginary) fear of being unpublished or outright blackballed, as if such etiquette-issues outrank ([§15] academic skills & integrity. (And o-yes accurate history.) [d] Scientific skills (or regular consultation with able scientists), & especially the purest scientists’ attitude of humble subservience to evidence (acquired by careers of frequent empirical contradiction). [e] Celerity of incorporation of knowledge-advancements (vs fn 20 here: 84 years!!) that will enable a historian of-scientific Kronos to grasp the essence of Hipparchos’ resolution of the Hellenistic conundrum. [f] Essential, genuine neutrality and curiosity (hardly compatible with a cult’s insistence on aggressively protecting sacred viewpoints for decades on end), enhanced by willingness to hypothesize-explore — ever subject to evidence-congruity (e.g., fnn 16&83 vs fn 20&25&40). [g] Philosophy-of-science&common-sense Occam's theory of fabrication explains both and to the Almagest’s 1st precision. Therefore, in each case (lunar or solr) which approach would Occam prefer?

123 Dozen-evidences for Hipparchos as author of 5458° = 5923°: Rawlins 2002H §C.

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

K APPENDIX 1: GRUSOME TESTABILITY WARS

K1 Delambre 1817 2:284 was 1st to notice that, in Alexandria’s 2nd century AD sky, some stars which transited a few degrees above the southern horizon, and were bright enough to have been recorded by hypothetical-observer Ptolemy, were nonetheless not in “his” Catalog — and, by-an-unfunny-coincidence (explored statistically in Rawlins 1982C), all these uncataloged stars were invisible to Hipparchos, who observed 5° north of Alexandria, so that his antarctic-circle (fn 31) of invisibility was radii 5° smaller 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 (§3B [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 Ptolemyism’s death-agonies: aliasing one’s own “eye”@@ for a crime too often requires positioning another. (Cf. C59: §A1.1.1978x, full sample, fn 30, 37, & 42.) I.e., even if one accepts Evans’ argument, it simply exchanges a charge of plagiarism against Ptolemy for a charge of lying. But Evans’ theory fails anyway since this paper’s revelations. (Ref-report to JAHH: “If DR revises the manuscript, I would be happy to look it over.”) Just as Gingerich did with R.Newton forty-seven years ago [now 50!]— details at Rawlins 1994S §B13. After all: must protect even ‘til-now-undefined Thailand and the antipodean Land-of-Ozrrolla [§A2] from the full truth about the integrity & ability of Ptolemy and his never-too-far-away Muffin public-relations organ.

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

37 Similarly, a recent paper (Brandt et al 2014B; see also fn 20) tries to deny the certainty of Ptolemy’s fabrication (from Hipparchan data) of any star declinations δ found in Alm 7.3. Various modern scholars have noticed that Ptolemy “proves” his false 1°/cy precession from his 18-star sample using the 1°/cy “Sixties” and ignores the 1°/cy Twelve (the “SickSix”); they have accurately yielded 1°/cy. From the consistent 6-fold persistence of the SickSix stars’ neatness, R.Newton 1977 pp.220-225 realized that Ptolemy had typically (above, §B2) just indoor-computed the SickSix declinations from his 1°/cy knowledge and then turned around to “prove” 1°/cy precession from said facks. The 2014 paper instead conjectures that Ptolemy quietly stole stars from a Lone-Mystery-Observer of 57 AD, though the proposed LMO is uncited by Ptolemy or any other ancient, so his existence lacks the slightest independent evidential indication. And the proposal inadvertently trades a charge of fabrication (Newton, loc cit) against Ptolemy for a charge of plagiarism: yet another example (above, §K1) of Polemisticians fumble-laying to refute one of the hero’s crimes while not remarking or even noticing that they’re simultaneously stipulating to another! And, again-typically (above, §B3[a]), choosing improbable theory over probable. Unconsidered question, quite aside from the issue of theft: from where and with what degree of inexcusable impotence in finding DR-errors? Or alternate solutions?

This is proposed as a central new theory in a DEFENSE of Ptolemy’s honesty? Immocately unarticitated by the paper’s authors: the 2014 paper’s proposed 57 AD date for the former is near the date which Peters & Knobel 1915 p.15 found for when the Star Catalog’s mean longitude-error is null: 58 AD. But we know from the Catalog’s evidences (§1) that the Catalog’s mean longitude-deviation is equal to all λ (in Hipparchos’ −126.278 catalog) causing an 8 decade shortfall from the intended 137 AD epoch, thus the Catalog’s naïvely-indicated 58 AD date is just as much an illusion as defenders’ proposed proximate 57 AD date for the Sick δ. If not, then are we to suppose that our secret LMO just-so-happened to pop up at the very time that agrees with precession-decident fabrication from Hipparchos? an epoch that’s nearly the same for Ptolemy’s declinations-list AND his Catalog. NB: Ptolemy’s SickSix fraud-mimicry in stellar declinations is especially obvious since his outlier Arcturus’ δ conspicuously is correct for just over 50° later than the date consistent with the remaining stars —
various of the non-cataloged Alexandria-visible stars were conveniently attachable to nearby constellations. And we know that Ptolemy was (or copied) a star-attacher: Alm 7.5-8.1 lists dozens of "informate" stars which are in the vicinity of traditional constellations though still outside them, but which he nonetheless appends to them. This 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 (post-200 BC). In fact, the Catalog (as cited by Ptolemy) contains 21/2 vs Rawlins 1992C Table 3), though not from Hipparchos’ Rhodos, since all were (see idem) of such dim μ as to be beyond Hipparchos’ in-practice mean magnitude limit158  μo for capture. Even more peculiarly absent from the Alm catalog are α and β Gru. Both of pre-extinction magnitude m ≈ 2 — i.e., of Big Dipper prominence! — and quite visible (§K2) to Ptolemy at μ about 3 and 4, resp (though at all hours below Hipparchos’ horizon), they could have just been set aside as a new59 constellation. After all, [i] There already was a two-star constellation, CMi (PK847-848); [ii] Ptolemy was inventor of the new60 asterism Antinous, which he formed c.130 AD from six61 stars “around” Aql (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 ε 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 Gru were below his horizon, and δ Gru’s μ = 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: γβδ (PK76&77) 11°. Αqr: βκε (PK632&636) 12°. Peg: γκε (PK316&318) 17°. Hya: βγγ (PK916&917) 22°. So the no-avail constellations argument is slaughtered by [2] the obvious explanation for why the Catalog’s sole Gru star, namely: of α, β, γ, and δ, only γ was visible to Hipparchos.

The SickFive, whose weighted mean error vanishes c.59 AD — since he faked it to co-prove false 1°/year 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 SickFive do indeed have near-null declination-error for that date), it is remarkable that the lone Ptolemy star (Arcturus), whose δ-error also goes null c.50° later than Hipparchos. (To be exact, 52° after Hipparchos; 56° after LMO.) 46 Hipparchos’ μo 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 §44) 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 33° later is adding his UNoriginality to squirm out of a religious paradox. See Swerdlow similarly at Rawlins 1992W fn 43.

47 Not just the date argues for Ptolemy’s Serapic temple being the source of Antinouos’ intermittent celestial immortality: additionally, there is a copy of that very temple in the Canopic Vale of Hadrian’s Villa, outside Rome. Go to www.dioi.org/ctm.htm#fhrv 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 (Kobenhavn).

48 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°, or 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 1992W fn 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.

**I36** To emphasize the precision and the breadth of this achievement, we display the three-anciently-adopted lunar speeds DO has mathematically traced to hugely-separated eclipse-pairs, all starting around the 13th century BC details www.dioi.org/thr.htm#cqtp:

- 1219/11/23 vs -280/11/16 & -1273/12/05 vs -262/11/26 = 6247° (SystemA),
- -1244/11/13 vs -140/11/27 = 5458° (Hipparch),
- -1200/07/11 vs 1254/05/11 & -1189/06/12 vs 1363/06/12 = 3512° (PlanHyp).

(Latter dates: earliest firm System A text is –262 J.Britton 1999 n.6; Rawlins 2002B [E6]; –140 [Rawlins 2002H §C] and +125 +136 [Rawlins 2003P [C] eclipses are attested.)

**I37** Again: mere integral division is DO’s twice-ATTESTED eclipse-cycle “method” (too fancy a term?) of exactly reproducing all 24 digits. In the 1 1/2 decades since these super-simple DIO solutions 2002-2003 debunk -his historian-of-science has publicly engaged by a single one’s science. Nothing beyond a rigid clique’s continued traditional insistence on its vaporous theory that UNATTESTED laborious Babylonian analysis of poor lunar horizon data could damnsuchly produced such accuracy — if only enough18 data were averaged! (This bizarre notion came inevitably out of the Neugebauer-Babylonianist cult, ever-clinging baselessly180 to its sacred tenet that Babylon gave rise to high Greek astronomy.) Naturally, no numbers are provided123 to show how such a fantastic reconstruction could. [a] repeatedly produce HYPER-accurate results, or [b] find the draconic month at all,122 or [c] distinguish...
Sourcing Ptolemy’s final lunisolar ratio, 105416⁰ = 8523⁰, occurred 2 decades ago (all 10¹ digits exactly elicited) by test-exploring Greek awareness of the 800⁰ sidereal eclipse-cycle nest (1/5 of 800⁰ cycle attested: Gemini 8.40-41): solution, awareness, & nest not suspected ere Rawlins 1996C eq.31. (Sidereal year accuracy: ibid fn 110.) Royal Muftia Cavilliars have produced no math error or alternate solution since. Predictable result is (see Rawlins 1996C’s title and boxed 2013 statement atop its p.2): permanent silence.

More muteness greeted 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)115 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 radius of the Sun and the Sun’s distance from the Earth.116

just the way Ptolemy at Almajest 4.2.8 & 9.9 explains explaining the first 5 months’ synodic, anomalistic, & draconitic. Notably, no matter where, over a 400° span (3° century BC to 2nd century AD), the pairs’ latter eclipses are located in time, all the prior ratio-solving eclipses turn out to be from the very same century, the thirteenth BC (§136). One might suppose the center’s largely old-guard pan-Babylonianists, would welcome the prospect that such remarkable Greek triple-accuracy could have a fundamental & irreplaceable debt to Babylon and would delight in the potential new vistas opened by these astonishingly exact matches. Instead, the entire history-of-ancient-astronomy shunniness, frustrated by inability (like §133) to find math error or alternate eclipses to show non-uniqueness, has been forced to just datelessly scoff (chief sneerleaders: A.Jones and D.Duke) at the very idea of such remote eclipse records as ridiculous a priori. But perhaps neither snickerer has heard about non-cult117 scholarship by Johannes Koch who had already (earlier) estimated Babylonian observations’ precision as about = 1350? Surprise realization that Hipparchos’ famous 600³ lunisolar tables effectively went back just that far only occurred118 in 2015.

So we should gauge the proposed eclipse-ratio method by comparing it to what may be verrry loosely referred to as “the competition” (e.g., fn 119). Facts: No other method is attested. (Twice: idem.) No other method is so simple & immediately-direct-to-the-result. No other method could ensure such high accuracy, 1-part-in-10⁸, 3 times out of 3, eliminating false nearby period-ratios (§137). No other method than eclipse-period integral ratios so naturally accounts for why all said motions were expressed as integral ratios. No other method explains the 4-digit size of each ratio’s 2 components: as in Almajest 4.2.8 & 9.9.

No other credible (fn 119 method, attested (or unattested) has math-reproduced ANY of the numbers sought, while DIO’s proposal has done for ALL 24 digits precisely — that is, all six 4-digit components — on-the-nose in each case: see www.dioi.org/thr.htm#cqtp.


116 §I36. The admirable exception to Hist.sci. ignoring ancient moonlength 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.

117 Moesgaard 1992 p.474. Initial Muftia tactic vs R.Newton & DIO was non-citation. But Isis Editor Margaret Rossister’s publishing DIO-respecting Thurston 2002S defied the 30th shun, inspiring (what else from pathological unre-generatedes?) DOUBLEshun: [a] Thurston’s swift ever-exile from JHA (www.dioi.org/pm/3.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-books for launch: Schaefer 2001 (Pb), Schaefer 2002, Jones 2002E (2nd to Pb), Duke 2005; Duke 2005W (Pb), Jones 2010B (2nd, triangle #1 blackballei no matter how; §§21-32), etc. All on JHA’s certified-Premier (fn 42 board). Re JHA’s prior villain, we quote from fn 22: “Rewards handed out to those who attacked the R.Newton satan include JHA’s boardship (R.Newton 1991 fn 2) & a MacArthur for miss-man [fn 96 here] Sleveland. (It’s hard to find good help anymore.) etc. maid-men Evans&Schaefer were elevated at JHA not long after their massive bungled 1998&2001-2002 attacks on Rawlins. (The unsuitability here may actually be deliberate.) Selecting boardmembers [thusly] will damage mean-IQ atop JHA for decades to come.”

118 For this recent shock, see www.dioi.org/thr.htm#bvk. Re Hipparchos’ 600³ tables, see Pliny 2.9.53, 42. Likewise, Schaefer 2001 proposes that atmospheric aerosols instead of rocks blocked Ptolemy just enough to fool us into mis-concluding that most of the catalog was observed at about Hipparchos’ Rhodes Island L = 36°N. This requires assuming (contra Pickering 2002A §f) that Schaefer’s modern Alexandria daytime atmospheric stats applied to ancient nocturnal air, and that the cataloger was so dumb as to not realize that the very clearest nights were those appropriate to searching out dim rocks just-south of Ptolemy’s putative observatory that just-so-happened to block just-enough southern sky as to make his putative observations’ declination-range deceptively far enough southern sky as to make his putative observations’ declination-range deceptively far.

119 More muteness greeted which is of course just what one would expect of a catalog stolen from an astronomer who worked about that far north of the thief. Note: no other original naked-eye 1000-star catalog’s lowest star was as high as 3⁰: Hipparchos, Ulugh Beg, Tycho, Hevelius. That is, Ptolemy’s “entirely normal” lowest star’s 6°+ altitude is more than double the altitude of anyone else’s lowest star.

