DIO

& The Journal for Hysterical Astronomy

Is a University Enhanced by a History of Science Department?

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

Download *DIO 22*, www.dioi.org/vols/wm0.pdf.

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

This *DIO* is formed of papers (‡1-‡3) on new discoveries and academic crimes, sent to centrist journals backed by major universities' scholars (& ‡4, on exchanges with one), invited to edit-out any parts they disliked. None found errors of math, science, fact, or other, *instead just cutting contact*, Boss-Tweedily sneering in-effect: whaddayagonnadoaboudit? Has academe become home to those who ignore colleagues' sins so long as their own funds flow? Papers ‡1&‡3-‡4 detail credit-theft from creators Gosselin, Delambre, Diller, Newton, Thurston by: mis-attack, lockstep-cult-noncitation, data-tampering, grab, shun, fake forever-controversy (‡2 §§H2-H3, ‡3 §C10, ‡4 §B4), *endangering knowledge-advances*.

Leaving each paper nearly[1] as submitted entailed overlaps, but reflected how many forums ignore corruption & flee demos of poor stats (‡3 p.44, §F8, fn 100; ‡4 §§C11&C19), abysmal science (‡2 §N7, ‡3 §I7), esp. scientists' *attitude*. (Ponder Ragep's inversion at ‡3 fn 9!) No hist.sci forum faces its field's hijinks&lojinks, nor suggests remedy-path, despite request for such, www.dioi.org/isw9o.pdf, so degeneracy persists, ***disgracing even CalTech, Princetitute, NYU*** (‡3 fnn 9&96, Table 1, §§C5-C11, resp). We value (e.g., p.44) History-of-science's mining of new ore but regret when sacredcowed minds warp its use.

DIO Publisher DR's modest qualifications for these critiques include researches (unexpectedly later-vindicated scores of times: www.dioi.org/vin.htm) appearing in such forums as *Nature*, *Amer J. Physics*, *Astronomical Journal* (AmerAstrSoc), *M.N.Royal Astronomical Society*, *P.A.S.P.*, *Isis* (Hist.sci.Soc), *Vistas in Astronomy*, *Geophysical J. RoyAstrSoc*, *Norsk Geografisk Tidsskrift*, *US Naval Institute Proc.*, *Archive for Hist. of Exact Sci*, *Astronomy*, *Queen's Quarterly*, *Sky&Tel*, CBS-News & page-one *NYTimes* fraud-exposures (p.44 here).

This *DIO*'s & others' bluntness has little to do with why historians-of-science flee us. [1] Calm scholars Johns Hopkins physicist R.Newton (Applied Physics Lab Space Sciences Supervisor) and Indiana University philologist Aubrey Diller (long world's leading expert on ancient geographical mss) were gentle but (**SEE** www.dioi.org/ns.htm) shunned&smeared anyway for heresy. (Among others: p.44.) Each's final paper appeared in *DIO*: 1991&2009. [2] In 2017, *DIO* told (e.g., ‡1) toppe history-of-science journal *Isis* of [a] its 2015 theft, www.dioi.org/isa.pdf, of an original *DIO* 2008 discovery (residing for 7[y] in ordmag 100 libraries worldwide), & [b] its 2016 laughably math-bungled (‡1 §D) attacks on DR's 1984 Greenwich Centenary conference paper; pseudo-refereeing of *Isis*' assault is Hist.sci-typical, as inside-witless confirms (*JHA* **breakfast→lunch refereeing**: ‡3 fn 4's finale). Angered by submission to *Isis* of an ultimately mild version (www.dioi.org/qjo.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/jhb.pdf, & pretend-refereeing (‡3 fn 66) journals? Are they exaggerating our import by cowering so transparently? The reader may judge.

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[1]Main changes: *DIO*'s letter to *Isis*, www.dioi.org/islg.pdf, has minigrown to ‡1; §N8 added to ‡2; & ‡3 is a plainly blunter version of www.dioi.org/qjo.doc (sent *Isis*), but has virtually the same content.

‡1   Ancient Accuracy Vs History of science Society

To *Isis* Editorial Board: 2017 March 20 & April 1

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

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

Your 2015 March issue's *lead* paper "The Two Earths of Eratosthenes" by C.Carman & James Evans [University of Puget Sound] *Isis* 106.1 pp.1-16 [advised by NYU's A.Jones], www.dioi.org/cev.pdf, is founded totally (abstract-to-conclusion) upon the theory that, though Eratosthenes' legendary Aswan-Alexandria experiment yields Earth-circumference $C = 250000$ stades for the Sun at infinite distance, it yields $C = 252000$ stades if parallactically adjusted for Eusebius' finite Sun-distance of c.100 Earth-radii. But this result had already been published in uncited "Eratosthenes' Too-Big Earth and Too-Tiny Universe", *DIO*, 2008, *14* ‡1 fn 6, www.dioi.org/je01.pdf, explored as an alternate explanation, even though reasonable traditional theory is that, whatever its origin, C was finally adjusted to 252000 stades so that $1° = 700$ stades, Eratosthenes' standard scale (Strabo 2.5.7).

Your 2016 December *lead* paper, www.dioi.org/shc.pdf, "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 §G 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 astrologers mangled them, accurate maps existed in antiquity with longitudes based on lunar eclipses. Shcheglov calls such maps a "delusion" since eclipse-use is "impractical", citing in support "badly overestimated" (Shcheglov 2016 n.8) eclipse-based longitudes of Kleomedes, Heron, and Pliny. But Heron is long known to be irrelevant; and Shcheglov miscomputes (§D below) the other two by treating a Pliny solar eclipse as lunar, and by putting Kleomedes' Spain in the wrong hemisphere (likewise for Xi'an & Luoyang: see *POSTSCRIPT* below).

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

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, datafitting hypothesis: that the 1.4-factor error (40% overestimate) in Ptolemy's Earth-longitudes was from just multiplicatively stretching them to shift from Eratosthenes-*Almajest*'s 700 stades/degree to Marinos-Ptolemy's 500 stades/degree. Shcheglov discards the 1.4-stretch theory by claiming that his true explanation for the 1.4-exaggerated longitudes "proves to be much more complex and intricate" than 700/500 simplicity. But nothing approaching the promised "proof" of the need for complexity ever actually appears in the article, where most complications are gratuitously, artificially injected, by his own myriad diversions from Occamite simplicity, and in his 20pp *he never derives his 1.4-grail any other way* (than a plain stretch), so he finally urges "further studies." Whose results can never work as efficiently as plain, raw multiplication.

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[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 R.Newton's criminal charges versus Ptolemy. Though I agree Ptolemy faked I've never said his stretch was anything but a bad mistake (end of §F below). [c] Ignorance of alternate theories is implied (vs *DIO 6* [‡1] fn 47, *DIO 20* [‡1] fn 2). [d] I'm mis-said (p.693) to claim accurate land-surveying underlay Ptolemy's longitudes. (My spare proposal was a simple longitude-multiplication, without any connexion to Shcheglov's amazing & valuably complete reservoir of centuries of stadelength guesses.)