120 A passing alibi by Evans 1987 p.166 even imaginatively hints that perhaps there were, say, rocks just-south of Ptolemy’s putative observatory that just-so-happened to block just-enough southern sky as to make his putative observations’ declination-range deceptively far just as if the observer were at Hipparchos’ latitude L instead of where Ptolemy’s
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 $\mu$ 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 apologists' head but actually existed. Yet, test [b] fails. (For both ancients.) By contrast, adopting an atmospheric opacity appropriate to the best nights (when else would one search for dim stars?) near Rhodes' sunlit tip, Cape Prassonesi (see fn 42 for geographical latitude $L$ and height $z$ above sealevel), Hipparchos' list [a] exhibits a startlingly clear\(^4\) split at a post-extinction magnitude $\mu_0$ slightly less bright than 5 (obviously his effective limit for capture): the stars dimmer than $\mu_0$ are in the Catalog, while those brighter than $\mu_0$ are.\(^4\) Comparing these sensible results, to those gotten from applying the same Magnitude Split Test (DIO 9.1 1999 p.2) to The Greatest Faker 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 astrohistorical mystery's ultimate pretender as the “Greatest Astronomer of Antiquity”? [a] Were public attacks on a famous scientist resented by science's politicians as endangering science funding?\(^4\) — but astrohistorian-Mathematician Ptolemy was not a scientist. Not empirical.

any other altitude than $h = 0^\circ$ — see the lucid and irrefutable discussion at ibid [F11]. Further, thanks to an amazing recreation by E.B. Goldstein, we now have the fact (Rawlins 1993D [L8]) that Ptolemy said in so many words that 1st magnitude stars (pre-extinction $m = 1$) were visible on the horizon in antiquity. (In the exchanging-frauds tradition noted at [K1], some loyalists disbelieve this, thereby [ibid fn 93] assuming Ptolemy's “horizon-stars-dishonesty [in order to argue] his Catalog-stars-honesty.”) But, if we assume Evans' preferred (0.20 mags/atm) modern-model opacity, then 1st magnitude stars' visibility on-horizon entails (ibid [L8]) ancients' eyesight perceiving 12th magnitude stars ($\mu = 12$); and at [L8] 2.23 magnitudes similarly entails seeing to 14th magnitude ($\mu = 14$). So: why didn't Archimedes beat Clyde Tombaugh to the discovery of Pluto?\(^4\)

16 Define split-Vagueness $V$ in §3.3's list [a]: dimmest Cataloged star's $\mu$ minus brightest non-Cataloged star's $\mu$. Testing Hipparchos' $\gamma$ (Ara vs $\epsilon$ Cru) at Cape Prassonesi (height $z = c.200$ m above sealevel) for five assumed selectable opacities: 0.14 mags/atm (negligible aerosols), 0.15 (Rawlins 1982C), 0.17 (Evans 1987, #2), 0.20 (Evans 1987, #1), 0.23 (Schafer 2001): $V = 1/5$, $1/4$, $1/3$, 1/2, 3/5, respectively.

Among those tested by Rawlins 1982C Table II, the only major star ([$m < 3$]) that seems unambiguously to be missing from the Catalog is $\mu$ Vel ($m = 2.7$; $\mu = 3/4$ for Hipparchos, 3/5 for Ptolemy). A speculation at DIO 4.3 1/4 showed how star PK964 could be a mangled version of a position originally based upon an hypothetical observation of $\mu$ Vel, high by P1/4 in R.A. (3° great-circle), but in declination fully accurate to ancient precision.

15 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 large error for Alexandros' geographical latitude $L$ ([G1]) and real ancient scientists' high-accuracy achievements ([M3]), participants would (Panglossianly assuming open minds all around) have soon shaken hands, parted in peace — and moved on to more challenging historical mysteries. (As DIO long since has, most of our research on ancients being reconstruction of lost astronomy.) Instead, Ptolemy's promoters from day-one followed his example by knowing all the answers before consulting either empirical evidence (as against texts) or actual able scientists (as against cult goonies). Decades of ugly and harmful warfare followed. Again: all needless. But as with many wars, when it becomes obvious to most observers who's going to lose in the long run, there is a bloody period when
Phys. Sci. 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 archontic cyonurose realize that the 6/5 factor has been standard among navigators&astronomers for over 1000? (All scientific navigation manuals have horizon-dip shrunk by $\sqrt{5}/6$ [vs straight-line geometry] and horizon-distance expanded by $\sqrt{6}/5$ [vs straight-line geometry], both due to atmospheric refraction. See, e.g., the Bowditch.) /  

Question #3: Would it matter? /  

Hypothesis-discoverer Rawlins’ own 1996 case110 for re-evaluation (emphases in original) follows. DIO’s new PHYSICAL — not standard kneejerk-metatological — theory (ascribing both ancient [Earth-C] values’ error to [atmospheric] refraction) simultaneously solves … both the (very discrepant) Eratosthenes & Posidonus values. . . through a single value for the stade: the same . . . 185m value . . . found even in most dictionaries.) No other simple, coherent theory does so. [Classic Eratosthenes stade-scruncher J.Dutka111 . . . 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 only the theory that fits all 3 data (both C, as well as the standard 185m stade) is not even understood by those who keep primarily churning out forced metrological retreats (as recently as late 2016! — in 9799), none of which can fit more than one of the 3 desiderata; and even that single fit is often several times worse than 1%. Hint to metropolitans: your century of stade-tweaking has been obsolesced — simply no longer needed to explain disparate C. Note 3 hyper-boritones here regarding Eratosthenes’ Earth-Circumference experiment, often seen as the most enduring astronomical legend of all, and the subject of centuries of failed metrological speculations. /  

110 Quote from Rawlins 1996C fn 47. Those who have spurned the 185-meter stade include F.Hultsch, E.Lehmann-Haupt, A.Diller, C.Sagan (more at [II]N10). The ad hoc nature of the abruptly mythic runty “Eratosthenian” stade is obvious to most specialists, e.g., P.Gosselin, E.Bunbury, D.Dicks, O.Neugebauer, J.Berggren, A.Jones (see, e.g., antiquity, p.5). Ptolemy’s stade is “greatest-of-cave blindness” — particularly since the first able translators of his text (“Eratosthenian stade-scruncher J.Dutka111 . . . 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’]”) /  

With nothing better. Has the-catatonia got the JHAD-tongue? [The dwarf-stade myth is efficiently, consistently, bluntly, and utterly evaporated by Engels 1985 p.309. Sexagesiminally-defined stade: Rawlins 2012T fn 2, self-contradicting the titular contention of Rawlins 2008Q & note ibid /&4(a) that early-Ptolemaic survey-based Earth-circumference determination was just legid. 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 ([II]6) citing the paper’s atmospheric refraction theory that explains these felicitous fits to the 2 respective ancient C-values at issue, & with no sign whatever of understanding the paper’s physics. Strabo’s arcs (where Earth-curvature is up to a meridian circle of circumference 39870000 m): 5000 stades Alexandria-Aswan & Aswan-Meroê, each good to ordmag 1 for 700 stades/degree: $7^{2}/8^2 + 7^{1}/8^1 = 14^{1}/14^1$. (Rawlins 2009WS&C notes Philo’s solar work at Meroê, presumably for an imperial survey.) Testimony for early-Ptolemaic meridians in 604th-Strabo 2.5.7 (Eratosthenes); also Geminos, etc.: Neugebauer 1975 pp.590 (n.2), 733, & 1364 (Fig.43). Is a Ptolemy I survey’s memory embedded in Kleomedes 1.10’s famous legend? [Rawlins 2008Q &A(a)] Diissmoor 1950 pp.230-251, cites 5 ordmags at 105°-distant stade-long stade-faces. (Shone & other experts think even more.) The only post-Ptolemy-I course (Athens, rebuilt about 1115) is also the 185m one. The ancient stade was 1/8 of a Roman mile (1480m): Engels 1985 p.308. Updated compendium of ancient Earth C-values, at issue: Aristotle 400000, Dikaearchos? 300000, Timocharis? 216000, Sostratos- Hearitophors 256000, and Poseidonios-GD 180000. Correct circumference C = 216000. (Meter = $\sqrt{5}/6$ + 10/10/10 + 10/10/10. The stade-long meridian circle of circumference 39870000 m: 5000 stades Alexandria-Aswan & Aswan-Meroê. Each good to ordmag 1 for 700 stades/degree: $7^{2}/8^2 + 7^{1}/8^1 = 14^{1}/14^1$.) (Rawlins 2009WS&C notes Philo’s solar work at Meroê, presumably for an imperial survey.) /  

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[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. [c] Inevitably-feeble attempts to logically back up prominently published one-sided (fn 11) salesmanship, pushing Ptolemy as The-Greatest, put one in mind of Aquinas’ voluminous Reforming-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 shunning (§B) of R.Newton’s valuable insights? With the Almagest as [i] the central surviving ancient work on mathematical astronomy but [ii] suffused with fraud, a grant-raising problem was presumably feared (perhaps needlessly: §J1), leading to attacks on Newton, but (far more tragic and longstanding) promotion of a now-widely-accepted misperception of all of ancient astronomy — just to cover for Ptolemy’s fudge, 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 Ptolemites. [Wherever there’s a weak, rational-indescifable 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 alibi Ptolemy by wrenching academia’s view of ancient astronomy to fit him has caused as much damage to modern scholarship (§§M1&N) as Ptolemy visited upon ancient and (above, §A) medieval.

M3 “Acquittal” adds that ancient astronomers “were mathematicians who concerned themselves with proof, rigor, logic, and consistency rather” than with observational accuracy. Gingerich 1976 p.477 approvingly quotes Neugebauer 1975 p.108, “It makes no sense to speak to condemn the ancients for the accuracy of 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’ accuracy? How did ancient observers find the mean distance to the Moon within c.2% (59 Earth-radii: Alm 5.13)? Or find their observatories’ geographical latitudes to ordmag 1’? (See [§4 Table 1] or Rawlins 46

46 A month before publication, Rawlins informed Scientific American of the Hiparchus evidence (§M2) proving their article’s central argument was false. The reaction is provided here at fn 52. (Similar stubbornness at §N5.)

47 To back The Acquittal’s fantastic claim that ancient scientists weren’t accurately empirical, Evans 1987 reports that on 1981/7/16 he observed from Seattle the longitude of star Α Sgr by measuring via cross-staff its angular distance from a lunar eclipse and found that his result was off by 2/3 of a degree, nearly triple the angle from lunar center to limb! This error is about equal to that of indoor-Ptolemy’s incorrect 135/16 (rawlins Arnold report (rightly deemed a fraud by R.N. 1977 p.190) — trancribing more relevantly also similar to outdoor-Hipparcos’ huge errors twice (Alm 3.1) when also comparing a star (Spica) to the Moon (during eclipses at all 3 large misses in the eclipse-star observations by himself &Hipparchus). Evans triumphantly concluded: “No better demonstration could be wished of 1 uncertainty in naked-eye observations, preaching that such reality-science’s ‘might temper [Ptolemy-skeptics’] judgments regarding the precision achievable’, no-validated, not-verifiably implicit: So, Msrs. Newton & Rawlins — NOW what’s so criminal about Ptolemy’s one-derogue-erroneous observations?

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

126 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 126 [of how this approach so both ancient & modern researchers arrived at 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 ¶1 §§M&W&X). Do not 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


Possidens&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 are EACH solved to 1% by the same airbend theory & the same unfudged stade.

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ordmag 1° accuracy, before Ptolemy's fateful stades/degree scale-shift\textsuperscript{105} (§111) stretched the map East-West, inflating longitude-differences?  

I22 In 1982, it was shown that Eratosthenes' original Earth-circumference \(C\) was neither of the long-accepted (variously rounded) stade-values, 250000 or 252000, but instead was 256000. In 2008, 26\textsuperscript{2} later, 3 more were checked\textsuperscript{116} 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 the one fixed on Sostratos' non-astronomical Pharos method (§24\textsuperscript{2}), which directly\textsuperscript{117} finds \(r\) from his Pharos Island lighthouse (in Alexandria harbor), built near Sostratos-Eratosthenes’ time and place. Again, our deliberately silent (§§I22&I26) JHAD-center — which accepts no discovery if not from its own network — must regard the temporal/spatial coincidences as completely without significance. (Like unrecognized confluence in another sphere: www.dioi.org/shg.pdf, “Kit Marlowe’s Perfect Crime” §G3.)  

I24 Proposed Sostratos ingenious idea/achievement: mount Pharos’ flame precisely \(h = 300\) feet or half a stade above sealevel, so the apt equation, \(r = v^2/2h\), becomes just \(r = v^2\), thus Earth-radius \(r\) in stades can be found by just squaring the flame’s coastal oversea visibility-distance \(v\) in stades. At first the trick seems suspiciously overeasy & dimensionally impossible. But it works. Note that squaring 202 stades, and rounding conventionally, yields Eusebius’ \(r = 40800\) stades (§I22): a 3-to-1-unlikely chance-hit (Rawlins 2008Q §I1; 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 6½ too high, while Poseidonios’ \(C = 180000\) stades, the other anciently standard Earth-size, is exactly 5½ too low. It is an Occam-DIO dream-come-true to perceive that since horizontal light rays’ curvature = 1/6 Earth’s, atmospheric refraction could have caused errors in \(C\) of 6½ and 5½, respectively, for two simple, clever, low-physical-labor never-leave-home methods\textsuperscript{106} of measuring the Earth: the Pharos-flame method (6/5) virtually attested by Pliny 2.65.164; and the also-physically-easy (& obvious!) double-sunset method (5/6). So DIO’s refraction theory at once satisfies both Sostratos&Eratosthenes&Hippiarchos’ (C 6½ high), as well as

\textsuperscript{105} Rawlins 1985G p.265, taking an idea due to (ibid n.22) Gosselin 1790, suggests an ancient, adjusting for the 700 stades/1° → 500 stades/1° switchover, stretched longitudes by 7/5, mis-assuming they were based on land-surveys (I §F). Or by 4½ via Poseidonios’ \(C = 240000\) stades—180000 stades. Proposing Greeks organized to compare eclipse observations (fn 13): Rawlins loc. cit, vs fn 90 above & with Burchard’s tidal-amp mass, the most-land known land, chosen as Marinus’ 0° longitude (www.dioi.org/501.pdf, §A5), to kill longitude sign-muffs like those (fn 97) cursing Shcheglov 2016, [Speculation 2018/7/15. Marinus = pseudonym, like “Ptolemy”? Or map-title from maritime Tyre?]  

\textsuperscript{116} C = 256000 stades 1° induced from a Nile map’s latitude intervals: Rawlins 1982N pp.212, 214, 216-217; Rawlins 1985G p.29; Thurston 2002S p.66. For which for C it fits Eusebius’ \(r = 40800\) stades: just multiply \(r\) by \(2π/2\): Rawlins 2008Q eqs.8-11&18, esp. eq. 11’s solar distance = 100.1 AU. Fn 10’s 102 AU is overexact (fn 42), even while fitting 252000 less well than 100 AU fits 256000 fits.  

\textsuperscript{117} Rawlins 2008Q eqs.2&11&28; eqs.14-15&17-18 for \(r\) as Sostratos’ direct empirical measure.  