B   Shcheglov (p.705) calls early accurate geography "a quaint 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 effected 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 §F; *JAHH 17* p.326)
$0°.1$ for star declinations (ditto)
1% for Earth-circumf. precision (*ArchiveHistExactSci 26* p.216; *DIO 14* ‡1 §A & eq.28)
$1'$ for Earth's tilt or "obliquity" (*Klio 27* p.266; *DIO 16* ‡3 §§A-B eq.2 and Tables 1&2)
1% for lunar mean distance (*Almajest* 5.13-17; *DIO 8* ‡1 §H4: 59 Earth-radii vs really 60)
10^m for lunar-eclipse-prediction (*DIO 1.1* ‡6 eq.32)
1^m for time of lunar eclipse (Greenwich 1984 in *Vistas in Astronomy 28* pp.258&265)
$0°.1$ for lunar limb vs Sun separation (*DIO 16* ‡1 fn 24)
ditto or even $1'$ for star-vs-eclipsed-Moon gap (*DIO 1.3* fn 288; *DIO 16* ‡1 §A fn 22)
1^h for solstices (*Bull.A.A.S. 17.2* p.583; *DIO 20* ‡2 eqs.21&25&Table 3; *P.Fouad* 267A)
$1'$ for −145/3/24 equinox on Alexandria Palaestra polestar-set ring (*Isis 73.2* p.263 n.17)
10^s for sidereal year (*DIO 6* ‡1 fn 38&§I7; *DIO 9.1* ‡3 Table 2; *DIO 11.1* ‡1 fnn 14-15)
$1'$/century for mean motion of Mars and arguably Venus (*DIO 11.3* ‡6 fn 26)
$0^s.1$ for synodic month (*DIO 6* ‡1 eq.2 & fnn 12&18; *DIO 11.1* ‡1 eqs.1-8)
1^s for anomalistic month (*DIO 6* ‡1 eq.13 & fn 12; *DIO 11.1* ‡1 §A3 & eq.2)
$0^s.1$ for draconitic month (*DIO 6* ‡1 eqs.2&19 & fn 12; *DIO 11.1* ‡3 eqs.1&3)

Most historians-of-astronomy are, like Shcheglov, unaware of these symptoms of high Greek science, some, e.g., Gingerich&Swerdlow, *speculating* **without attestation** [‡2 §M2] that scientists kept only theory-accordant data [**flatly contradicted by Hipparchos' record**: ‡3 fn 8], thus unwittingly modeling all ancient science on a blundering *astrologer*, Ptolemy. QUESTION: *how could the above-listed measures have ever progressively evolved into accurate achievement* **by following a tradition of just keeping on confirming prejudice**? [Classic projection from own behavior? See below *POSTSCRIPT*'s final line.]

C   That ancient geographers' longitudes were based on eclipses is doubted by Shcheglov p.690 as "too impractical". I've outdoor-eyeball-timed enough lunar eclipses to know their accuracy is ordmag 1^m [anciently somewhat vitiated by sundial graduation limitations, ordmagly] agreeing (at $4^m/1°$) with the well-under-$1°$ accuracy of pre-stretch *Geography* longitudes, D.Rawlins 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, *Almajest* 4.6), Shcheglov's n.8 accepts **INDOOR** (Rawlins *op cit* §10) astrologer Ptolemy's giveaway-incredible claim (*Geography* 1.4.2) that few eclipse data were available. But outdoor Hipparchos (Strabo 1.1.12 or Shcheglov, n.7) says nothing for rarity or impracticality, instead recommending eclipse-comparison as the best method for scientific longitude-difference determination. Yet, revealingly, the sole eclipse-pair Ptolemy provides (*Geography* 1.4.2), to illustrate this central method, is *half a millennium old*, the −330/9/20 "Arbela eclipse". Ptolemy's reported time (longitude) gap is 4/3 too big, so Shcheglov's n.8 tries alibiing 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^h Spain vs Persia; Heron 2^h Rome vs Alexandria; Pliny 3^h Campania vs Armenia (4th pair: Pliny's correct 2^h 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^h gap is virtually correct since Cadiz at 25^mW longitude and Persepolis at 3^h32^mE are 3^h57^m apart. Shcheglov just mis-signed Cadiz and found 3^h07^m (comfortingly consistent with Ptolemy's false 4/3 factor

for his Carthage-Arbela gaffe-gap). As for Pliny 2.72.180, Shcheglov knows Campania & Armenia are c.2^h apart. [Longitude gaps between Naples & the *Geography*'s Armenian cities (Diller *DIO 5* Table 17; 1984) Dioskourias, Artaxata, Gaggara are 1^h47^m, 2^h01^m, 2^h23^m, respectively, all indeed about 2^h.] So Shcheglov concluded that Pliny's 3^h is too high. Yet Pliny doesn't say the Campania-Armenia longitude gap is 3^h but that the eclipse was seen 3^h 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^h29^m, 2^h48^m, 3^h14^m, respectively, mean 2^h50^m. So Pliny's 3^h was not "badly over-estimated". (Neugebauer, *op.cit.*, p.668, 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 Arbela lunar eclipse Ptolemy mis-reports as 8 P.M. at Carthage, 11 P.M. at Arbela. But Pliny has the same event 6 P.M. at Sicily (west Sicily was under Carthage then), 8 P.M. at Arbela, resp, both times correct within minutes. Shcheglov n.8 doesn't connect the two Arbela-eclipse reports; & neither he nor any other historian-of-science has noted that "authoritative" (§G) scientist Ptolemy has accidentally misassigned Arbela's 8 P.M. to Carthage! A check of his probable source, Pliny *loc.cit.*, reveals how: by grammatical accident, Pliny's Latin sentence places 8 P.M. nearer Sicily than Arbela, while 6 P.M. is expressed as a word ("moonrise") not a number. Unequal to the Latin, Ptolemy thought 8 P.M. was Carthage time. Since his 4/3-stretched map already had Arbela 3^h east of Carthage (real gap $2^h1/4$), he faked Arbela thusly: 8 P.M. + 3^h = 11 P.M. This, THIS! is the prime astronomical observation in the *Geography* of Neugebauer-Evans-Gingerich's Greatest Astronomer of Antiquity? Ptolemy's times mega-disagree with not just reality but *his own tables*: 2^h-3^h! [Error about as big as quantity sought: like ‡3 fn 42.] Shcheglov notes no discords nor Arbela-Carthage-mixup, though all are at fn 45 of the same paper, www.dioi.org/je03.pdf, he's uncomprehendingly consulting in his nn.12&15.

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 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 *dim*onstrably unreliable on his own, why doubt that, when switching to 500 stades/degree, he was so isolated from scientists (see above [§§C&D] innocence of ordinary eclipse data and use thereof) as to believe that his source map's longitudes were overland-distance-based, so that he needed to multiply by 7/5 a traditional 700 stades/degree globe's longitude-degrees? Any real astronomer knew the degrees were based on eclipses and should be left alone.

G   So Shcheglov's n.16 calling Poseidonios a dilettante but Ptolemy an "astronomer" and (p.694) a *geographical* **authority** is Quaint at best. Unmentioned in Shcheglov's attempt to convince historians-of-science that Ptolemy should be somewhat "rehabilitated" (p.687):
[1] He "usurped" Hipparchos' 1025-star catalog (Tycho Brahe, *Omnia Opera 3*, p.337).
[2] "Astronomer" Ptolemy's four allegedly outdoor solar observations are **fifty times** closer to his 280^y-old indoor Hipparchan tables than to the sky. (Hipparchos' ratio is less than 3.)
[3] His adopted latitude 30°58' was −14' off reality, vs just 0', 3', 0', 3' errors for adopted

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[2]Shcheglov's solar-eclipse misadventures [are similar to NGS-NavFou's at *DIO 21* ‡5 §§B3-B4, www.dioi.org/jl05.pdf, also] reincarnating a backfired attempt to empirically justify demeaning Greek accuracy: Evans' 1987 claim that his 1981 measure of a star's distance from the eclipsed Moon was ordmag $1°$ off, like Hipparchos' two bad measures of Spica. But undoing mis-signed parallax lowers all three $0°.6$ errors to $0°.1$ or less; same for Hipparchos' $-35'$ Regulus error, so odds against **all four** errors' being outdoor are astronomical. Evans won't reveal his 1981 data; but his 1998 book repeated his 1987 analysis, a non-observed 1977 eclipse quietly subbed for the 1981 event! Sly details at *DIO 16* ‡1 §A & fn 7. [Shcheglov, NavFou, & Evans have in common that all 3 are self-torpedoed by innocence re parallax, as was early Hipparchos (vs his consistently accurate later work: ‡3 §B8).]

latitudes of real observers Timocharis, Aristyllos, Hipparchos, & Ptolemy's Anonymous.
[4] *Almajest* 10 reported Venus' 136 greatest elongation for two different dates, 37^d apart.

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

I   Shcheglov admires *Geography* latitude-accuracy (p.689, emphasis added): "Methods for determining latitude, being rather simple, had [long] been known in Greece . . . . By Ptolemy's time, latitudes of a number of the most important cities had been determined (e.g., Alexandria, Rhodes, Athens, Rome, Massalia) . . . Ptolemy calls such cities . . . *'foundations' that should be used* **as reference points** *for developing the rest of his map*." No mention that all five "foundation" 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 (*Isis* 1982; & *DIO* 1994, thrice cited in "Secrets", which Shcheglov read), Y.Maeyama (*Centaurus* 1984), & J.Brandt (*JAHH* 2014) show that all 4 **real, non-amateur Greek scientists** cited above at §G [3] knew their latitude to ordmag 1′. This twice-confirmed Rawlins discovery undoes Shcheglov's entire inaccurate-geography thesis. He doesn't mention it. Nor does he mention the contradiction it obviously creates versus the *Geography*'s **mean latitude error of ordmag 1°**. In response to the disjunct, one *JHA* Editorial Boredperson has offered that geographers must have ignored astronomers! (So, did astronomer-geographer Hipparchos ignore himself?) DR mathematically contends ("Achievement" pp.260-264) these 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 inconsistent with the astronomer-scientist Shcheglov sees Ptolemy as.

J   "Achievement" p.262 lists 17 cities where, in the *Geography*, latitude matches klima. Dropping notoriously-flawed Bithynia (*DIO 20* ‡2 §L4) & way-south Meroë leaves 14 cities.

K   Selling or owning klimata tables for every latitude-degree was impractically voluminous. [Thus, if *Almajest*'s 1^h/4 klimata-interval was adopted, then each city whose longest-day was closer than 1^h/8 to a klima was grouped under it, its latitude made equal to exactly that klima's latitude.] Such groupings of cities under ONE latitude is explicitly attested at *Geography* 1.4.2 (even while justly criticized at *ibid* 8.1.1; different authors, in all likelihood). Effects of such data-tampering are obvious from errors found [in §J's sample], which aren't ordmag 1′ (as expected if due to real astronomers, like those of §G [3]): 64′, −43′, −251′, −30′, −26′, 148′, −40′, −59′, −84′, −108′, 38′, 204′, 10′, 124′. Dropping −251′ (confused Carthage mis-latitude: *DIO 16* ‡3 fn 43; 2009), rms error is 93′; but the (more reliable) median is 59′, hinting both are skewed high by a few goofs.

L   Given this mess, one might ask: who says there ever were accurate ancient maps? We reply by turning to the same 14 latitude-awful cities' longitudes, and receive a shock. (Sample originally compiled in 1984 for another purpose so not prebiased for longitudes.)

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

N   Dividing 4/3 into §J's 14 *Geography* degree-longitudes vs Alexandria, to unstretch them: those 6 cities within 30^m of Alexandria show rms longitude-error c.2^m, or about half a degree. The other 8 cities, several of them ordmag 1000 miles from Alexandria, likewise show rms error 2^m. Same 2^m 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.

O   Errors' small size is apt (*ibid* p.258) to longitudes based on accurate eclipse timings. *As is their remoteness-independence* (§N), since the error in local-time difference for

eclipse observers longitudinally 1° apart is no more or less accurate than for 100° apart. Which is why the unstretched 42° from Carthage to Persepolis is correct to ordmag 1%.

P   It should be noted that sampling here has ignored some civilized areas (e.g., the western Mediterranean) that are not even close to according with 4/3. But this anomaly can perhaps help date the original map through testing when nonfitting regions came under the rule of Alexander's successors: was the original earlier? But that would not explain why London is in perfect accord with 4/3-stretch. I leave these tantalizers to other investigators.

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

R   Contra *Isis*, the data are consistent with early currency of astronomically-constructed, accurate pre-*Geography* maps, which professional astrologer Hipparchos semi-**randomly** ruined through doctoring latitudes by lumping them into discrete klima-cubbyholes where all cities in a cell are force-assigned the same latitude (§K; [*Geography* 1.4.2]; "Achievement" p.261; "Secrets" §D) for handy astrologer-access to inevitably-too-widely-spaced klimata tables: *Almajest* 2.6. (Three centuries later, professional astrologer Ptolemy ruined longitudes **systematically**, stretching them by factor 4/3 or 7/5. Summary: ‡3 §I11.)

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

T   The history-of-science enterprise is proud of being nonjudgemental, e.g., of superstition. It rejects any implication by astronomers that Ptolemy's occult profession lessens him. Ironically, this well-intended discipline has long blinded the field to the obvious: just as his *Tetrabiblos* was his religion's handbook for horoscopic interpretation, his *Almajest* and *Geography* were also world astrologer-handbooks. (The 1st fully competent translations of *Almajest* and *Geography* called each a "handbook". Ptolemy's exact title of what most now call the *Geography* was actually *Geographical Directory*, as *DIO* routinely calls it.)

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

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

W   Both *Isis* papers cite D.Rawlins 1982, "The Eratosthenes-Strabo Nile Map. Is It the Earliest Surviving Instance of Spherical Cartography? Did It Supply the 5000 Stades Arc for Eratosthenes' Experiment?", *Archive for History of Exact Sciences 26.2* pp.211-219. But both fail to mention 3 unmissably central and intensely relevant discoveries in that paper and/or "Too-Big" which *Isis* readers need awareness of:
[i] The Nile Map shows that Eratosthenes' original circumference C was 256000 stades (later nudged to 252000, perhaps for 700 stades/degree-convenience).
[ii] Eusebius' Sun-distance, 4080000 stades, is thus 100r (Earth-radii), in the Aristarchos-

Archimedes-Hipparchos-Poseidonios tradition that too-big-for-precision Sun-distance is a power of 10: their $1000r$ or $10000r$, likely origin of the very idea of order-of-magnitude. [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, *47.2*:126-128, p.127. Cited to discoverer Rawlins for years in the 1990s in the well-known textbook, Halliday, Resnick, & Walker, *Fundamentals of Physics*, as its kickoff example of applied science, illustrated by diagram (plus frontispiece sunset-photo). See also J.Gerver and Rawlins in *Scientific American* 1979 May. But uniformly silent Historians-of-science will not so much as admit the existence of the airbend solution, not even when they have provably read it (e.g., J.Dutka at *AHES 46* p.64, 1993; F.Ragep, *Archimedes 23* p.124, 2010; *Isis* 2015 & again in 2016). But, as we've seen already, *Isis* & Shcheglov in 2016 exceed their predecessors, by showing that all the above-noted scientists & forums are Deluded in finding precise ancient geodesy credible. [Above paragraph's conclusion added 2017/5/24&6/22.]