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 moguls with such a degree and breadth of misperception of their own field’s realities may be unprecedented. If they are deliberately trying to acquire a reputation as the sore-dumb retard-s and fanatics of the history-of-science discipline, their plans could hardly be improved upon. (Which actually would be quite unnat... in that such men as Neugebauer, Aaboe, Britton, and others are brilliant despite their blindspots.) Think this too strong? Well, if you have the independence to actually read what follows in this section, you’ll be taking evidence before making up your mind. (We trust this doesn’t break a cult principle.) The specific delusions that have long been orthodoxy in this unread field:
Table 2: Ancient Observers’ Epochs E, Adopted and Actual Geographical Latitudes L

<table>
<thead>
<tr>
<th></th>
<th>E ± σE</th>
<th>Adop L</th>
<th>Its Error x</th>
<th>Actual L ± σL</th>
<th>σ0</th>
<th>σr</th>
</tr>
</thead>
<tbody>
<tr>
<td>Timoch</td>
<td>-294±11°</td>
<td>31°12’</td>
<td>-1’8±2.7’</td>
<td>31°13’8±2.7’</td>
<td>±9.0</td>
<td>±8.8’</td>
</tr>
<tr>
<td>Aristyll</td>
<td>-258±8°</td>
<td>31°15’</td>
<td>+1’0±1.7’</td>
<td>31°14’0±2.7’</td>
<td>±6.1</td>
<td>±4.2’</td>
</tr>
<tr>
<td>Hipp</td>
<td>-131±0°</td>
<td>36°08’</td>
<td>+0°2±1.2’</td>
<td>36°07’8±1.2’</td>
<td>±5.2’</td>
<td>±5.0’</td>
</tr>
<tr>
<td>Anon</td>
<td>+159±0°</td>
<td>31°15’</td>
<td>+4°4±2’0’</td>
<td>31°10’6±2.0’</td>
<td>±6.0’</td>
<td>±5.6’</td>
</tr>
</tbody>
</table>

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 (§3), who noticed back in the 1940s that Ptolemy’s iterative proofs of planetary orbital eccentricities start with highly precise estimates, but by the final iteration they’re round as can be. Real iterations proceed in the reverse direction. [2] Another instance of JHAD inverse-perception of ancestry-direction is shown below at §N15 item [F]. [3] Mars’ Alm mean synodic motion is accurate to ordmag 1°/century, yet the “observations” on which Ptolemy’s proof of it is purportedly based are off by ordmag 1° (Rawlins 1987 p.237). [4] The Alm proof of Mercury’s mean synodic motion was already shown above (§F2) to be pretense, by one who had started with the answer and (if the Canonic Inscription’s elements were also based on alleged observations then) TWICE — differently — fabricated the “observations” to prove it. [5] The centrally phony aspect of the whole Alm is seldom understood (PHuber the happy exception — Rawlins 1991W fn 224), namely, its near-exclusive use of n observations to solve geometrically for n orbital elements. From the often excellent results sampled here throughout, we realize that ancients’ actual historical evolution towards accurate elements obviously involved repeated adjustments from numerous observations. As a mathematician not a scientist, Ptolemy never sensed the value of overdetermination — where the number of equations of condition exceed the number of unknowns sought.

N2 Deeming (Rawlins 2008R §A) the infamous faker Ptolemy an observing astronomer, while antiquity’s ultimate fabricator was the immortal empiricist Aristarchos whose universe was rightly at least trillions of times larger than Ptolemy’s geocentric misconception. Even claiming (Rawlins 2008R fn 10) that indoor Ptolemy (errors ordmag 1°) was a better outdoor observer than Hipparchos, whose errors were actually an ordmag smaller.

N4 Accepting that Greek astronomers were not primarily empirical (§M3; DIO 1.1 ¶ fn 11; Rawlins 2008R §A1 & fn 20). Among objections beyond the a priori: far too many extremely accurate ancient measures survive (§M3&N1, fn 8 [3]).

N5 Because of our 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 Neugebauer believes (e.g., §M3; 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 (§§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’ Cyclistics: 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 Swardlow keeps asserting (with Evans’ and Jones’ evident assent: fn 11) that sooticles 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 Swardlow is not knowingly ladling nonsense to vulnerable archons too predisposed and sub-conscious to recognize the blank.) Due to just such JHA-published pseudoscientific, Ptolemaic-fabricated, and even perversely teach — complete with now-ironically Pomponius sners (quoted, R Newt 1977 loc cit) at sub-JHA untouchedables — that ancient equinoxes were more accurate (fn 11) than sooticles, from their own unfamiliarity (e.g., JHA Editor-to-be Evans fn 11), with [a] the instrumental and astronomical problems involved (R.Newton 1977 pp.81-82; and

amination of nulls in declinations’ fractional-endings profile revealed observers’ assumed L, subtraction of x from which produced actual L. All four observers’ L were ordmag 1° accurate. (Vs ancient astrogolders’ geographical tables’ ordmag-1° L-errors; see §11 R on Ptolemy’s Geography.) DIO repeated its solutions in 2016 — see results at Table 2 — inspired by J.Brandt’s idea to use satellite-based declinations; though (vs 1994’s table), no improvement exceeding 1° in E or even half of 1° in L was effected.

I10 The main Hipparchos observational’s deduced latitude, 36°08’±1’, was just recently independently supported by 2016 realization of the mutually confirmatory reliability of his computing reliability (§D3) together with his trigonometry tables’ accuracy (§C14) which jointly bring 1° precision to a 1997 exploratory finding102 of brackets for observatory-latitude, 36°00’22” < L < 36°00’09”’. This rules out frequently assumed L = 36°4 (Rhodos City) and barely conflicts with commonly rounded 36°, while consistent with above 36°08’, indicating Hipparchos’ permanently fixed instruments were near Rhodos Island’s Lindos (36°05”).

I20 Latitude L of the perhaps-portable (inferior) transit circle of Hipparchos’ most southern observation-spot has been reconstructed103 by: [1] Thurston-inspired spherical-trigonometric transformation of the Almajest star catalog’s southern ecliptical data, restoring originally-observed equatorial coordinates, then [2] testing the declination data thus recovered, to learn which L produces (in implicit zenith-distances) the dramatic, R.Newton’s 00’ endings expected104 for n ancient instrumental observations. That L (35°50’) suggested the southern stars’ observer worked on Rhodos Island’s south tip, Cape Prassonesi (altitude over 200m), L = 35°53’ — intentionally ensuring an unobstructed southern horizon, to see as far south celestially as possible from the island. (Unobstructed southern sea-horizon was also chosen by Eudoxus at Knidos, Pytheas at Marseilles’ Cape Croisette, Tycho at Hvin Island.)

I21 Using Alexandria or Cape Verde Islands as 0° longitude, did far-apart scientists organize to compare each others’ Moon eclipse data, thus fixing longitudes of sites to such extils is to intimate pushback-scholars into silence. And, in current grant-hungry-corrupted academy, it nearly always works. Nearly. None of these three purportedly-refereed journals (vs above & fn 48&49) encourages non-closeted investigation of DIO accuracy or of their own behavior. None of Isis’ ‘bunkered editors’ emails on what ultimately became www.dioi.org/qjo.doc, nor Isis’ 2017 March “referee report” ever mentioned the paper’s history or science or mis-math or factual errors. Not what really matters anymore in history-of-science. (After demanding the paper’s muting, Cohen finally sent an earlier unedited version to his referee, ensuring the negative verdict he sought.)

I10° Table 2 here.

I102Rawlins 1997A eq.1.

I103Rawlins 1994L §E4. Enjoy chronology of Thurston’s felicitously-persistent inspiration of dullard Rawlins’ eventual discovery: ibid §A. The transformation’s statistically unequivocal success (ibid §§E5-E7: overkill standard deviations) is the strongest proof of Hipparchos’ possession of full spherical trigonometry. For by far the smartest and most comprehensive case for Hipparchos’ observship of the Almajest star catalog, see Pickering 2002; also, importantly, Duke 2002C.

I104Excess of 00’ endings (crucial fractional-endings test) discovered by R.Newton 1977 p.247. Cape Croisette: Rawlins 2009P §B.
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 — Timocharios, Aristyllos, Hipparchos, Ptolemy’s anonymous observer — through bivariate least-squares testing upon these observers’ star-declination data.\(^{100}\) In 1994, ex-

\(^{100}\) Rawlins 1994U §F3-F8, where nulls show Timocharios (known observations c. –300–271), earliest of Alexandria’s 3 observers of extant star declinations by surveying instrument, alone knew his exact \( L \). Later, but similarly, all 4 for each individual observer. Mayer-Amsler et al. (1994) in DIO’s bivariate least-squares (Rawlins 1994U 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.dioi.org/bjz0.pdf, says, most of Rawlins’ geographical latitudes \( L \) “close to our values”. But their \( L \) are merely assumed, so DIO’s JAHH-requested 2014/8/26 referee report, www.dioi.org/jau8q.pdf, asked that the paper notice DIO’s entirely original 1994 nulls-method of non-assumptively determining \( L \) (Rawlins 1994L loc cit), a discovery neither available nor even cited anywhere else — a quarter-century later. A few referee-recommended corrections were fortunately applied to the same JAHH’s standard-deviation (computed with eq.10 & Table 3.) We can test the point: the mean systematic error of Hipparchos’ equinoxes was \( 7\)° (consistent calculational conclusion of Britton, Newton, and Rawlins: summarized at ibid §B4), while in spite of \( 6\)° rounding, the errors in recoverable ancient solstices (one in Köppeliss; one by Aristarchos, two by Hipparchos: ibid Table 3 & eqs.1&2&7&8) are \( +3\), \( 0\), \( 4\)°, \( -1\)°, resp, indicating that Hipparchos’ rms solstitial systematic error (\( 1\)°) was more than \( 4\) times smaller than his mean equinoctial systematic error. Unrecorded ancient equinoxes doubtless had less scatter than solstices, but the latter obviously suffered smaller systematic problems (remember, too, that the ancients [needlessly] worried about [and corrected-for: \( \pm 1\)°] the effect of several arcm of solar parallaxes, which would degrade equinoxes not solstices), precisely one of the cult-unperceived reasons why — when ancients sought reliable cardinal points for gauging yearlength — they chose solstices.\(^{38}\)

\(^{38}\) It will be convenient to here list in one place prominent instances, of the pile-on-passion of the most elite historians-of-science, for trashing ancient empirical science: Neugebauer 1975 pp.108, 284, 642-643. Gingerich 1976 p.477. SC`Am 1979. Van Helden 1985 pp.27 & 65. Evans 1987. Evans 1992 p.68. Evans 1998 pp.273-274 & n.32. Duke 2008W p.287. Shcheglov 2016 pp.687&693. (Further discussion at §M3 & §8 fn 8.) Another interlude: Ptolemy’s Tetr 1.1 astrology-promo suggests he’s fighting more resistance than historians-of-science know, i.e., a general nature draw their causes from the enveloping heavens. But . . . everything that is hard to attain is easily assailed by the generality of men . . . . there are specious” criticisms of astrology, but doubts of astronomy “could be made only by the blind”. JHAD’s best anti-fraud defense of its hero might be to plead him “Illegally Blind” §D. See also §E4; & §E6 on his night-shy eyeballs.\(^{48}\)

\(^{48}\) Irony [due to yearlength-estimators’ dependence on truncated (Rawlins 2018U §2C1) prior solstices]: despite known reliable solstices (Rawlins 2018U Table 3), no known ancient got an accurate yearlength. In 1977, Brigham Young Univ astronomer H.Kimball Hansen conceived a simple method ancients could’ve used: [1] Find a stable stone point on a hill which near an equinox casts a North-South shadow at apparent noon on a stable stone surface below, both stone locations being more secularly immobile than human equipment. [2] On some date around an equinox (no need to be just 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 minutes more accurate than any yearlength known to have been adopted in antiquity (even though these were based on intervals of an ordmag longer) all of which were seriously erroneous, for reasons (analysed at ibid §3C-DQ) which do not apply to the remote Babylonian data which ultimately&fortunately made possible the hyper-accurate Greek lunar periods of Rawlins 2017E §B2-B4.
N10 Historians-of-science unexceptionally ignore the perfectly Occamite 3-for-3§ hit-succes of the spare atmospheric-refraction theory that explains and fits both ancient Earth- 

59 Only 2 ancient Earth-circumference C values were widely adopted: Eratosthenes' (really Sos- 

stratos') 256,000 stades, and Poseidonios' 180,000 stades. These latter were 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 2.65.164): [1] Pharos-flame visibility & [2] double- 
sunset (Rawlins 2008Q §A4). Resolution arises since atmospheric refraction causes horizontal light rays' curvature to be 1/6 Earth's, thus the lighthouse-flame method's result is expanded by factor 6/5, 

while the double-sunset method's result is contracted by factor 5/6. However, to see this, one must be able to follow the mathematical physics. There is as yet no evidence that any historians-of-science have ever done so, despite the DO of a century (summary and citations going back to 1982 provided at Rawlins 1996C fn 47) since DIO published this hyper-neat triple solution. Are navigators' 

See that their formula for the horizon's "dip" has been based upon identical 6/5-mathematics for over 

a century?!) These 2 DIO matches exceptionally are achieved without fudging the stade at all, but just 

by adopting the standard 185 meter value. [Thus serving as the final, controversy-ending proof that 

185m was indeed the true length of high antiquity's stade.] Our achievement here may be compared to 

the controversy's endlessly wheel-spinning metrological-solution literature: sifting/excising through 

ancient lore in search of hints of oddball stades, which of course existed all over the place before 

the Poltemies 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 (§N10), 

moreover this resolution matches all 3 targeted data within about 1% of Eratosthenes' 

[ 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 filling journals' pages, can only match 1 out of the 3 (and even that match is usually several 

times looser than 1%). Notice the astronomer-defating shock that the Pharos solution of the famous 

Sostratos-Eratosthenes' C is geographical, not astronomical. [Speculation follows.] But refutation is 

effectuated when we realize that the unclutter (but low-refraction) presumably-Kleomedean-astronomical- 

survey-based 185 meter stade implies C = 216000 stades (since the product equals actual C = 40 

million meters) but 216000 is the cube of 60, hinting that (before Sostratos cleverly but wrongly 

found for C = 256000 stades, c.270 BC) scientific surveyors had obtained an accurate C to which 

Greek science had naturally applied standard Greek sexagesimal division to the Earth's meridians (the 

process' step 1 is even attested: §C 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' 

laterous (but seriously too-high) 700 stades (of 185m each) per degree. But: when did this hypothetical 

achievement occur? Traditional games at the Olympic stadium began before 300 BC, but when were 

the stade-long-footrace markers now found there (Engels 1985 p.298) established? (If athletes ran 
in various states' races, there must have been gradual standardization.) Before 300 BC, while neither of the other two later 

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in various states' races, there must have been gradual standardization.) Before 300 BC, while neither of the other two later 

111 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 naively 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 collaborated (as recommended by Hipparcos: Shcheglov 2016 n.7) local 
times of lunar eclipses at even far-distant sites (fn 105). E.g., the unstretched 42° longitude-

differeence in comparing both Cartaghe and Persepolis is correct to ording 1%. NB: The distorted 

remains of accurate ancient geography in Ptolemy's GD were ruined in two widely separated 

stages, and in two very different ways (but sharing an astrologer-source in each case): 

[1] Latitudes were semi-randomly wrecked by Hipparcos (contra Rawlins 2009S fn 18): his 

discrete tabulation of them (GD 1.4.2) for professional reasons. (See sources here at 

fn 13 for details; Hipparcos was publishing tables [Tihon 2010's valued direct revelation] 

which served 3 astrological traditions simultaneously, sidereal (or anomalistic), Kallippic, & 

Metonic, 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 astrolager 

Ptolemy's ignorant stretch of correct ellipse-based longitudes by 7/5 or 4/3 ($\pm$1%§F&M). 