X   Shcheglov's n.15 cites fn 13 of "The Ptolemy GEOGRAPHY's Secrets", *DIO*, 2008, *14*:33-58, which describes this solution. And "Secrets" seven times cites "Too-Big", our dedicated explanation of the refraction theory, right in the SAME volume 14 of *DIO*. (Also bearing the 2008 *DIO* parallactic derivation [above] of $C = 252000$ stades, which Shcheglov n.14 credits to 2015 runnerups Carman&Evans, never citing "Too-Big" at all.)

Y   Though $C = 256000$ stades is unignorably prominent in all Rawlins material *Isis* cites, none of the authors ever mentions that number or *DIO*'s 1982 discovery of it, nor do any mention that it's 2π times Eusebius' Eratosthenian implicit Earth-radius $r = 40800$ stades: §W [ii], above, finally realized 26 years later in 2008's *DIO 14* ‡1 eq.11.

Z   None notes *DIO*'s refraction solution (6/5, 5/6) their own citations prove they know of. No historian-of-science has ever shown grasp of its physics [‡3 §I26]. Its triple-consistency (to 1%) with both 40%-disparate C (Eratosthenes-*Almajest* vs Poseidonios-*Geography*) AND the 185 meter stade (above), should be known, so that scholars can make up their own minds if it should at last mercifully end-the-endless, the ancient-Earth-circumference debate. If so, the vast literature Shcheglov has unexceedably compiled chronicles 2 centuries of pursuit of a METROLOGICAL-solution chimera, while the ultimately-accepted solution should turn out to be not a complex spliced "chain" (p.705) of stade-juggling ad-hoccery, but one natural (zero stade-manipulation) unifying PHYSICAL theory: refraction by air.

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

POSTSCRIPT [Below items originally "Not for publication" but: why shield shunners?]:
At least six Experts (p.689) vetted Shcheglov. Besides the foregoing weightier problems, how'd the following mostly-minor but mostly-obvious slips elude 6 putative readers?
n.4 line 1: Xi'an & Luoyang are located in China not Montana (wrong hemisphere again).
p.693 line 2: Eratosthenes' Earth-circumference is not 25 miles.
n.19: Engels' central *Am.J.Philol*. vol.106 1985 article is pp.298-311 (as in our ‡3 fn 110).
p.703: Publication date of Pliny's 77 AD *Natural History* is confused with his deathdate.
p.689: Blest Isles' location matches Cape Verde Islands, not the 800-MILE-DISTANT Canary Islands (one called "Kerne" at *Geography* 4.6.33); mistake followed by all for centuries since Ptolemy called one Blessed Isle "Kanaria". (Language over coordinates!) Try a modern map vs *Geography* 4.6.34; or 2008's *DIO 14* ‡3 §F, which Shcheglov read. *DIO*'s Cape Verde Islands discovery just might be cited somewhere, sometime, in *Isis*. What does it say of tradition's grip on the field that this simple fact wasn't noted before?

Afterword: The History of science Society Hunkers and Bunkers

Due to cultish historians-of-astronomy, Greeks' patiently-won accuracy is unknown. (Perverse-ironically: it's widely believed that semi-literate Mesoamericans were better!) Given Editor H.F.Cohen's haughty rebuff (‡3 p.45), a Letter-to-the-Editor (pp.3-8 here), with cover letter www.dioi.org/isa.pdf, was sent 2017/3/20 to the 30-person *Isis* Editorial Board; separate emails to ordmag 10 board-members (requesting all 30 be informed of the letter), including Maria Portuondo (history of astronomy), head of Johns Hopkins University's History of science Department, plus a message left on her answering machine 2017/6/11. No response. (Asked later to review these doings, her JHU colleague R.Kargon [history of physics] pled too "rusty": 2017/9/5.) Having heard from neither Editor nor Board, DR wrote the latter 2017/4/1, www.dioi.org/isb.pdf, hoping (emph in original)

> to encourage communication while correcting [*Isis 107.4*'s] unfortunate December misinformation, unwary *Isis* publication of which might have been avoided, had Cohen possessed the humility to recognize he didn't understand Shcheglov's [2016 December *Isis*] paper *except that it enticingly attacked one who was upsetting[3] Cohen by asking Isis to publish too-accurate criticisms of his fellow pols*. For Shcheglov, Cohen should've sought refereeing from not just the usual suspects but from *DIO* (re, after all, a huge attack on *DIO*&DR) during a period when Cohen was actually exchanging emails with *DIO*, but preferred secrecy. Now, instead of owning to errors, he's coverupping for not just Ptolemy but for Cohen, taking you all into hiding with him.

> The *Isis* board's non-reply so far risks being interpreted as ... doing nothing — about mere plagiarism, and miscomputed demeaning of accurate *and scientifically refereed* [p.45 below] Greenwich-Centenary scholarship .... Less speculatively, we know exactly what Cohen was up to, when he did not tell us he was sending the large paper [www.dioi.org/qjo.doc; now less diplomatically transformed into paper ‡3 below, here] to a referee *until he got a negative report safely in hand* [a report again not evaluating any scholarship]. Does he imagine such transparent tactics are not noticed by serious academe?

> Truthseeking institutions communicate. And will not hide their demonstrated miscalculations. And don't doubly (2015/3 & 2016/12 n.14), knowingly appropriate credit for a (needlessly) rival journal's discovery. If *Isis* does not acknowledge receipt of this letter ... it will be reasonable for previously unenlightened observers to conclude that your society is unprincipled,[4] and you will not hear directly from *DIO* again.

Out of dozens of potential HsS respondents, *Isis*' sole burp was a 2017/4/2 email from former HsS chief Lynn Nyhart (Vilas-Bablitch-Kelch Distinguished Achievement Professor) of the Univ.Wisconsin History of science Department, reading (in its entirety):

> I received your note and have read the attachments. In my view, the decision of what to publish (or not) in any specific case is the prerogative of the editor. So I'm afraid I cannot help you out here. Sincerely yours, Lynn Nyhart

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

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

[4]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 Ptolemist 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/oww3l.pdf, that *JAHH*'s initial request for cuts was made in vain hopes (encouraged by years of Gingerich-circle slander of DR) that *DIO* would adamantly refuse revision or cuts, thus killing the paper without *JAHH* being indictable for censorship. *DIO* instead agreeably refused to fall into that trap; thus, fleeing was *JAHH*'s only escape-option to effect pre-ordained rejection. The 2015/9/30 version follows, very slightly enhanced.]

ABSTRACT

Over a halfdozen simple independent evidences demonstrate that the history-of-ancient-astronomy subfield rulership's decades-long insistence on the integrity of its ultimate icon Claudius Ptolemy has never been defensible by reason. Thus it resorted to other means. A sampling of subsequent chauvinist tactics provides an educational case study in how a subfield can be hijacked for the better part of a century by a determinedly-careerist cult, at the expense of the 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 (§J1 below) of our valued heritage from classical antiquity's mathematical astronomy. Though Princetitute'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 astrology's bible, the *Tetrabiblos*. His employers no doubt appreciated his consistent demonstrations that reality was in *perfect* accord (§M2) with divine celestial theories. However, for centuries, historically savvy astronomers have known that this famous 2nd century AD astrologer-geocentrist-mathematician accomplished said illusion by indoor-faking or plagiarizing all his allegedly-outdoor "observations" of celestial phenomena, to force precise accord with indoor mathematical models (some of which had already existed for centuries) and tables computed therefrom. Ptolemy's depredations even included stealing and mis-precessing Hipparchos' immortal 128 BC 1025-star catalog (R.Newton 1977 pp.239-242), a deed which for over a millennium polluted astronomers' attempts to gauge precession, until Tycho in 1598 detected (Rawlins 1993D fn 141) and threw out Ptolemy's fakes & was thus able *for the 1*st *time in history* to predict star-positions — to ordmag 1′ accuracy! — 100^y in advance (*ibid* Table 23: 100 select stars for 1701.03). But a few invincibly innocent & deeply committed (§B2; fn 11) archonal historians-of-astronomy keep intermittently trying to breathe life back into their longstanding tradition — e.g., Neugebauer 1975 p.284 & Pedersen