By successive approximations, mathematician Hugh Thurston in the 1940s de- 

scribed this hyper-neat triple solution. (Are they even aware that naviga-

49 It's long been assumed that Hipparcos around 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 

Hipparcos using exact time for each when gauging yearlength, thus revealing (fn 66) the 

origin of his hitherto-unexplained 365°1/4 = 1°15’30”, which Tihon was 1° to reveal. 

117 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 

Hipparcos' era, not Ptolemy's; sixteen years later, Ptolemists have yet to produce a 

collection of such reconstructions based upon the opposite theory, leaving the independent 

implication that the catalogue was observed, later plagiarized by Ptolemy. 

57 See www.dioi.org/orig.htm, for DIO's long-time $1000 prize for each wrong solution among a 

proposed set of DR discoveries, which Duke&Jones won by overturning DR's historically false 

solution of Mars' & Jupiter's Almajest mean motion bases, Rawlins 2003 §§G&H, cover, & fn 21. (Both men 

agree to Rawlins' period-relations solutions for Mercury, Venus, Saturn.) 'General Theory of Ancients' Cyclicitics': 

Rawlins 2002B §H. Non-planetary cyclicity examples: ibid & Rawlins 1996C. 

Pickering 2002C.
be good to ordmag $i^t$, which invites equinoxx-vs-solstice accuracy-comparison ($\frac{2}{3}$ \& $\frac{7}{9}$).

I6  
Rawlins 1991H found the Babylonian yearlength on famous Astronomical Cuneiform Text 210 was (\S2) based on well-known Greek solstices (~431/6/27 & ~134/6/26), the 1:\frac{9}{10}  

datable transmission of an orbit-element between Babylonian & Greek astronomy, the 1:9.3 viable date for transiting the Muffia's holiest teton (\$37\&fn 120).

I7  
Aristarchos\&Archimedes put minimum solar\&stellar distances at 10000\&10000 AU, resp. because humans can see (as no historian-of-science seems aware) to c.1/10000 rad. Rawlins 2017E \S2: “For advancing history of science, knowing \textit{science} matters.”

I8  
Ptolemy’s allegedly-outdoor 4 solar “observations” (132-140 AD: \textit{Almajest} 3.1) averaged over 1\% error (not even 1 shot encroached anywhere on the solar disk) and were \textit{fifty} times nearer Hipparchos’ 3-century-old indoor tables than to outdoor reality (as known for 2003: \textit{Facette} 2010 p.121): undeniable but also (for over 300) JHA-unprintable." Despite null ancient attestation of their speculations, Ptolemites insist (\$n\&127) this is because ancient scientists kept only observations agreeing with theory. (i.e., they cheated.)

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

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

C. How could Greeks just \textit{copying predecessors} achieve their many accurate discoveries? (e.g., Greek vs recent monthlengths, Rawlins 2017E \S\S2-B4: \textit{startling proximity}.) However, most scholars (A.Pannekoek, R.Newton, Y.Maeyama, J.Brandt, P.Zimmer among the welcome exceptions) reject ancient high scientific accuracy (\$nn1\&35 here), underdurable 2016 subscriber D.Shcheglov even (fn 13 here) calling it a “delusion.”

I9  
Aristarchos discovered precession 150 before Hipparchos: \S3.

II  
Cosmic-order-of-magnitude estimates yielded Greek accuracy (11 \S1): 1\% for lunisolar diameter; 1\% for moon distance; 1\% for Earth-sun distance (\textit{The Greatest Faker of Antiquity} 2018 D.Rawlins 37).

II  
Rawlins’ order-of-magnitude estimates still 2003: \textit{Facette} 2010 p.121: undeniable but also (for over 300) JHA-unprintable.” Despite null ancient attestation of their speculations, Ptolemites insist (\$n\&127) this is because ancient scientists kept only observations agreeing with theory. (i.e., they cheated.)

I10  
Aristarchos discovered precession 150 before Hipparchos: \S3.

I11  
Philo’s at Meroë: Rawlins 2009S \S7, the latitude difference between Alexandria (31\frac{1}{2}°) & Meroë (16\frac{5}{7}°) was knowable angularly as 2\frac{3}{8} sixtieths of C. (At the later standard of 700 stades/degree, this is 9975 stades, only a quarter-percent short of the Strabo-attested distance of 10000 stades.) But, putting Spain into the eastern hemisphere (and China’s Xi’an & Luoyang into the western). Yet another history-of-science journal’s all-too-common PB-paper duffer-science: details of these latest \textit{Isis} disasters can be found here at \$D, DIO’s Letter-to-\textit{Isis} hiding-since-reception Editor H.F.Cohen.
astronomers did their high-precision celestial work (Alm 7.3) by recording angles in the old pedagogical tradition of clumsy fractions of right angles, etc., a position recently undercut by the high-school-level discovery of the previously-unperceived fact that Archimedes’ solar diameter was measured and bracketed in degree-fractions (Rawlins 2018U fn 4)

N13 Selling crude priestly Babylonian indoor astrology as “impressively accurate” (Jones 1991H p.118), and so brilliant it inspired Greek astronomy, according to Neugebauer [1975 p.622 believes in “the advanced state of astronomical techniques” in Babylon] and others of his persuasion (Rawlins 1991W fn 73; Rawlins 1996C fn 128) — even though the sole empirical datum traced in either temporal direction is Greek—Babylonian (Dicks 1994 fn 37). The greater antiquity of raw records from Babylon may say no more than that clay outlasts papyrus. Unlike for Greek astronomy, no record exists of how Babylon arrived at its naked celestial tables (Dicks op cit [C4; DIO 13.1 §2 [4H.]). Clue: virtually all useful astronomical texts from Babylon post-date its conquest by Greece, and its periodic functions are not trigonometric ones (Greek astronomers had trigonometry from no later than c.130 BC) but approximations thereto, by zigzag or even step functions, suggesting (§3 fn 120 below), though not necessarily proving, mere derivative degeneration from its conquerors’ superior technology. (See also Rawlins 2018U §34.) An obvious & devastating point, never previously emphasized for its implications: not a single trig table survives in any form from Seleukid-era Babylon. Babylon made no solstice or equinox observations (Neugebauer 1975 p.366), or meridian or vertical observations of any kind, because (Rawlins 1991W §E3) Babylon had no transit instruments (vs Greek celestial transit data from c.300 BC onward, at Alexandria and Marseilles), and accordingly didn’t know or care what the city’s geographical latitude L was. Which may explain why the only attested figure for it in [38], [a] is not firmly in line in Greek records, not a word on any other hypothesis, not materia­al, another crushing blow to Babylonianist pretensions, and [b] is too far north by $2^\circ$28’ — 148 nmi. Finally, while Greek planetary order was physical — Mer-Ven-Mar-Jup-Sat — Babylon’s was astrological, Beneficent — Maleficent: Jup-Ven-Mer-Sat-Mar —

N14 Failure to learn anything from the first of DIO’s eclipse-cycle solutions (§3 §33), in which equating 9660 synodic months with 781 sidereal years (the interval between two attested local-midnight lunar eclipse records, Babylon – 719/3/8-9 & Alexandria [Heron] 62/3/13-14) neatly recovers all 10 digits of Ptolemy’s previously mysterious final lunar-solar equation (Rawlins 1996C eqs.21-31): 8523 tropical years = 105416 synodic months. (Had JHADists not shunned this remarkable match, they might well have anticipated the vaster discoveries of §N16, long before DIO. Similarly at Rawlins 2009F fn 7.) This is also the 1st irreputable evidence for ancient use of sidereal—tropical transformation of period-relations (Rawlins 1996C eqs.26-27), a process later extensively employed in DIO 11.2, which allows (here, in fn 8 [4I]) 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-cyclical-solutions of no less than 4 long-mysterious periodic lunar mysteries (§§N14 and §§N16-N17) fall upon locked-up minds, disbelieved without a glance by jeering JHADists who (frazzled by inability to find error in heresy’s math but determined [§B3] to reject the obviously probable in favor of the preconception-accordant improbable) resort to whatever dodge will serve. Besides jeerleader Jones’ private carelessly unchecked & glaringly one-sided misfire (§N16), there’s a long-popular theory (item [E]: put into writing privately by P.Huber, likely inspired by Neugebauer [also primarily a mathematician, like Ptolemy]) that is so in­verse-contrary to sensible scientific practice as to gain special popularity among gobbing cult-minds, thirsty for any refutation of undeserving outsiders’ proposals, thus not just suggesting but insisting-upon JHADists’ pure speculation that every long-period relation must have originated from splicing together a few much shorter relations, & no other hypotheses need apply. (The truth was 180°-opposite from this rigid position, of course: ancient’s more-convenient short relations descended from less-handy ultra-long empirical ones [DIO 11.2], for reasons about to be explored, below.) Which demonstrates yet again our JHAD’s unerr­ing attraction to the erring. Considerations:

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

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

H1 Now for a case of ancient data-tampering that actually happened! — the very sort which modern-chaudvinist tamperers have, with dedicated persistence & Creativity, tried misprojecting89 onto valid data (above §3-C-G) that commit the crime of being incompetent to prevailing Muffa preconceptuation.

H2 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 1st — and still only available — coherent solution (fn 91 below) ever devised for the circuli indicated they are no more arithmetical than Hipparchus-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 test91 for possible ancient data-alteration, since all seven circuli are clearly a version of the traditional Seven Klimata.

H3 Fit-probes upon them initially produce ridiculous, yet trending obliquities. Experimentally shifting all circle M by the same simple round constant, 92 1° (or 4π), finds gratifying consistency with the same accurate Hipparchan 23°2/3 obliquity discovered by Diller and insubstantially & cementally rejected by Neugebauer&Jones (fnm 18&25, resp).

H4 Rawlins' 1984 paper (invited for the Greenwich Meridian centenary Longitude Zero Symposium), featuring its 1°-constant-shift solution (fnm 91-93 here) of the circuli, has never been cited by Jones, though his own pale constant-shift ploy for the Hipparchos-Strabo klimata (§§C5-C10 above) is either suspiciously or parapsychologically similar, with the difference that Rawlins' constant-shift solution for circuli turns chaos into coherence, while Jones' constant-shift for klimata is subtractively designed to accomplish the very reverse, leaving such a poor fit to the data that he doesn't 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-putdown tell readers that these restored Pliny data are consistent with (fn 92 here) the very same accurate Diller-discovered 23°2/3 Hipparchan obliquity Jones is busy trying to

89 Conversely, our tamper-projectors staunchly remain prosecution for either of our cases here where its need is screamingly obvious: Trio A's 3° 4-based eclipse (fn 54 above) and Pliny's circuli (present chapter). An awesomely perfect psi-missing record for perceiving when data-correction is and is not appropriate, and, as ever, implying that DIO's simple, neat fits are merely declassé sorcery.


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

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

94 "Crude": Jones 2002E fn 11. Or clever? See restored circle's smoothly and flawlessly spring-tracking curve, graphed in Rawlins 2009S Fig.1, in hollow dots: close proximity to the curve of the data-filled 1°-shifted-Rawlins klimata (values), over the restricted Mediterranean range (much smaller than Diller's) for which the circuli were designed.
is attested at all; [2] is the sole ancient method even nearly capable of such hyper-accuracy; [3] automatically spits out ratios; and, [4] inducing the three solutions without manipulating a single digit, one finds ratios that are exact matches for all six 4-digit factors (24 digits!) in the previously-unexplained ancient records. During the 1 1/2 decades since publication of this unexpected and fruitful discovery, no scoffer has found in the analyses [a] any math error, nor [b] any alternate eclipses also producing the data we have solved-for (which would show our solutions’ non-uniqueness), nor [c] any below-horizon eclipses used in the DIO analyses. Nor [d] the courage to cite or debate this theory in print: fn 6. NB: Whereas the selected pairs’ back-end eclipses are spread across 4 classical-era centuries, the front-end eclipses are 4 times more tightly grouped, in a single productive century: the 13th BC.

N17 It is revealing that the JHAD clique has not only failed for decades to understand the central (§M5) significance of the Almajest’s three lunar periods’ high accuracy — but to then let shunning dictate refusal to cite the 1st solution (§N16) anyone has yet achieved, for how these periods were obtained?! That’s non-mention with an impressively unanimous lockstep... . . (Is there a prize we don’t know about, for this special brand of perfection?) To summarize, cultists keep secret from JHA readers and the larger public BOTH: [1] the historically revolutionary empirical significance (§M3) of the inescapable fact that all three ancient lunar-speeds are micro-accurate, and [2] the eclipse-cycle source (§N16) of such accuracy. (Let Animal House’s Dean Wormer top THAT Double Secrecy.) And don’t miss the saddest&sadist-mostachist part: all disbeliefing archons are religious Babylonians, but their coherent disdain for target-heretics trumps even worship of Babylon, as their rabidbogging shunning requires every single cringing cultist to forgo reveling in the mathematical recovery of the greatest heritage from Babylonian astronomy, not to mention the earliest major standardization by any civilization: observation and presentation of pairs of precious eclipse-records, now-lost, but invaluable to classical-era scientists, who, without the huge N (§N15) which Babylon’s data repeatedly made possible, could never have found those above-quoted astonishingly accurate lunar speeds, that are among the prize glories and proofs of empirical Greek astronomy. Rejectors 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 (§N14 & §J33). [b] Is it JHAD-banned to use intelligence to induce Greek astronomers’ possession of the §N16 front-end eclipse-records? — just as DIO induced (§N19) Hipparchos’ – 157 solstice & adoption of Kalippic motion, both of which (after same JHADists’ scoffs) turned out (§N19) to be anciently attested. In this field, what is intelligence for, if not to revise lost antiques? Were the earlier data extant, there’d be no inductive mysteries here. Who deserves field with no challenges, no advances beyond texts?

N18 Rigid, total, as-usual-unanimous, high-odds-oblivious rejection of 5 new data-match-based proposals central to the field: [1] Computing from his saros-based 4868° Great Year, Aristarchos originated the “Babylonian” month, 29°19'/00°50′, decades before Babylon (§J3 for Q4; or Rawlins 2002A eqs.4-8). [2] DR’s 2001/6/27 British Museum lecture showed (ibid: eqs.9-11; or §J3 & §G5) Hipparchos applied to this the Metonic cycle (235 months ≈ 19°), resulting in his Metonic “tropical” year YN = 365° 4/14 - 15/4868 (fatefully off by 6°, virtually same as Metonic cycle itself). [3] Years before item [2] was discovered, the matching recovery of exactly this yearlength was discerned in Vatican-held ms data listed under Aristarchos’ name on Vat. gr. 191 fol. 170v (data at Neugebauer op cit p.601), & θ ∈ δ’ k’ ∼ δ’ or 365° 4' 20" 60’ 2; if expressed as the continued-fraction 365° 1/4 + 1/20 + 2/60) (Rawlins, op cit, sect 12.13), this is YN = 365° 4/14 - 15/4868, verifying above item [2]. All without altering any Vatican document number and much aided by Neugebauer’s 1975 p.602’s perceptive interpretation of θ as sixtieths. [4] The sidereal-year companion data, Aristarchos’ θ ∈ δ’ k’ θ’ or 365° 4' 10'' 4' from Vat. gr. 381 fol. 163° (Neugebauer 1975 p.601), we write as continued-fraction 364° + 1/4 - 1/10 - 1/41 (§J3 & §G2; Rawlins, op cit fn 14-15), yielding sidereal year YN = 365° 4/14 + 1/152 (good within a few time-seconds), again altering no Vatican-ms digit; and, again, a hit: the (Which expert mathematician66 Jones tends to treat as a kind of evidence.87 Whenever out of the real kind.) From Islam to Aquinas to the JHA to Isis to the Berkeley vandal-shouters: heretical material is valueless junk, so why not cage or destroy it?