1974 p.258 — of imagining[1] (& see Neugebauer 1975 pp.69, 119, 205; J.Evans, 1987-to-

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[1] E.g., the completely-nonbiased-impersonally-neutral-unprejudiced-tripleblind-randomly-chosen anonymous *Journal of Astronomical History & Heritage* referee for this very paper, even after "carefully" reading its array of independent, elementary analyses, is darned if he can see the slightest evidence of fraud, and continues to believe that Ptolemy is "The Greatest Astronomer of Antiquity," impenetrably hoping the paper does nothing more than "contributes to the discussion" — which he is praying will go on forever. After all, eternal indecision on this controversy would allow *JHA* Number-Two Owen Gingerich to keep evading being established as having spent decades secretly (fn 5) attacking the reputations of those who have now been proven right, in a classic truth-seekers-vs-truth-possessors confrontation (*DIO 1.1* ‡1 fn 1), while he has promoted — as The Greatest ancient astronomer — a figure he somehow never noticed was just a faker of truly monumental, indeed (for an academic icon) *epochal* ineptitude [§§C&G3], leaving Gingerich at the last looking like an emperor with no evidential clothes, but doesn't know it because his court jesters keep complimenting his garb. (Is this how he wants to be remembered, just because faith and political ambition overruled reason? And **self-control**.) The referee generously adds that the paper's author is "clearly quite knowledgeable in the astronomical history involved" and "could contribute to solid progress in the field" — while *unprogressively* attempting even now to inaccurately stigmatize doubt of Ptolemy's honesty as an extreme view. His fantasy and demand: after believers' 1/2 century of falsely slandering (fn 35) skeptics — even in anonymous referee reports — and, wherever possible, preventing their getting a hearing (fnn 3&5), these same cult-priests should now be spared a reckoning of appropriately clear, uncensored criticism of their record of evidence-immune pseudo-scientific defenses of Ptolemy; so it would be better if the paper were sterilized of all that might embarrass him and his band of unheavyweight apologists by analysing their half-century of industriously, suppressively, and viciously dig-dig-digging their own ultimate reputational graves. (Question: Why make publication of "solid" research, by a scientist who is "clearly quite knowledgeable," contingent upon his not showing the light side of years of dark scheming against open discourse?) Again: nobody's supposed to laugh? — at a cornudopia of pratfall-attempts at science, IF they are proffered in defense of archons' orthodoxy, with the intended result that a lopsided ex-controversy can be cosmetized to fake it as the eternal sober-scholar-vs-sober-scholar stalemate which Gingerich has by now been reduced to being ready to settle for, as his best hope for continuing postponement of [A] recognition that the disagreement has always been scientists versus . . . (well, let's just say: projectors that call *other* people kooks); and [B] his own inevitable public condemnation for his and his cult's half-century of inexcusable (to all but [his cult]) bad libel and bad science. (And — as R.Newton often commented — bad history, as unwell: e.g., end of fn 11 on ancients' yearlengths!) But, in an internet age: does Gingerich seriously imagine that attempts — by himself and those he influences — at protecting a crumbling dike (Rawlins 1996C p.4) from crashing evidential waves (that aren't going to go away), by trying to mute or kill this paper, can keep it from the eyes of technically competent historians and scientists? FOREVER? It might seem incredible, but: most journals, upon receiving a Rawlins paper on Ptolemy, have chosen his most slanderous enemy Gingerich as referee, occasionally with veto-power over content, even while keeping him in anonymous clothing. [Anonymous refereeing is disgustingly common in academe (though NEVER used at *DIO*) — allegedly in order to protect referees from archons' vengeance. (Which implies a revealing reality.) But Gingerich *and virtually all referees called upon* by history of astronomy journals *are* archons — and of the undislodgeable ilk — so there are no consequences for them to fear, no matter what their customarily-superficial (fn 3) reports claim. (The ubiquity of these journals' near-exclusive use of power-people instead of scholarly experts as referees is an ongoing anti-progressive scandal that's even worse than the individual ones exposed throughout this paper.)] Given Ptolemists' numeracy: are they somehow estimating that, if they can put off a reckoning (on Ptolemy Scandal #1, antiquity, and Ptolemy Scandal #2, the JHAD) for another half-century, the verdict of history will rate 100^y-suppression as less awful than 50^y? Mercy-to-the-merciless is pleaded by our referee, despite his glaring omission to contravene — anywhere in the paper — a single fact, calculation, source-citation, or believer-blunder-blitzout. He professes to support its publication, even while familiarily unable to resist *psychoanalysing* the "pompous" author's incontrovertible (referee-unchallenged) mathematical skewering of an unrelieved succession of transparently flawed apologia, as exhibiting (emph. added) a "NEED to disparage those with different views" — thereby confusing the author's (actually quite liberal: fn 6) attitude toward dissent with his non-admiration of an intolerant herd's scientific fecklessness, cultishness, and *a priori* mentality. The referee's objection to the paper's alleged "pomposity" towards Ptolemists (read: tragicomedic astonishment — at eagerly-volunteering miss-men, who imagine they're much smarter at science than the unfortunate reality) may tell us that

date; A.Jones 2010A[2] p.xiii) that Ptolemy was actually an honest outdoor astronomer (even [below, at §N3] rating Ptolemy a better *observer* than Hipparchos!); or at least (Schaefer 2013 p.47) that there is still a serious question about whether he observed outdoors: classic the-controversy-continues resort (latest example: fn 1), ever dear to those fearing faces-loss in a prominent controversy. Even after a 50^y avalanche of discoveries consistently (fn 12) contradicting his position, Earth's most loyal Ptolemist responded to Thurston 2002S p.69's prominent challenge by *unqualifiedly* verbatim-reaffirming (Gingerich 2002 p.70) his original 40^y-ago verbatim echo (Gingerich 1976) of the once-unopposably-dominant mantra (§M2) that Ptolemy was "the greatest astronomer of antiquity" (Neugebauer 1975 p.931).

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

B1   In the history-of-astronomy community, for nearly half a century, the issue of Ptolemy's honesty, originality, and competence has been a trigger for blackballing (Rawlins 1991W §B), shunning (Gingerich 1990; Schaefer 2002 p.40), censorship (Rawlins 1996C p.4, *DIO 8* p.2), and intimidation (*idem* fn 1). E.g., the field-central and highly esteemed *Journal for the History of Astronomy*[3] (*JHA*) may be just a minim short of perfection in its

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some don't empathize with the laughter a genuine scientist can hardly stifle, upon carefully examining the goal-directed imitation-science that is (not merely *charged* here but is) repeatedly *proven in detail*, throughout the paper, to be what has been consistently and exclusively used for decades to do what a once-ruling cult wanted done: make a case — any case (§H2 & ‡3 fn 66) — that Ptolemy was an honest, outdoor-observing astronomer. The referee imagines that the bad-old-days still apply, when Ptolemists could keep doubters from being taken seriously just by portraying them as those who had committed the crimes [a] of being few, and [b] of merely *disagreeing* with formerly-mid-spectrum orthodoxy. He actually dream-contends (fn 5) that the view that Ptolemy faked or plagiarized is still today a way-out *end-of-the-spectrum* theory, a charge which uninformedly classifies Ptolemy-doubters Dennis Duke (fn 23), Sam Goldstein, Gerd Graßhoff, Kimball Hansen, Willy Hartner, Alex Jones (fn 2), Charles Kowal, Robert Newton, Myles Standish, Richard Stephenson, Hugh Thurston, Gerald Toomer (fn 17), Bart van der Waerden, Curtis Wilson, and Don Yeomans (2005/4/28) as fringe! (Also Kristian Moesgaard, who was the 1st centrist to realize the significance of R.Newton's fractional-endings test: §I2. Moesgaard 1980C says of the R.Newton fractional-endings analysis: "This renders it probable that the [*Almajest* star catalog] longitudes" were plagiarized from Hipparchos. But the original version of the paper [sent to DR 1978/4/30], as submitted to the R.Newton-hating *JHA*, read instead [emph added] "*Beyond the shade of a doubt* this renders probable that the [*Almajest* star catalog] longitudes" were plagiarized from Hipparchos. The incident is nearly an exact repeat of frightened Astronomer Royal Geo.Airy's equally revealing bowdlerization of "shadow" from his 1846/7/9 letter to Neptune-affair co-conspirator J.Challis: see Rawlins 1992W §B2 & Rawlins 1999N §H2.) And for more unintended qwasi-humor, we have the referee's judgement — as a seasoned (fn 5!) authority on *non-insult etiquette and fair play* towards "those with different views" — that the paper shows insufficient doses of the **respect he believes is owed** to those wannabee-numerate historians-of-science who have repeatedly (§B2) libelled physicists R.Newton and Rawlins as dishonest crazy incompetents and who have (fn 5) for years ducked debating Rawlins (compare to fn 6, below), though having the courage to serially portray his work in the most negative light to the extent of over 100 pages (1987-2008) in the *Journal for the History of Astronomy*, from which he has been banned (§B1) for the last 1/3 of a century, during which time virtually none of Newton's or *DIO*'s dozens of positive contributions to knowledge (e.g., R.Newton 1977 & R.Newton 1982, www.dioi.org/vin.htm) have been credited there, a blank entirely in accord with shun-necessities&practices all too obvious from fn 34 & ‡3 fn 109.

² Unless CalTech's Noel Swerdlow, not Jones, wrote this section of the preface — though Editor Jones is responsible for publishing it, while Jones 2005 p.21 knows that Ptolemy's solar data were faked, and that his 180° *ekumene* was rigged (*ibid* p.35; Berggren & Jones 2000 p.76 n.53).

³ The "premier" (Schaefer 2002 p.40) quality of the *Journal for the History of Astronomy*'s refereeing may be gauged from its often fascinating Original Science, e.g., here at §N7 and fn 14. Not to mention rocks at §L2 and Pluto at fn 42. Non-appreciation of these gems and the like fully justifies Rawlins' banishment (fn 1) from *JHA*. As an example of just how "impossible to deal with" he is (1970-2013

refereeing (fn 3) but is alert as can be in not permitting any author to argue in its pages that Ptolemy was a liar, even while allowing the defense clique to contend he was not (Evans 1993 p.145; Rawlins 1999 §§F7-F8). That the controversy has been rife with "unprofessional" acts, such as shunning, has been nationally published (Schaefer 2002 *loc cit*), followed soon after by a direct appeal (fn 35) to the head of the American Astronomical Society, urging supervision of its shamelessly shunning Historical Astronomy Division (H.A.D.), but the unprofessionalism of 2002 has only worsened since. History-of-ancient-astronomy's lengthy communal monolithic and craniolithic insistence 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-flouting (§M3), holey-corpse honest-Ptolemy-myth as its ultimate herd-sacred tenet, can only weaken the field's cred. Same for launching fantastic, irrelevant, and transparently projective descriptions of bemused skeptics as "angry" (Pedersen 1974 p.23) and "bitter" (Evans 1998 p.268). For which side is *provably* angry, see *DIO 11.1* p.2; also the case of an eminent referee and Jesus-devotee (fn 5) who routinely (fn 1) calls today's numerous (*idem*) Ptolemy-skeptics a tiny bunch of paranoids[4] for believing what he himself privately knows[5] to be true. Healthy restoration

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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#kqlz), this advice was indisputably a "damned lie." (So claimed patient, sedate, judicious [Schaefer 2002 p.40] Hoskin in his 1983/3/3 letter to hothead-horribilis [*idem*] Rawlins, announcing his exile from *JHA*, while suit-threateningly rejecting Rawlins' gentle mathematical criticism of *JHA* refereeing for a 1982 October *JHA* paper — Hoskin's letter mailed, ironically, just before *JHA* received the ethical author's agreement that, after all, the paper in question was just as invalid as Rawlins had told Hoskin: see fundamental correction in the 1984 June *JHA*.) Who better than Hoskin to impart the secret of reliable refereeing? — as in Hoskin's 2003/6/21 letter to H.Thurston, www.dioi.org/pm3.htm, enlightening us on *JHA* email-era refereeing efficiency: "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."

⁴How has the academic community become so used to (and for so long) the peculiarity of the history-of-astronomy field's dominant figure privately spreading false libel? (Do those pols and media who defer to OG have any idea of or concern about the tactics here displayed? Or the degree of respect held for his scholarship by working scholars?) But he is not a lonely figure. Indeed, he is Mr.Establishment: political connexions, influence over funds & publication, and so on — which scares scholars (§B1) and lures apologists (fn 1) into, ironically, so protecting him as to encourage [a] unhelpful disconnect from controversies' realities, [b] a sense of impunity (ever blaming the slanderee), and [c] blithe persistence in anonymous personal irrelevancy despite being repeatedly surprise-revealed at it (thanks to leaks by ethical scientists who are revolted by it). Does he personify the desire of organized science to protect the public from knowing that "Great-Scientist" Ptolemy faked? (We recall then-*Science* Editor Daniel Koshland even trying to put over on the public and Congress that 99.9999% of scientific papers are honest.) The establishment's most trusted forums — AAAS and its *Science* and the like — all continue (despite repeated protests) to side with the faker and ignore his and his modern worshippers' dirty science, dirty math, dirty history, and dirty tricks. Given the ethics exhibited, it's no surprise that these institutions defend Ptolemy. The situation is almost as astonishing as the case's long history of suppression and intimidation of dissent — contrary to all that those same finger-crossing organizations claim to stand for, each of whom by instinct continues as ever to go for the bet-redoubling choice of preventing scandal over journals' shunnings and coverup not by coming clean but by just doing a better job of covering up — which inadvertently adds in the extra new labor of covering up the previous coverup. Etc. Etc. Hey, it's worked so far. Sorta. Hasn't it?

⁵ Gingerich's long-secret 1977/7/8 referee report (www.dioi.org/pm2.htm) to *Publications of the Astronomical Society of the Pacific* (on the paper which became Rawlins 1982C, whose value is no longer doubted) says Rawlins "believes there is a conspiracy to suppress all criticisms of Ptolemy . . . . Rollins [*sic*] has become almost paranoid about this . . . ." (See at Rawlins 1994S §§H4-H6 for fuller quote, and comments on Gingerich's various imaginings.) Branding as paranoid someone's sense of unfairness and avoidance of debate, even while secretly stabbing him in the back, thereby *both engaging-in, and simultaneously proving the clandestine reality of, that same unfairness and*

will merely (!) require the opening of discourse[6] and minds.