G9 No other academic combatant ever previously thought of explicitly defending such manuscript vandalism. (Though see Rawlins 2000A 13.) I.e., it’s the kind of originality that explains why pioneer Jones adorns JHA’s elite Board of Advisory Editors.

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

G11 Though Jones’ paper does not even recognize the existence of DIO’s inductions (based upon very material data), he is fully aware of them: [i] According to Jones — Rawlins 1999/7/14 letter, he was reading the very DIO issue that 1st disseminated DR’s continued-fraction analysis (of the now-Jones-de-accented Vatican mss data) resulting explicitly in Aristarchos’ Metonic yearlength, 365° 4/14 – 15/4868. [ii] Jones was an active listener when Rawlins’ 2001 British Museum talk presented his now-completely-developed series of astronomical-odds, digit-for-digit matches to UNTAMPERED data, e.g., 1778022° & the multiple confirmation of 4868° (i.e., §G5’s Discoveries #1 and #2), the culmination of a century of scientific analysts’ ultimately quadruple-confirmation88 of Aristarchos’ 4868° Great Year. The out-of-the-gate after-lecture commentator was Jones, denying their slightest possible significance. To the immortal discoverer of the Winter Equinox (fn 86 here), it’s all mere Luciferan quadruple (fn 88) coincidence, though his 100% rejection’s plausibility may be gauged from his 2010 paper’s silence on DIO’s Aristarchos numbers (no claim of DIO mis-calculation), even while (fn 85) he defaces the ms data they match.

66 Jones 2002E p.17 (as he chooses his own calculation that doesn’t fit, over Diller’s which does): “I BELIEVE we have to regard the shadow-ratio [which Neugebauer and others rightly argue is not even a shadow-ratio: fn 25 above] as the more trustworthy datum” (emphasis added). Jones has also believed, in JHA print: [a] There are Winter Equinoxes (Jones 1991H p.119). [b] That 128 – 65 = 65 (not a mere typo). [c] Hipparchos-Ptolemy’s mean solar motion from V.Equinox to apogee was 1°/day (Velinolongitude). [d] A trio of longitudes, e.g., (Almajest 5 &5) cannot be satisfied (fn 63 above) by a 3-element orbit solution. [items [b][c][d] all at Jones 1991H p.117. See DIO 4.1 4.5 [a] [1994] for 365°year recognition.) [e] Hipparchos’ Alexandria city geographical latitude L was 31° (Jones 2002E p.16, contra fn 27 & Neugebaur 1975 pp.305&1313.). [f] Hipparchos’ latitude L was found from the “equinoctial shadow-ratio” (Jones loc cit contra fn 27 above). [g] Hipparchos’ Marseilles klima L was 43° (ibid p.17, contra the text [fn 28] and Rawlins 2009R pp 404&41). [h] The Diller-Rawlins 14-for-14 fits, in an ultra-sensitive case (fn 33 above), are yet 14 MORE major accidents (fn 55 here). [i] Hipparchos didn’t use celestial tables (Jones 1991H p.120; discussed at Rawlins 1991W [E4], contra direct testimony of 305-revealed papirus P.St Andrews 267A.) [j] Hipparchos’ Syracuse was 200 stades from where Hipparchos placed it (fn 27 above). For one with a trie less than an entirely perfect judgemental record, to issue overrule-decrees dispensing with coherent solutions, in favor of his own very material data, might suggest non-normal humility & caution. [k] Like confusion: fn 28, or Rawlins 2009S §H. Rummaging: Jones 2005 pp.21-22: fnn 24&27-28. [l] Keep in mind that we have mutually-confirmatory quadruple evidences of the 4868° Aristarchos Great Year. (But not enough for Jones, who at §G8 destroys one of these evidences — [3] below — apparently figuring that, if he can cripple the fourfold-case to holomere-triple-confirmation, he might yet swing observers to sharing his occultist rejection of Tannery-Heath-DR reconstructions.) [n] Censorinus’ testimony (fn 76 above, and item [2]). [o] Almajest 4.2 saros or eilemos expression (fn 81 above); developed at Neugebaur 1975 p.603, where 1,218 is sexagesimalmes of 4868. [p] Vat. gr. 191 fol. 170v – 365° 4/14 – 15/4868 (fn 72-73 above). [q] 4868° 31° 50'08''/20''° 235'/19 = 1778022°, thus (fn 83) matching the remainder-numerator of item [3]’s Great-Year yearlength 365° 4/14 – 15/4868 (§G5). (See parallel fn 83&84 above. And carefully check the gallopings successes reviewed at Rawlins 2002A, culminating in its eqs.10-13.)
precisely produces day-interval $1778037^d$, Aristarchos’ Kallippic Great Year of $4865^d$ — results which yield:

Discovery #1: The Vatican ms’ Aristarchos-marked year, $365^{d\frac{5}{4}}/1 - 15/4868$, is certified as Aristarchos’ by its denominator’s match to his $4865^{d\frac{5}{4}}$ Great Year, as already identified by P.Tannery.

Discovery #2: Merging [A] Aristarchos’ Great Year, [B] his $M$ ($G4$ above), & [C] Meton’s famous lunisolar relation$^{25}$ $235^d = 19^d$, next arrive at a vindication for the kind of exploratory hypothesizing ($\S1$ [f]) that can occasionally move knowledge ahead: 4868 years of $235^{M}/19$ each equals Aristarchos’ Metonic Great Year, $177802^d$ ($15^d$ less than his Kallippic $4865^{d\frac{5}{4}}$ Great Year. Dividing by $4868$ to find the cycle’s yearlength $365^{d\frac{5}{4}}/1 - 15/4868$, perfectly matching the figure (Discovery #1) independently found ($\S2$) two decades earlier from Var. gr. 191 fol. 170v: classic predictive success. Nonetheless, Mufffiosi typically refuse to cite (e.g., $\S11$ below) the confirming evidence. Jones even goes out of his way to destroy ($\S8$ 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 denying that the origin of such wisdom and precision is to be found only in the cuneiform tablets of a plainly inferior, scientifically-unsophisticated and triggles civilization (fn 120; §§2 [N13]), which their cult has become permanently, indelibly in-love with?

$G6$ We can also merge both cycles found from the Vatican mss ($\S2$ by noting that $152^d$ is virtually $1/32$ of $4868^d$, which allows us to see [2017/6/6] that Aristarchos’ Sidereal Great Year is $32^d$ longer than his Kallippic Great Year, thus $1778069^d$. (Heath 1913 p.315’s reconstructed year, showing Aristarchos toyed with a day-rounded exeligmos, is an admirably clever revelation, but not sidereal.) The sidereal year must therefore be $365^{d\frac{5}{4}} + 32/4868 = 365^{d\frac{5}{4}}/1 + 1/(152^d + 1/8)$, or about $365^{d\frac{5}{4}}/1 + 1/152$ (which of course matches $\S2$’s Var. gr. 381 fol. 163v’s yearlength). [Since $15^d + 32^d = 47^d$, we see (2017/12/27): by Aristarchos’ Great Year scheme, precession is $47^d/4868^d$, roughly a degree per century.] Summarizing: the two continued-fraction solutions we found to be embedded in the Vatican ms’s data, were derived by him from his Metonic Great Year of $1778024^d$ & his Sidereal Great Year of $1778069^d$, resp. The latter’s obvious parallel to the former just adds to the astonishing multiple-vindication$^{28}$ 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, to undercut a paper that doesn’t, and how to trash into chaos coherent/kutampered inductions which twice accurately extract cycles ($4868^d & 152^d$) connected to the very astronomer — Aristarchos — explicitly named in each instance right on the Vatican documents.

$G8$ Though Rawlins’ math is ineluctably rigorous, subtractor Jones won’t be denied & so nimbly sidewinds to a sly Gordian tactic: erase all accents on the mss (the cripple—triple ploy of fn 88), which automatically, deliberately wrecs the data-basis for Rawlins 1999’s refined, precise, precisely inductive journey from Vatican mss to [1] ancient science’s ingeniously constructed (Rawlins 2002A §5A) & modern induction’s 4-way (fn 88) reconstructed $4868^d$ Great Year [itself from superaccurate $M$: $\S$G4-G5, www.dio.org/jbl1.pdf, eqs.5-7,] & [2] Aristarchos’ sidereal year $365^{d\frac{5}{4}}/1 + 1/152$. How does such holy warfare differ from the Jonesian DIO 2010A (query en-passant: would ancient astronomy historians accept NYU-InStudAncWorld Director Jones’ deleting all accents from the mss of the Almajest$^d$?) Like-Talibanish is his authoritarian justification for across-the-board wipout: decreeing accents on Greek mathematical data are destoryingly worthless in his Expert judgement.

$^{25}$ See Moesgaard 1983 or Neugebauer 1975 pp.354f.


$^{d5}$ Dozens of similar DIO vindications are collected at www.dio.org/vinn.htm.

$^{d4}$ DIO’s exact confirmatory hits are akin to R.Newton’s also-years-later fruitful success: $\S3$.

$^{d3}$ Jones 2010A p.21. Counter-exh by eliminating accents: *ibid* n.27.
close to the known (also seriously false) tropical yearlengths of Hipparchos & Ptolemy. The 2nd expression suggested 365.25/4 = 1/10 (− 1/4) = 365.25/4 + 1/152, differing but
ormag 10/ from the actual sidereal year then (fn 114 below).

G3 Both results’ implicit periods, 48668' (Great Year) & 152' (2 Kallicp 76' cycles
between itsic Meton’s and Aristarchos’ S.Solstices, − 431 & − 279, respectively), are
among the EXTREMELY few numbers long known to be relatable to Aristarchos, and the
difference between the 2 induced yearlengths IS precession, the very discovery traditionally
mis-ascribed to Hippparchos. Said difference is close to 1/ century, which presumably
later influenced Hippparchos to treat 1'/cy as a lower limit, though Ptolemy eventually
adopted 1'/cy exactly (Almajest 7.2-4). Note that Aristarchos is the only astronomer on the
Vatican manuscript’s different yearlengths, obviously suggesting precession. As the 1st
astronomer we knew was a public geomobilist, he is an apt candidate for true discoverer
of Earth’s precessional wobble.

G4 To measure the Moon’s mean motion & apogee, ancient scientists wisely chose
(Almajest 4.2) the 4267 month eclipse cycle for its 126007
Almajest (Great Year) & 152 cycles
precession, the very discovery traditionally
mis-ascribed to Hipparchos. Said difference is close
later inuenced Hipparchos to treat 1'
to anything Babylonian. TWICE. [See www.dioi.org/j913.pdf, Tables 1&2.]

G5 We know that Almajest 4.2’s saros of 223 M agrees to 1 part in 24 million with
idem’s deceptively-round-looking saros expression, 18 + 10/23 or 18 + 4/15 (where
superscript K signifies Kallicp Years of 365 1/4 each) — which, times twice 153, so

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951Compiled 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.
observations by that amount on average (while not affecting his solstices), an ordnag higher than his actual 2' random solar-declination single-data rms error (or scatter).

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

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

F11 He calls the EH orbit “neither conclusive nor satisfying” since (emphasis added) “parameters deduced from trio analyses [fn 46 above], are very sensitive to small changes in the input data.” CHANGES? It appears that orbit-challenged Duke explored resorting again (as at D5 above, item [c]) to data-alteration, but STILL couldn’t find alternate orbits [i] which fit all the relevant data of *Almajest* 4.11 and 5.38 & 5 — which Rawlins 2012V calls successively Trios A, B, and C — as do the EH—PH (“Frankenstein”), EH, and UH orbits, respectively; AND (ii) whose underlying cardinal points (Vernal & Autumnal Equinoxes and Summer Solstice) uniformly hit upon Hipparchos’ standard 1°/4 precision — dawn, noon, evening, midnight — as 9 cardinal points for EH&PH&UH (not to mention P.Foud 267A: fn 66 here) conspicuously do. Duke has been publicly challenged (fn 70) to produce his alternate orbits. Nothing has come forth.

G Aristarchos’ Yearlengths, Pre-Hipparchos Precession, & Pre-Babylonian Accurate Monthlength. History-of-science Archon’s Talismanization of Evidence. G1 The mystery of the superficially-nonsense ancient yearlengths71 found on Vat. gr. 191 fol. 170v and Vat. gr. 381 fol. 163v lay unsolved through decades of fruitless disagreements (fn 87 here). The name of Aristarchos of Samos is written beside two of these yearlengths: \(\tau x_e \xi \epsilon \delta \kappa \xi \beta \) and \(\tau x_e \xi \epsilon \delta \kappa \xi \beta \), 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 treated73 both Aristarchos expressions as continued fractions, and swiftly sent the results to the *Journal for the History of Astronomy*.74 Listening to Neugebauer’s perceptive recognition that 60 could signify 602, Rawlins saw that the 15 expression could be visualized as 3657/1[4 + 1/20 + 2/60] = 365 1/4 – 15/4868, a classic Metonic “tropical” year, quite

p.199. Invented *Almajest* –134/6/26 noon solstice: Duke 2008W Table 1. Challenging him to produce 1/4-day-cardinal-point Hipparchos orbits with (§F11 & fn 69) distinctly alternate elements: Rawlins 2018U §N2. Awareness of the P.Foud 267A papyrus that confirmed DIO’s uncited §F4 discoveries: Duke *op cit* n.9. For the –145/3/24 11 AM equinox observation (Aristarchos 3.1) of the refraacted Sun, measured on the large χρικος (ring, originally set into the equatorial plane via [refracted] polestar light), which stood for centuries in Alexandria’s Palaestra: given that actual V.Equinox was 15° and accounting for refraction, there was a 1°-2’ error of observation, some of even that perhaps from over 100’ of the ring’s settlement (§10 & fn 122 below). See Rawlins 1982G fn 17 & Rawlins 2018U §B4.