B2 Ptolemy regularly fabricated data from which he perversely claimed he derived his models' elements, even while practicing the very opposite. Loyalist Pedersen 1974 p.23 originally disagreed but summed up the esteemed astronomer J.Delambre's view of Ptolemy as "a scientific cheat, swindling with the very method of science and betraying the empirical character of astronomy, setting forth results computed from theory disguised as empirical data in support of this same theory." *Ibid* p.258 also originally rejected the charge that Ptolemy stole from Hipparchos the thousand star-positions in the Ancient Star Catalog — even while suppressing[7] citation of Delambre's 1817 discovery of proof (§K1) that Ptolemy

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avoidance, is, ethically speaking . . . original. And, then, it's always stimulating to be diagnosed as insane by one who has for 50[y] 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 shrinkoanalysed as paranoid by one who thinks the whole universe is run by an invisible mind and who publicly calls on "our Lord Jesus Christ." A world where sanity is insanity and vice-versa. With respect to its obvious duty to return the Ptolemy controversy to a *scientific basis* — above politics, smearing, shunning, religious devotion — the American Astronomical Society for a half-century hasn't lifted a finger. But (fn 35) has generously given it to the skeptics. Soon after Gingerich had brought *P.A.S.P.* up to snuff on who is and isn't reliable, *Skeptical Inquirer* Editor K.Frazier asked him to debate Rawlins on Ptolemy in *SkInq*; Gingerich (1978/2/2) explained his refusal by calling Rawlins "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 Rawlins, Gingerich got miffed at Frazier! (1978/6/2 letter) — so exalted is his permanent mental state, assured of ontological immunity from the ethical standards of ordinary mortals, sinless by very definition. Like his image of his inherently unindictable ancient astronomer-hero. E.g., only such an elevated being could — without any sense of hypocrisy, of damage to knowledge, or of harm to the slanderee — accuse someone of insanity for believing in a conspiracy to suppress heresy even WHILE he was so conspiring (Gingerich to *P.A.S.P.* at the head of this note). At Gingerich's insistence, U. Notre Dame's 1999/7/3 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 pols' reverent silence on his secret actions, some even going so far as to supplicate respectfully for personal permission, before publishing anything unblesséd), Gingerich in a 2000 referee report to *Isis* on an article (Thurston 2002S) appreciative of Rawlins' 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 could only 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] 'mufia' [*sic*] are preventing their viewpoint from being aired in the standard journals." Hmmm. On 1976/9/15, Rawlins 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 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-mythologizer Cardinal John Henry Newman's politically deft holy war on straightforwardly ethical independent idealist Chas. Kingsley; at Newman's 1890 death, after the standard public eulogy, Cardinal Henry Edward Manning privately mourned his colleague thusly: "Poor Newman. Poor Newman. He was a great hater." (Lytton Strachey, *Eminent Victorians*, 1918, Manning chapter, end of part 9.)

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

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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-reeeeally — for either 1974 Pedersen or *JHA* Editor Evans. Each could always be counted upon to explain-away (fn 11) any negative evidence, trying to make the issue of Ptolemy's integrity untestable thus irresolvable, so never having to admit their original mistake in defending it. And far, far harder: to face the shame of having for decades (mostly behind-the-back: fn 5) gang-condemned as fools and cranks those who turned out to be more prescient (§M3 and fn 12) than their denigrators. Pioneer skeptic Robert Russell Newton is deceased. Nothing can now compensate him for the hateful, largely-whispered bile heaped upon him to prevent his case from getting a fair hearing while he lived: sampling at fn 35, plus MacArthur Genius and amateur[8] scientist Noel Swerdlow's branding this brilliant physicist a Velikovskian "con-man" (see *DIO 1.1* ‡3 §§D2-D3; and for who's really Velikovskian, see below at §N19, and *Worlds in Collision* p.330). Given such a heavy 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 shrunken[9] chauvinist

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[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-McCarthiesque incompetency-accusation against him (quoted verbatim at R.Newton 1991 §E22), 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* — applying its proud smear-review policy — naturally approved this much-rewarded (fn 28) libel's publication **in that state**, no questions-asked. (Swerdlow's ugly response to R.Newton aimed less at proving his thesis wrong than at portraying him as astronomically incompetent. Given the two sides' relative technical talent, such Ptolemist antics reliably kept on giving Newton and Rawlins their top entertainment of the controversy.) Perhaps the least-believable incident was when *JHA* Book Review Editor Gingerich chose as reviewer, for Newton's 1976 book, miss-man Swerdlow — whose predictably maw-foaming throat-lunge (samples included at fn 35, direct quote at R.Newton 1991 §D13) inadvertently revealed that Swerdlow didn't understand even so much as the TITLE of the book he thought he knew enough to denigrate: see it happen at R.Newton 1991 §A2. We respond with the sort of list Swerdlow should have laid out against Newton but could not (admire in-passing Swerdlow's personal reinvention of the mathematics underlying the most elementary issues, including both of his attempts to answer central heresy on Ptolemy): [1] Ineducability on the high-school maximum problem cited at §N7, being unfamiliar with the long-standard (see any Bowditch *Navigator*) Equal-Altitudes technique, for years repeatedly (fn 11 below) insisting that a quadratic's maximum can't be found accurately, **because near maximum, the function isn't varying much** — e.g., Swerdlow 1989 p.36, when trying to alibi Ptolemy's Venus fakes. [2] Obliviousness to the fact that ALL observing ancients used solstices not equinoxes to gauge yearlength (their awareness is itemized at fn 11 [2]). [3] Innocence of need (similarly at Rawlins & Pickering 2001) to convert longitude residuals to great-circle by factor $\cos\beta$ (during his attempt to answer the absent-error-waves test, Swerdlow 1992 p.176; misunderstanding clarified for him by Rawlins 1992V fn 31). [4] Following (Swerdlow 1989) Ptolemy's elaborate fraudulent mathematics to complicate the finding of Venus' orbit from greatest elongations — though simply bisecting the angles between their sight-lines sets-up solution of the orbit (Rawlins 2002V; Thurston 2002V) with 10th-grade ease. (The Venus synodic mean motion deduced from the *Alm*'s proofs is the worst of all the planets. But, if adjusted according to the theory that its reporter had confused tropical and sidereal cycles, it strikingly becomes among the best, as accurate as Mars', good to ordmag 1′/century — see Rawlins 2002V §C3.) [5] Obvious mis-math: here at §D3. [6] And some Swerdlowian fudgery (R.Newton 1991 fn 7) that Ptolemy would've been proud of.

[9] Ptolemists' 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

bunker — to admit error *in the slightest degree*.[10]

B3   Notable features of modern Ptolemists are: [a] Consistent preference (fnn 18&50; §N15) for the inherently unlikely[11] over the inherently likely. [b] Failure to notice that serial-proposal of a disjointed collection of *odd-hoc* theories, each tailored specifically and entirely for dodging the latest[12] individual Ptolemy-indicting bullet (see also §D6 and fn 18) defies probability — as well as Occam's Razor, which seeks the single coherent[13] theory

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[10] Ptolemy-doubting scientists like Sam Goldstein, R.Newton, B.L.van der Waerden, have all freely admitted in print their occasional mistakes; Rawlins even did so by self-lampoon, right on the cover of www.dioi.org/vols/wb2.pdf, *DIO 11.2* (2003) [and p.30, p.33 items 5&6]. We await the century when Swerdlow or Gingerich can display in print the same humility as the "pompous" (fn 1) skeptical side.