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


75 “The Acquittal of Ptolemy.” Written by Swerdlow-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 & 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 of many whom even today keep believing that if they can just preserve or salvage some sliver of doubt that Ptolemy faked, they are thereby not utterly convicted of the vilest brand of academic misbehavior, in their decades of gang-smearing those who were — the ultimate irony — on the side of truth right along. Which merely adds ethical incomprehension to scientific. And why would a party have ever in the 1st place resorted to slander & shunning & running instead of inviting debate, [I]F [§M1] 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?”
3 Astronomer in Wonderland: Historians-of-science

The technically&ethically-rockbottom brand of “research” skewered in the following pages recudes DIO to surrealistic, summary during NatGeoSoc’s 1989/12/14 & 15, formal-launch of NGS’ amateur [Rawlins 2017B] data-juggling defense of its dying Peary North Pole hoax:

Orchestrates more fiddle factors than the New York Philharmonic.

A physicist-astronomer examines the integrity, refereeing, technical skills, & evaluation-criteria of history-of-science archons, 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 solar motion, epoch, & precession.)

The technically&ethically-rockbottom brand of “research” skewered in the following pages

F5 Reaction to DIO’s vindications has been less than inspirational — but valuably revealing, nonetheless. While still under the influence (fn 59 above) of the late Curtis Wilson, Jones graciously assented66 to UH; but nowhere has it been acknowledged that (repeating for emphasis) Rawlins 1991W and (fnn 27&96) Rawlins 1985H revealed, years in advance of P.Fouad 267A’s surfacing in 2005: [a] a Hipparchos’ search for a – 157 solstice; [b] his tables’ use of Kallippos’ way-out-of-date solar motion; [c] ancient solstices were accurate to ordmag 1°.

F6 NB: These DIO induction-predictions aren’t side-issues. They are central to understanding the early years of Hipparchos’ evolution from amateur-observer-astrologer into an immortal empirical scientist. And subtractors have been uniformly oblivious to a central steel connexion, revealing his original resort to calculating not observing his earliest, mid orientation star 10i Dra, Nature 412:699 (2001/8/16); 1.00pm

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This is but a jawdropper case of confusing systematic error with random. The Duke paper’s Table I displays admirably well-computed times of Hipparchos’ equinoxes, whose errors are clearly sprinkled ordmag 0.1 positively&negatively on either side of zero. Undoing Duke’s historian-usual (§4 fn 43) listing of C – O as “error” (O – C), we see he more crucially overlooked that the Vernal Equinox O – C errors are all negative, while the Autumnal O – C are all positive, since the Rhodos equinoxes were subject to 7° systematic error (found independently by 4 different scholars)67 which corrupted all these equinox denigination (equally well-refereed: see challenge here at fn 70) was adduced to head off that hideous eventualit. (And without even citing Rawlins 1991W, the very paper whose thesis is being trashed! — a wisdom 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 (fn 116&125), such intermittent commando operations are simply standard-maintenance. And, unlike for a military attack, you can completely screw up, but — to your outlander-researching mob (§2) — it’s still a successful kill. Because, besides DIO, no known reader — surely no mythic JHA referee — of the paper in question (Duke 2008W) has yet read beyond its bald claim (that the Hipparchian eclipse trios [analysed in Rawlins 1991W] are worthless), to evaluate its credibility or that of the multiply-misbegotten case brought forth against Greek accuracy. (See Rawlins 2018U §B&N and fnn 10k&19, for the three main errors of Duke 2008W.) So, since the defamation of heresy is uncritically accepted by all, it’s completely effective for its purpose. Why take the trouble to question any of the paper’s assertions? After all (fn 4 here) if they’re in the JHA, they must be true.

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

66. EH&PH elements compared at Rawlins 1991W: [K9 vs §K10]. Duke 2008W pp.293-294 calls DIO’s “reconclusiveness” in the case to be “excessively due to DIO’s error in the past but the putative said math is too “sensitive”, implying (fn 66) that §F4’s double-vindication by papyrus was merely spooky-lucky. These inexcusably (esp. §6 here) citationless attacks are met in fn 37 above, and indeed had been anticipated decades ago in Rawlins 1991H §H3 & Rawlins 1991W fn 205.

by him from UH, unbeknownst to Ptolemy); however, the second alone agrees (by chance, as it happens), though the underlying mean longitude he lists for it is discrepant by Σ vs Ψ — even while tellingly agreeing to the arcmin with UH.

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

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

[6] Σ vs Ψ (from UH, unbeknownst to Ptolemy); however, the 2nd version of the 4868’ cycle at 1778021d (not Hipparchos’ 1778022d; §5 below) it embossed an astounding quintuple of geometrically expanding cycles. Previous analyses never got past the 1st cycle of the five, e.g., Swerdlow 2010 p.174.

F4 From fitting ΕΗ to eclipse-ΤriB, ΜΟO mathematically induced (Rawlins 1991W §4.K-9) in 1991 that Hipparchos’ earliest Sun orbit, ΕΗ, [a] used a –157 summer solstice, [b] adopted Kalippic solar motion, 360°/365°/4 for Αιος Α. Β. Β. Fings [a] & [b] were both previously unsuspected. But, 14 later, papyrus P.Foud 267A was examined by A.Tihon (paper 1st presented: Peking 2005) & was found64 to explicitly verify 1991


F5 Quintuple succession of doublings (Rawlins 2002A at fn 14’s conclusion): 304°1/4 (1st difference between Hipparchian and calendars); [360°/2] (h Hipparchian calendar); 365°2.157 (1st return of Sun), 1217°2 (1st return of Sun); 2434°8 (1st return of Moon); 4868° (integer number of days) — every one successively featuring a fresh characteristic cyclic-return, where each of these includes (like the unfresh 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 the 1°-0.66 (very nearly, Ydorl’s) 0.25° (157°6’), 0.12°, 0.06° (157°6’/2), 0.03°, 0.02°, 0.01°, 0.07’ (0.25°/4), 0.04’ (0.25°/16), etc.)

F6 Tihon op cit. The papyrus’ solstice-day –157/6626 (correct) seriously differs from Hipparchos’ original false indoor computed-Solstices (–157/6628), as reconstructed at Rawlins 1991W §88, a point precisely resolved when Rawlins 2018U §5 discovered both [1] the solstices’ 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°1/4 – 1/390. 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-15 systematic errors (from Earth-orbit eccentricity), ibid: eqs.10-13. Not to mention DIO 20’s lead paper, with its important fresh discovery—§1 below of Archimedes’ 3rd century BC use of degrees. There has been no engagement on any point as yet, except for a somnambulist-refered JHA paper, Duke 2008W, which (at its pp.293-294) doubts Greek observational accuracy by centrally confusing systematic error with random error, causing misfeye by a factor of ordmag 10 (see §F8 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 ΉHA’s board in traditional (fn 116 below) fashion by attacking it, albeit frustratingly reduced to merely non-quantitatively implying that §F4’s three hits (upon the right year, and twice on the right solar motion) must have been just another trio of JD3-like big &big &big coincidences! (Now do you understand the advantages of dispensing with real refereeing? Another at fn 4.) This Duke claim appeared soon after Tihon 2010 was presented at CaTech in 2007, timing which suggests the possibility that the ΗΗΑ had perceived danger right away: the nightmare of general recognition of the forgoing triple-miraculous papyrus-vindication, of a banishese’s paper which had been so satirically advertised such typically-refered ΗΙΑ discoveries as the Velikovskian 366° yearlength by Duke’s ΉΑ co-boardmember Jones (Rawlins 1991W §G9,a DIO 4.14,1994) — so the usually bungled lead-paper

been patching-up for decades without the slightest discernable (positive) effect on the field. E.g., three-cornered History of science journals cut contact with DIO, when, e.g. (fn 97), asked to print the embarrassing but unquestioned fact that their icon Ptolemy’s four Sun “observations” were FIFTY TIMES closer to Hipparchos’ 28°-old indoor tables than to the outdoor sky, none doubting (2 §N8) Neugebauer-Gingerich-Science’s decree that an astrologer & clumsy faker whose frauds damaged &retarded predictive astronomy for 1000’ (2 §A) was “The Greatest Astronomer of Antiquity” (fn 1 here). Have shunning, censoring, data-fudging, & viciously (§4 fn 2) defending naked fraud by a cult-glorified pseudoscientific superstition-peddler (long notorious among scientists) devolved from merely-tolerable to insistently-normative? Simultaneously with Isis’ resistance to the below history, it was learned (see §1 here for links to all papers) that: [1] a 7°-old DIO discovery (Rawlins 2008Q fn 6) had been unattributedly published, www.dioi.org/cov.pdf, as Isis’ 2015 LEAD article (repair request repulsed), and [2] Isis’ pseudo-refered final 2016 LEAD article had extensively attacked Rawlins 1985G (Greenwich Meridian Cente-
History-of-science — Data-Tampering, Idea-Theft, Seminumeracy, Smearing, Shuns, Club-Preferredie
Wellspring of a Projective Myth: Greek Science as Fumbling, Fabricating, and Unempirical
Muffia Cult’s 842 War On Greek Astronomers’ Cornucopia of High-Accuracy Achievements
Current Historical Advances Endangered

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

Carefully crafted and refereed advances in the history of ancient astronomy and ancient mathematics: [a] have long been exiled by centrist-journal editors who shamelessly flee (fn 100 below) whenever they cannot justify their actions, as observed 34 unprogressive years ago by Robert Newton (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; 2 displaying scant evidence of refuting or even such epiecanons 3 of scientific evaluation as simplicity, minimal-premises, fruitfulness, and predictivity. Auto-rejection has been inspired by simplicity of durable grantmagnet (Diller 1984 fn 26) orthodoxy that the famed ancient data-faking (§18), bumbling (§1 §E) mathematician-astrologer Claudius Ptolemy was “The Greatest Astronomer of Antiquity” (like hype at, e.g., fn 9) whose allegedly- outdoor solar observations’ New early spherical trigonometry date, 2nd century BC (fn 16-17 & 24 below); the same era’s 1st-accurate trig tables & 1st-accurate calculations (fn 37); 3rd century BC Greek scientists’ use (fn 42) of order-of-magnitude (ordmag) and their adoption of degrees (fn 94). (Synopsis: glossary: §[32].) JHAD = cartel of JHA (Journal for the History of Astronomy, O.Gingerich principal editor for 40”) & HAD (Historical Astronomy Division, Gingerich long-dominant co-founder) of the American Astronomical Society, whose Ethics Statement has just denoted [2017/10/11] research ethics three notches: now behind [1] race/gender, [2] sex-triggers, and [ironic in present context] [3] bullying. DIO argues evidentially for high Greek accuracy (§10; Rawlins 2017E) Vs.hist sci reverence for aligning inaccuracy, to ameliorate Ptolemy’s gross fabrications, e.g., inverting relation of theory&evicience (Ragep crudely fn 9), & focusing on Greek “theoretical structure, erected in spite of the enormous difficulties that beset the attempts to obtain reliable empirical data” prominently quoted by Gingerich 1976 p.477; see fn 8, 6.2, & 97 here. Neugebauer 1975 p.931 crowned indoor astrolæger (Rawlins 2003X) Ptolemy “the greatest astronomer of antiquity”, echoed verbatim by Gingerich 1976 [AAAAS!] & Gingerick 2002. Since Ptolemy’s Almajest 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 Almajest 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-only, 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: §§[1]C1, D2&D3, E2, G7-G9, G11, H4, I22; fn 12, 33, 44&45, 55, 68&69, 89. 3DIO’s principled approaches to knowledge are brought together below, at §J[1] [g]. See, too, fn 10.

BM55555 [ACT §210] (c. 100) bears a yearlength computed from Greek solstice data, an unexpected, shockingly-contra-orthodoxy discovery: 1st definite proof of what must have been substantial Greek influence on Babylonian astronomy (e.g., fn 120 & §2 [N13]. Backed immediately by Britton and long accepted near-universally; but lately unnoted, except opposed by Duke (non-citationally), using a faked Almajest 3.1 & report: fn 70 & §JF. Reconstruction of the UH orbit cleared up a half-dozen mostly-hanging mysteries simultaneously (resulting DIO papers never cited by JHA): [1] Why the Aristarchos 279 and Hipparchos 134 Summer Solstices are the only 61 two among the twenty-eight solar records 62 of Almajest 3.1 & 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 positions 63 of the Sun at Almajest 5.3 & generally conflict with PH, but all consistent with UH. (Jones appears to accept this analysis.) [3] When Ptolemy recomputes those true longitudes (via the PH orbit he adopted throughout the Almajest), he twice finds disagreement with Hipparchos’ reported values (all computed

61 Below, §[9]; or ibid §§B3&B4. BM55555’s revelation: ibid eqs.6&8. I will ever be grateful to the late Willy Hartner, who was 1st to suggest (letter to DR 1980/8/15: ibid §(A) that scholars (including DR) were ignoring Ptolemy’s hour-omission for two of the Almajest 3.1 & 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 Commentaries, which are cruder than for his regular longitudes (Almajest 3.1 & 2) or declinations (Almajest 7.3). Neugebauer 1975 pp.642-643, deems Aristarchos’ data nonempirically faked (similarly Evans 1998 p.72) vs Rawlins 2006R §A, sardonically at §A3, condensing the most unexcusable of JHAD fantasies (emphasis due).) “rebel & heliocentrist-pioneer Aristarchos 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 1st 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’s distance, and sidereal to high precision — was a minor, confused figure. But a fabricating, 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 Hipparchos: www.dioi.org/jrb24.pdf, DIO 11.2 [2003] [4 Appendix 2 §4.8.) 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 forums’ consensuses, a hypothesis suggests itself which is consistent with this grotesque historical-distortion-by-historians-of-ancient-science: in a grant-grubbing era, public history’s accuracy and balance is now determined by which figure left the most works. By lawyerly fake-justifying the superstitions which powerful institutions (religion, astrology, gov’t) want to promote, go-along-conservative Ptolemy’s Aquinian-earnomous pandering to power-institutions and their brainwashed victims, ensured his works’ massive preservation — thus, historians-of-science today can make a living by writing them, and holding conferences on a fat corpus. Revolutionary pioneer Aristarchos, by fighting the same institutions in honest and principled defense of emerging valid but upsetting scientific perceptions, ensured his works’ near-extinction, so there’s virtually nothing (explicitly — for those who cannot induce beyond the texts) left for moderns to 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).
all 4 solutions had appeared earlier in the very DIO paper he is non-citing while trying to exit & replace it.

**E2** Nonciting Newton’s and DIO’s analyses protects readers from learning that, after application of Newton’s unexpectedly productive-predictive 1st correction, Trio A is satisfied by unzany elements, which turned out — as discovered 14th 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 & prepared), while immediately adopting PH’s zero-point & apogee (neither needing tabulation). Subtractors must see as further mere-coincidences both the correctly-paired split (between tabulated and table-irrelevant parameters) — AND the temporal order matching §E1’s chronology, above. Coincidence piled on coincidence? Or shall we turn for guidance to Night Life, 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 (Almagest 3.2&6) & still worshiped by Julian over 500th after creation. But DIO reconstructed §E2 two other Hipparchos orbits: his —157 Early EH orbit (§E1) & —134 Ultimate UH orbit (idem), each seriously differing from PH & previously unknown. (But Hipparchos’ use of a late non-PH orbit was presciently suspected before anyone by Britton a half-century ago.)

**F2** Discovery of Hipparchos’ final UH orbit arose from calculations, §57 referred and supported by Thurston §58 and Curtis Wilson, §59 based on realization §60 that cuneiform text (eccentricity & speed) that had to wait for adoption ’til tabulated were naturally those that temporarily stayed EH, & prepared), while immediately adopting PH’s zero-point & apogee (neither needing tabulation). Subtractors must see as further mere-coincidences both the correctly-paired split (between tabulated and table-irrelevant parameters) — AND the temporal order matching §E1’s chronology, above. Coincidence piled on coincidence? Or shall we turn for guidance to Night Life, 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?!*

**G Postscript**

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

In the contemporary history-of-astronomy subfield, numerous coherent, mathematically-coppper-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 careerrist serial knowledge-subtractors, — commonly called the Muffia or the JHAD (fn 1) hereabouts — have themselves made a discovery, to wit: that their network of politically centrist captive journals will help wage JHAD against heterodoxy by publishing effectively unrefered articles that:

4 Wikipedia’s article on the virtually unrefered Journal for the History of Astronomy (JHA) actually claims the journal is “peer reviewed”! (See fn 109 below, also re Wikipedia’s 2008-2014 war upon Rawlins’ Wik-biography.) Meanwhile, the best-referred 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 CSCICP-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 above at 51’s POSTSCRIPT; also www.dioi.org/jhb.htm, www.dioi.org/ff.htm#Sxg. Historians-of-science seeking ready buddy-publication (to convince their universities and funders that they’re academically contributory) evidently prefer such laxity as being refereed scrupulously, since far more vitae get padded by indiscreetness than by care. Given this spectacular mufselficiency 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, cartel: “The Mufa”. Considering the history laid out in the current paper, who could resist such an appellation? A secret of maintaining eternal mufself 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 Princetitute’s iconic O. Neugebauer (Science seconding). “It makes no sense to praise or to condemn the ancients for . . . accuracy or . . . errors in their numerical results. What is really admirable in ancient astronomy is its theoretical structure”, a view defied by physicist R. Newton’s 1977 Johns Hopkins University book, *The Crime of Claudius Ptolemy*, and by D. Rawlins’ scientific-history journal, *DIO* (www.dioi.org/dioimd.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 predecessary could advance to their surprisingly numerous but heretofore remarkably unappreciated high-accuracy Greek measurements. (Below, compare §§10 & [11-12] below; Ptolemy’s celestial fakes; Hipparchos’ elaborate 1st-accurate calculations, effected by 1st-accurate trig tables; his use of spherical trig; Archimedes-admired pioneer in heliocentricity & spatial-vastness, Aristarchos: P. Tannery’s and DIO’s quadruply-verified (fn 88) reconstruction of his temporally-vast 4868 Great Year, 1st-accurate monthlength, and pre-Hipparchos discovery of precession. One of R. Newton’s favorite expressions for counter-revolutionary mis-scholarship: A subtraction from the sum of human knowledge.
A Noncote the heretical paper primarily targeted. Deliberately: §§C10, D6, G11, fn 10, twice more at fn 121. (This pattern goes back at least to the prominent Sky&Tel attack on R.Newton by dutiful Muffiooa Janice Henderson 1976, without citing his name or papers.) [B] Exhibit daring proclivity for the inherently improbable over the probable (even the most remotely of each: fn 33&36&45), whenever seeking apparent evidential support for any cult-in-too-deep position threatened by new findings.

[Effect sterile destruction by demeaning or outright altering (e.g., §D5 item [C]; §F10 fn 25&85 [& see fn 30&118&58]) the data upon which an offensive discovery is based, decreeing that whatever version of said data was previously accepted (right up until a few hours ago) there instead found the 41st reason for not resembling or now — of a convenient sudden — become suspect of unreliability, often requiring the subtractor’s own Expert revision or doctoring to have any hope of the data over attaining cult—Acceptability. In-balance is the possibility that one or two or dozens of valid potential advances might languish indefinitely- unrecognized, wasted.

A2 Below, we analyse, in §§C-G, a flock of recent instances of such data-fudgery-for-orthodoxy, aimed at submerging competitively 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 outsiders — 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, 38! ago: DIO 1.2 fn 6c&[B4] whereby “editors” become little more than printers. Note DIO’s typical insensitivity, www.dioi.org/tbh.pdf, in asking an editor to edit.) And it shows. Lucky for us, JHA refereeing’s judicious deliberateness is self-extolled by proud 2013 Editor M.A.Hoskin, www.dioi.org/pm3.htm, emph added: “it is quite common for an article received at breakfast to be refereed during the morning . . . and the verdict sent to the author by lunchtime.” For about 60 cases of thus-inevitable JHA-published odd and/or miscomprehended scholarship, see: www.dioi.org/p1.htm, samples here at fn 10-11&86s. No like list of batches debutng in DIO has been or can be compiled. Maybe due to scrupulous DIO refereeing? At least until a few years ago.


6 O.Gingerich, longtime head of Harvard’s History of science Dep’t, defames Ptolemy-skepticisms, 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 (§2 fn 5), merely for objecting to communal shunnings he and everyone in the field knows are real, even attested: fn 5 here; and www.dioi.org/pm1.htm, www.dioi.org/j43pdf, "Naked Came the Arrogance", Rawlins 1945 §§SS-B8; Gingerich. loc cit; and cult echo-slander sampled at www.dioi.org/j111.pdf, DIO 1.1 §7; also see fn 16&20, as well as ibid §3 §§D2-D3, vs §3’s fn 7. Establishment-serve Gingerich’s whackamole campaign to control heresy is detailed at www.dioi.org/j43pdf, 1994. His referee reports on skeptics often pretend (between slanders) that it would be ever so good to have the other side heard (§4 §A2). Were this not shams, this 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,

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 Almagest 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 course49 to satisfy Trio B, which doesn’t work, anyway, unless an extra variable d (hitherto not in evidence, in Ptolemy, India, Toomer, DIO, or elsewhere) is arbitrarily brought in to reconcile this situation; [B] finds no round elements; [C] alters extremely precise numbers like 51°30’23” and 8°48’28” to instead become extremely precisely 57°19’37” and 7°44’08” respectively, though there’s no reasonable potential of error, or any independent justification of fudgeries so shamelessly explicit, besides rigorous issuance of The Right Answer. All to smother DIO’s natural-flow-multitif coherent solution under a pillow fluffly with special assumptions&tampering, resurrecting the spirit of co-subtractor Jones, above (in §C) & below (fn 8s). Like Jones (§C10), Duke has refused DR’s request to withdraw the paper.

The non-manipulated Rawlins 1991W fourfold-fit reconstruction (above, in §§D2-D3) is never cited throughout Duke’s prank (did hypothetical referees even know of it?), though his paper originated as a challenge to that very reconstruction, vying for a DIO prize, www.dioi.org/prl.htm, but evaluated and rejected by DIO prize-judge Thurston. It was later published by Centaurus. (After refereeing whose superficiality is shockingly obvious for math and even text.)50 Contra Duke’s attraction to committing fudgery: throughout Rawlins’ researches, it proved unnecessary to correct any of Hipparchos’ calculations in order to draw coherent results from his data, so a historically new conclusion51 emerged:

Hipparchos’ purely mechanical computations are dependably flawless.

E Robert Newton’s Foolishly-Ignored Discoveries of Hipparchos’ One-Element-Eclipse-Degree

E1 DIO shows (fn 56 below) Hipparchos’ –157 Early solar orbit “EH” was succeeded by his –145 prime-years’ orbit “PH”, in fact replaced by his –134 Ultimate orbit “U1”. He adopted EH+PH when computing lunar elements from long-earlier lunar-eclipse Trios A&B, below fn 52. In 1977, physicist Robt. M6 discerns that the 2 elements—encounters from ignoring Newton’s 157 warning—will ow from analysis. The orbital elements derived by warning-rejectors Jones and Duke inadvertently double-confirm49 Newton’s “incredible” prediction. Like Jones, Duke does not notice Newton’s 157 warning, & deliberately (§D6) acts as if the carefully-referred (§D3) DIO paper that did heed it does not exist, though it was unsuccessfully challenged (§D6) by Duke to arbitrate Thurston and (fn 32 above) precis in Isis in 2002. Duke’s paper independently computes52 best-fit e&A of the Trio B-accordant EH orbit, & of the inevitably weird-unrestored-Trio-A-accordant orbit, as if original, though


50 Rawlins 2012V fn 17, and especially fn 22, where we find that no referee ever read the Duke paper’s TEXT. For practices & business enterprises that lead to similar disasters, see fn 4 & §2 fn 47.

51 Below fn 72. The 1st shift, discussed below in §E, turned out not to be a mistake but (worse) a deliberate Hipparchos-school fudge, as shown in Rawlins 2012V §G & fn 11.

52 Ibid §A3.


55 Ibid p.293. The Ake for Trios A&B, which are independently computed and presented at Duke 2005ST fn 5 and Duke 2008W loc cit, were published years earlier at Rawlins 1991W fn 205&162, respectively. Half&half discovery (§E2 below): ibid §M5; and §M6 discerns that the 2 elements


The text is a complex and dense discussion of historical and scientific subjects, particularly focusing on the匹配 of mathematical and astronomical data, the behavior of historians, and the findings of a modern astronomer named Trevor Rawlins. The passage is replete with references to ancient scientists such as Hipparchus and Eratosthenes, as well as modern scholars like Trevor Rawlins.

The text also critiques the behavior of historians and scientists, calling out what it sees as a lack of transparency and integrity in the field. It questions the reliability of certain historical claims and the motives behind them. The author, Trevor Rawlins, appears to be a modern astronomer who has conducted extensive research on ancient astronomical data and is critical of what he sees as the shortcomings of his peers.

Overall, the text is a testament to the complexity and rigor of historical and scientific research, and the challenges faced by modern scholars in navigating the often contentious world of academia.
B2 In 1878, in order to justify the modern Ptolemaic vision of antiquity, JHA-Editor-in-
progress J.Evans published an exceptionally polemical, technically pathetic, but politically brilliant Step-One towards becoming a Mulha 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 eldest 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 with The Amendment Catalog thief who were impecedly wrong were elevated — the most-impecedently 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 1st: respectively, -40°, -33°, +33‘.

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

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

D1 As recounted in 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). Ptolemy 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 $e = 327 2/3$ units; for Trio B (-200-199), $R = 3122 1/2$ units, epicyclic $r = 247 1/2$ units. G.Toomer tried an ingenious and daring reconstruction that 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 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., Almajest 3.1) user of Aristarchan data, he could have computed the $R$ via Aristarchos’ famous 87° half-Moon elongation and a typically ordmag-rounded solar distance of 1000‘, as follows:

9 Rawlins 2009E §A (emph in original), critiquing Evans 1987, http://journals.sagepub.com/doi/pdf/10.1177/002182868701800401, also Evans 1998 (appreciatively reviewed for its considerable merits by Thurston 1998D in DIO 8). Overview-question-in-passing: is there any reason other than ambition that would lead a scholar to look for a way to defend Ptolemy’s honesty where (as for the Moon-star case at hand) the evidence is too obscure for non-specialists to understand? And even if already for at least 1200‘ ($[\text{eq.11}]$ for both orbit radii 1200‘, $[\text{eq.12}]$ for $\text{A}8$’s $[\text{eq.11}]$) mis-saw the Moon outdoors by -140 for its radius, he allegedly never thought of science Society’s brushoff of Ptolemy’s connexion to astrology, believing that only unenlightened, “paradigm”-insensitive scientists could suppose it reflects negatively on him. But even if Ptolemy’s disinterest in his line’s mathematical sophistication had resulted from his lack of time, 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 $e = 327 2/3$ units; for Trio B (-200-199), $R = 3122 1/2$ units, epicyclic $r = 247 1/2$ units. G.Toomer tried an ingenious and daring reconstruction that 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.

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his theory’s doubtless-illusive woes: they are the unreliable party, having committed the offense of disagreeing with the theory of the most authoritative expert (here, in fn 86), adding that Hipparchos’ trigonometry tables are suspect of a parallel disloyalty. Meanwhile, DR contends that the glad & enlightening opposite is recommended — both for Strabo’s klima data\textsuperscript{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\textsuperscript{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, convincing it to the Orwelian-Gebrelian flames it deserves.

\textbf{C12} Note: it’s been obvious since [the Rawlins 1994AM investigation] that 1\textsuperscript{st} accuracy is crucial\textsuperscript{12} for the 18\textsuperscript{th} klima’s fit (Table 1 above; or Rawlins 2009S Table 2), suggesting that the historical process of refinement of high-accuracy trig tables goes back further than generally believed, as successfully presumed throughout an earlier\textsuperscript{38} (1991) trigonometric DIO reconstruction of Hipparchan lunar orbits, discussed below, in §D and fn 38&39.

\textbf{NB:} We now have consistent confirmatory double-evidence for Hipparchos 1\textsuperscript{st}-accuracy: [a] Table 1’s 18\textsuperscript{th} klima, as just noted (fn 37). [b] Below fn 46’s neat hits for attested &cr.

\textbf{C13} Ironically, Duke’s rejection (fn 69 below) of that reconstruction unintentionally highlights the Hipparchan trigonometry tables’ accuracy. It just seemed incredible to Duke that DIO’s analyses (precised at [§D] here) of Hipparchos’ \textit{Almagest} 4.11 ellipse-trios could possibly be reliable, from sensitivity to tiny uncertainties. Yet we have multiple-verification of those analyses’ validity, because Hipparchos’ calculations (fn 24&52 here) & trigonometries were more accurate than previously believed by Duke or anyone else (including DR, before 1991 testing). So his/kathers’ new lunanautical doubts thankfully just emphasize the shockingly newness of the discovery of Hipparchos-era 1\textsuperscript{st}-accuracy computation & trigonometry tables: [i] the two ellipse trios (customarily called “A”&“B”) confirmed each other by both producing Hipparchan orbits based on Kallippic motion (period 365\textsuperscript{2}/4) [2] new papyrus testimony vindicated in 2005 (§F4) both of the novel inductions of DIO’s 1991 analyses: [A] Hipparchos’ search for a –157 Summer Solstice, [B] his contemporary passing adoption of Kallippic solar motion.

\textbf{C14} None of these confirmations could have succeeded unless Hipparchos’ trig tables were indeed (as already indicated: fn 37 below) accurate to 1\textsuperscript{st}, as later were Ptolemy’s (\textit{Almagest} 1.1). Specifically, if DIO’s 1991 elicitation of lunar eccentricity e from Trio A had computed with a trig keg function by 1\textsuperscript{st} (from slightly unreliable trig tables), Hipparchos would have found other\textsuperscript{13} than e = 327/23, the correct value, which is attested (§D1 below) & is found to agree (§D3) with calculation via 1\textsuperscript{st}-accurate trigonometry tables. To repeat (§C12): [i] the Diller-klimata table’s 18\textsuperscript{th} entry (here, in Table 1 & fn 37) and [ii] DIO’s eclipse-pair-based orbit reconstructive matches, both consistently establish the 2\textsuperscript{nd} century BC as the earliest date we know 1\textsuperscript{st}-accurate trigonometry tables existed.

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

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

\textsuperscript{36} This, in a familiar chauvinist tradition we keep encountering here, e.g., in fn 258&85, and even more astonishingly at www.dioi.org/hr/hmftcscy, and below in §J. Computing odds against Jones’ theory (\& Neugebauer’s): Rawlins 2009S §§J1, J3, & J6.

\textsuperscript{37} At the 18\textsuperscript{th} klima in above Table 1, 58\textsuperscript{2} of 58\textsuperscript{2} rounds to matching 58\textsuperscript{1}/4; but 58\textsuperscript{2} 29” wouldn’t.


\textsuperscript{39} Ibid §N14. Further deep thanks (not necessarily facetious) to our loyal royal cavaliers: here at fn 10, 51, 71, 73, & 98; also §2 fn 42, and www.dioi.org/jb12.pdf, “Gratitude to Opposites” p.10.

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

Among the gymnastic hysterical-astronomy prattfalls 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 \textit{op cit} n.50 (p.275) presents his own non-teleological (cross-staff) 1981 July 16 Sebastian observational determination of the longitude of a star (\lambda 5gr) by using a lunar eclipse (as Hipparchos had) [measuring the star’s angular distance from the Moon when [it was] 180° from the Sun’s already-tabulated position] — which after Evans’ reduction produced a longitude erroneous by –2°3/3, thus according to him [idem] showing that the huge errors in some ancient observations were indeed (so many historians of science) that such were a poor basis for learning anything about ancient science [i.e., condemning Ptolemy’s gross errors]. As further examples, Evans specifically mentions (\textit{idem} & p.235) Hipparchos’ two hugely disparate Spica data [also eclipse-based],. . . which disagree by over 1°. He then draws for us a [Ptolemaist] lesson (empd added): “No better demonstration could be wished of the uncertainty attached to the method” of fixing stars’ longitudes by eclipses. However, when instructor Evans repeats the very same sermon (on Hipparchos’ eclipse-star errors) \textsuperscript{11} later [in many-ways-valuable J.Evans, \textit{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 interval) Evans had read [the 1991 revelation]\textsuperscript{10} . . . that DR had discovered


\textbf{C12} New papyrus testimony vindicated in 2005 (§F4) both of the novel inductions of DIO’s 1991 analyses: \[A\] Hipparchos’ search for a –157 Summer Solstice, \[B\] his contemporary passing adoption of Kallippic solar motion.

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that Evans’ and Hipparchos’ errors . . . WERE NOT OF MEASUREMENT BUT OF BASIC SPHERICAL-ASTRONOMY MATHEMATICS. . . . [Yet] when ineducable educator Evans’ 1st-hand evidence somersaults, he just pretends he was right anyway, unable to admit DOI scored & “premier” JHA bellyflopped . . . Contra Evans, neither his own nor Hipparchos’ problems were observational. Both simply misconceived . . . valid observational data by using invalid math: the wrong sign for their parallax corrections . . . the [1981 Seattle] longitudinal lunar parallax $p_\lambda$ was virtually $1°/3$. . . . the sign mixup would naturally cause an error of . . . $-40°$, & the laughably $90°$ change here in fn 12 & [§10.81] visibly he didn’t have his klimata data. He did, indeed (Evans 1987 loco 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 best ancient . . .


More Evansience appreciated in Rawlins 1992T §§[HI-H7 & 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 Hoskin as JHA Editor. Utterly apathy. Intermittent listing at www.dioi.org/HHharm, but list of other examples of deliberately-ignored (recall 1994/2 letter, above) revelations of undeniably erroneous but never-retracted Evans scholarship. See also DOI 8 §4 fn 4 on the unsuitably-‘adventurously’ non-empirical—adventurously non-direct discovery ([§4], DOI 1.1 §7 [C1]; Rawlins 1991W §R9, fn 263 &272; Rawlins 2008R fn 17): Aristarchos’ 8th½-Moon elongation as not precise but a lower limit. And don’t you forget www.dioi.org/jhb.htm#congm, on Evans twice copying J. Dreyer’s prose without quotation-marks. Irony: DOI’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 Muffa at Rawlins 1991W §S3.] Evans’ modesty precludes him from citing any of these thank-yous. Compare to DOI’s rule of always correcting its errors — for both integrity and refusal to mislead readers: www.dioi.org/er/htm, as well as below at §14 & fn 98k &110, also DOI 1.1 §4 [A2 (1991) & DOI 11.2.3) (2003) front cover & p.30. JHAM’s perverse reaction to the contrast neon its priorities for all to see. And evermore.


12 Rawlins 2009E fn 5: “E.g., Rawlins 1982Gp 263 & n.17 (dissect by Swerdlow, rejected by 1°

C9 JHD’s 2002 switch from Neugebauer’s half-fit, to Jones’ even-worse-fit (fn 33): a cornered cult flexibly changing-its-story (& data) to continue inflexibly rejecting Diller. C10 JHD-unrealized: Hipparchos’ data have unexpectedly SPECTACULAR sensitivity to even the most minuscule33 imperfection in the ε or the constant-shift $A$ assumed for testing fits, which renders it especially astonishing that the Diller-Rawlins theory (even without help from an arbitrary [Jonesian] crust-to-resort to a constant-shift) accords with all 14 data. (Superior even to the mathematically best-fit solution found by least-squares, which fails at klima 14°/4.). Jones’ premises, [i] Hipparchos’ ε was Eratosthenes’, and/or [ii] Strabo’s data need alteration by $A = 100$ stades, overkill-wreck any ‘observed’ $\pm 0.001°$ he ‘obtained’, with $A$ so radically he didn’t know how $\pm 0.001$ to run the required least-squares (a disability seemingly near-universal among historians-of-science: fn 34 &100) — much less the multivariate version. We have attempted enlightenment by devising a simple method of estimating bivariate-probability loci (fn 33 here), comparing the klimata analyses of Neugebauer & DOI: www.dioi.org/sta.htm#xmxw. Even accepting one of Jones’ two premises & using least-squares to adjust the other unknown, several non-fitting klimata ensue, as emphasized just because he didn’t know how $\pm 0.001$ to run the required least-squares — a (usually) multivariate model, which Jones calls “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 best ancient . . .
of Hipparchos. Jones does not merely pollute Strabo’s klimata with these alien data from scattered other works of Hipparchos, but uses them as an excuse to *shift the entire dozen*, suggesting Strabo’s data could have been anciently corrupted by addition of a constant, A = 100 stades (meanwhile Jones consistently miscomputes Syracus’ latitude by 200 stades), which he argues must be corrected-for — thereby justifying-excusing his replacement of already-perfectly-ﬁtted unmanipulated data, with sub-50%-ﬁtted (Table 1) revised data. Seeing Jones extrapolating from a very few exceptional klimata to the whole set, atheist DR is reminded of creationists who generalize from rare, anomalous geological strata to rejecting natural selection entirely. Obvious point against the signiﬁcance of Jones’ disparate data-injections: he well knows26 that Hipparchos changed adopted parameters over time.

C6 Further, Jones (ignoring 2°23′/3′’s conﬁrmations: [JH; Rawlins 2009S fn 23&54] adopts Ptolemy’s 1.12 testimony that Hipparchos’ obliquity was Eratosthenes’ $\epsilon = 23°51′20″$, a value which Jones imaginatively attributes to a speculative Hipparchean computation from a conjured-up non-Hipparchan Alexandria $L = 31°$, without realizing that obliquity $\epsilon$ would already be known since ancient Strabo found it concurrently with $L$ — and via solstices, not as Jones curiously assumes27 equinoxes.

C7 Jones also-traditionally alters28 the *Almajest*’s text for Hipparchos’ Marseilles latitude from, $L = 43°0′4″$ (consistent with $43°1/2$ of the *Tolemaic Geographical Directory* [GD]) to $L = 43°0′1″$, in order to reconstruct (using Eratosthenes’ $\epsilon$) 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, $41°4/5$ (whose precision argued 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 hoake* 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 (Neugebaureanly: §C2) lodges his half-ﬁtted theory — unvetted (§C10) and untabulated-whimsical — as SUPERIOR to Diller’s ultimately-perfectly-ﬁting one. Though willing to refer to Diller’s 1934 paper (with 2 nonfts of 11 listed klimata, until DIO’s 1994&2009 upgrades: $\S$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 [Isis] & 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 blemishless-perfection (Table 1 here) of Diller’s 14-for-14 victory is too irrefutably to deal with. Except by fleeing.

B5 (By 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 awful-looking Hipparchos lunar-eclipse star-placement measures cited above were accurate to ordmaga $1°$ if his parallax-corrections were correctly signed; [ii] further, if Hipparchos’ hitherto-unexamined only other eclipse (~140/1/27) was used to ﬁx nearby Regulus, undoing the very same parallax-sign-error shrinks the same apparent ordmaga $1°$ error (common to all 3 mis-longitudes) down to just a few arcmin (7′, mostly rounding error), yet again.

[Note: All data are subject to trivial error from ordmag 10⁶, mostly non-observational: Rawlins 2018U fn 11 below; and colleagues’ cooperative averted gaze (fn 11 above) is consistent with the dreary theory that they prefer their own ubs 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 Hipparchan geographical latitudes $L$ corresponding to what ancients called “klimata”.33 narrow referee Toomer, Rawlins 1985G passim. [Rawlins 1985H,] Pro-Greek-competency: DIO 1.1 (1991) fn 24. Hipparchos measures of lunar limb-vs-Sun separation (Almajest 5.3&5), mean error $0°1$: Thurston 1998A 01. For Rhodos equinoxes, Hipparchos’ scatter (mean single-datum error) was $2°$: fn 70 here. With error $0′+1′$ (Rawlins 1994L, §G3), he found his geographical latitude $L$, presumably from polestars, knowing stellar parallax was negligible. His $L$ is inferable from his star-declinations, which show merely $5′$ scatter (here in Table 2). Regulus-restoration: Rawlins 2009E eq.8. Correcting the four-star-places discussed here, for proposed parallax-sign-slips & for Hipparchos’ PH orbit’s shortcomings (at that era, primarily an error wave of amplitude $0°4.4$), the above-$\beta$’s exact before-correction-vs-after data are found here at $\S$B7 (or at Rawlins 2009E fn 22). Evans’ refusal to recognize that DIO’s analyses have ordmaga-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 [34 fn 48], & unprecedentedly insufferable quadrupled-lése-majesté.

13 Familiarity with klimata is vital to understanding the disgraceful ordmaga $1°$ inaccuracy of Polymag’s geographical latitudes. For the purely astrological Hipparchos cause, see, e.g., here at §I 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-finesse of sundials/kastroubles used for timing eclipse-start/ends), to ﬁnd longitudes to a mean accuracy of ordmaga a degree ($\S$11 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 ancient writers were so incompetent that eclipse-based longitude-differences, as reported in Kleomedes&Pliny, were “badly overestimated”. But see DIO’s 2017/3/20k Letter-to-the-Editor (published here as article §J, 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
constant-L strip-regions extending east-west around the Earth, sharing the same longest-day $M$ — which is mathematically determined by $L$. Since klimata were used for Hellenistic horoscopes’ house-divisions, ancient astrologers tabulated them for every quarter-hour or so of $M$. (Klimata tables are found, e.g., in Almajest 2.6. And phenomena are tabulated according to klimata at Almajest 2.8 for rising-times, at Almajest 2.13 for paralax.) In 1984 the eminent planetologist Diller suspected a mathematical error, and discovered, unaided, epochal discoveries: [1] all Hipparchos’ klimata were computed via spherical trigonometry, (which in 1934 scholars doubted was available as early as the 2nd century BC), [2] using an obliquity untested but the most accurate anciently adopted: $23^\circ/23'$.

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 historical irony; his mathematical mis-appraisals: details in fn 97 below.

From H.Bucke History of Civilization in England 1873 (1:318-320). In medieval times “the credibility 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 Fenace to no-decision until he “engaged his adversary in a theoretical 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-respecting SCIENCE journals (12 in 11) disingenuously dismiss Diller’s recognition (uncited by Neugebauer’s prot´eg´e & eulogist Swerdlow 2010 p.151) wasn’t a gnomon shadow-ratio at all, but the Alexandria klima’s shortest:longest-day ratio, ignored (favoring Neugebauer 1975 p.101 n.1) by Jones and the N-Y-A journals, which discerned in Table 1 of www.dioi.org/jg03.pdf, GD Jones’ 1934M Table 1, augmented in later printings, as previously unnoticed Strabo klimata surfaced, each unfailingly consistent with Diller’s theory.

Most Neugebauer recipients the 14th Diller-Rawlins klimata table (Rawlins 1994M p.56). All ducked, with an unwillingness to acknowledge Diller’s vindication (or even a minum of merit) that was unabashedly, unblemishly unanimous, even Britton: DIO 16 p.2 (2009). (See also Dicks 1960 pp.102-104, written after Dicks escaped the Neugebauer cult.) Neugebauer 1975 p.334’s half-truth theory is directly promoted by Toomer 1984 p.19. (F.Ragep 2010 pp.128-130 omits Diller and DIO.) Do those obscuring Diller’s 1934 view even care about the dirt done a refined, dedicated scholar? During decades of insisting on fitless joke-mathematics, to deny him credit for his most brilliant strike, did any feel a tremor of human pathos? — when reading (Rawlins 1994M fn 7) of Diller’s late-life lift (at age 76) while his immense discovery was “rescued 45 years later [of his life having passed] by a phone call from a stranger [Rawlins] in San Diego.” See Boltzmann parallel at fn 108.


16 Rawlins 1944M Table 1, augmented in later printings, as previously unnoticed Strabo klimata surfaced, each unfailingly consistent with Diller’s theory.

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18 Rawlins 1994M fn 10 and Table 1.

19 Table 1 col.3 = Diller 1934 theory’s unrounded $L$, nonfits (ere col.4’s 5 rounding) there underlined.


21 Diller 1934 Table 4. The 14 klimata: Equator, Cinnamon Country, Meroe, Syene, Lower Egypt, Phoenicia, Rhodes, Hellas, Massalia, Pontos, Borysthenes, Tanais, South Britain, North Britain.

22 Jones’s cited Strabo 2.5.38 Alexandria gronom ratio 5.3 is just a common textual alteration: the original Greek is 5.7, which E.Honigsman&Neugebauer realized (uncited by Neugebauer-prot´eg´e & eulogist Swerdlow 2010 p.151) wasn’t a gronom 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 Cartaghe GD latitude (Rawlins 2009S fn 35): where same $m:M$ mixup with shadow-ratio (in same Strabo 2.5.38) caused 1000 years of North-Africa latitudinal map-distortion, (Rawlins op cit fn4; similarly durable longitude-disaster for Arbeia eclipse: Neugebauer 1975 p.938.)