[11] For examples of downright embarrassingly improbable apologia to defend archons' pro-Ptolemy commitments, see here at, e.g., §§K&L2, fn 37. (As well as Rawlins 1985G n.12; Rawlins 1991W §E3 & fn 99. Cumulative osculation-oscillation historical review at Rawlins 1992V §C31.) The ultimate far-fetchedness in service to orthodoxy was devised by Evans and promoted by Swerdlow 1992 p.177, attempting evasion of the fractional-endings argument (R.Newton 1977 pp.245f). The incredible result (Evans 1987 p.243) is spoofed at Rawlins 1992V fn 46 (emphasis in original): "Let's see, we start by setting [the armillary astrolabe's] ring 5 NOT on the chosen fundamental star's ACTUAL [Catalog] longitude at ring 3 but rather at the nearest whole-degree value LESS than [this longitude, on which Evans prefers a 40′ ending]; then, after sighting the stellar quarry with ring 2, we read where ring 2 meets ring 3 AND THEN ["mentally"] ADD BACK, ONTO THIS READING, THE AMOUNT [40′] WE JUST AS NEEDLESSLY SUBTRACTED OFF IN THE FIRST PLACE. . . . Got it?" And don't miss that this bizarre Evans scheme not only causes the unnecessary trouble highlighted here (and expands the amplitude of the absent error waves [§H1] from 1°/2 to 3°/4!), but would (Evans, *loc cit*) further commit Ptolemy to the extra bother of having to fudge over a hundred (Rawlins 1994L Table 1) stars' resulting 25′ and 55′ endings (into 20′s and 00′s, respectively) in the manner shown at §I4. As one encounters no less than 64 pages of such desperate resorts, we recall (§§B2 and H2; Rawlins 1992V §§C31-C32) it is intermittently contended that Ptolemy's plagiarizing the Catalog doesn't prove dishonesty. But, if so, then: why did the *JHA* waste over 100 pages fighting academe's acceptance of a Catalog-theft that (whenever JHADists momentarily admit it) doesn't-really-matter? (Iraq war apologists dodge similarly: *DIO 18* §F.) For further imaginative excursions, see (Gingerich 1976 in *Science*) "On Ptolemy as the Greatest Astronomer of Antiquity" and (§M2) *Scientific American*, 1979, "The Acquittal of Ptolemy." (In the 4 decades since these two worshipfully and optimistically titled Ptolemy-advertisements, neither *Science* nor *Scientific American* has printed a word on Ptolemy's frailties; likewise, *Sky and Telescope*, which has instead repeatedly defended him: 1976 Feb-to-2002 Feb. The vaunted Free Press at work.) Extra community-embarrassments: massive double-Pb-paper Evans 1987 (below, fn 47); top Newton-exiler (below, fn 35) Swerdlow 1989, on whose invincible math-innocence (repeated 1979, 1981, 1989, 2010 while reaching for Ptolemy-exculpations), see above, at fn 8 or Rawlins 2018U §§B2-B3. Two more cases of shamefully shoddy science seem descended from solstice-denier Swerdlow's delusional argument (§N7 below): [1] The instrumental (*and historical*) misconception of Jones 2002E p.16, that ancients found geographical latitude *L* from observations at equinox. (Contra chapter-one Ptolemy, *Alm* 1.12; see Rawlins 2009S §§F2-F3 & eq.8.) [2] Evans 1998 p.206 explicitly echoes Swerdlow's highschool incomprehension, claiming a transit instrument "could not determine the time of solstice very precisely. owing to the [gradual] nature of the solstice itself. A precise measurement of the length of the year could not be based on the solstices. More reliable for this purpose were the times of the equinoxes." Aside from his clique's instrumental blind-spot (§N7): is Evans *even aware* (as is, e.g., Toomer) that all outdoor astronomers' extant ancient yearlengths were solstice-based? — Meton (*Alm* 3.1), Euktemon (*idem*), Kallippos (Toomer 1984 pp.12&214, fn 72; Rawlins 1991W §K4), Aristarchos (§N18 below), Hipparchos (§N19 below). (Recoverable Greek solstices' rms error 2[h] [§N7] vs Greek equinoxes' rms error 7[h] [§M3].) The risibility recreates *Funny . . . Forum*'s general Miles Gloriosus as philosopher: *Stand Aside. I Take Laaarge Logical Steps.*

[12] Of Ptolemy's 1970s promoters, who yet today dreamily (Rawlins 2002V fn 13) expect their judgement to be taken as authoritative, **none** discovered (or even anticipated) *any* of the cascade of tests of his honesty soon-after unleashed: all, one after another after another, favoring skepticism (e.g., Rawlins 1992V §C22), an asymmetry from which they exhibit proud determination to learn nothing.

[13] To the attempts of Swerdlow 1992 ("The Enigma of Ptolemy's Catalogue of Stars") to throw the usual flock of disconnected alibis at skeptics, in order to — like any other criminal lawyer —

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that explains multiple features of the available evidence. Ptolemy's fraudulence is that single simple theory which does so.

B4   *NB*: In case a response to this paper appears later, look carefully to see if it deals with *all nine* (§§C-I&K-L) of our proofs of Ptolemy's dishonesty hereabouts (a display which would unavoidably highlight defenders' alibi-incoherence: §B3 and *most especially* Rawlins 1992V §§C31-C33) — or more likely instead just tries either [i] to claim that faking data and stealing stars isn't dishonest (§§B2 and H2), or [ii] to extrapolate-concoct blanket-rejection of doubt by attempting improbable maybe-coulda-happened theories (fn 11) for one or two proofs, before changing the subject, to divert from the Occamite power of the single obvious solution to all nine.

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

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

C1   The Greatest Astronomer of Antiquity's sloppiness with his Venus swindles was so extreme that he inadvertently reports (*Alm* 10.1-2) having 1st-hand "observed" **the same event** — the 136 AD greatest evening elongation of Venus — on 2 different dates 37[d] apart (136/11/18&12/25), at 2 different positions 37° apart, and at 2 different maximum elongations from the mean Sun, differing by 1°/5 [nearly the Sun's semidiameter!]: 47°1/3 versus 47°16/30. Not only do these data disagree with the outdoor sky, they also disagree with Ptolemy's own Venus tables. (Similarly for the Sun [§D5] and the Moon [§G3].)

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

D   ILLEGALLY BLIND: SKY or SLY? MULTICORROBORATION

D1   The Greatest Astronomer of Antiquity's four solar "observations" (*Alm* 3.1 and 7) are on average about 50 times nearer[15] to his indoor tables than to the real outdoor sky

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create Reasonable Doubt, Rawlins 1992V §C20 responded: "Besides a range of specific evidences of plagiarism, there is the simplicity of that hypothesis' fit to the larger evidential situation: if we merely assume that Ptolemy swiped the Catalog, virtually all of [defenders'] central purported 'Enigmas' (*ibid* [§C22]) of the case immediately evaporate."
[Ptolemists are hereby asked to specify any that don't.]

[14] However, O.Gingerich (2000 Summer referee report on Thurston 2002S) regards the Venus double-dating as a testament to Ptolemy's "great ingenuity in tackling an otherwise essentially insoluble problem," admittedly fabrication but innocent since (Gingerich 2002 p.72) there was no other way to solve for the orbit: Venus-made-me-do-it. At D.Duke's instigation, three non-fraudulent solutions (by himself, D.Rawlins, and H.Thurston) to "insoluble" Venus swiftly appeared in *DIO 11.3* (Duke 2002B, Rawlins 2002V, Thurston 2002V) with the comment (Rawlins, *op cit* §I7) that Swerdlow's and Gingerich's straightfaced "alibiing of Ptolemy's Venus fumblefarces is akin to a defense-lawyer going into court to get-off a counterfeiter who was so stupidly careless that he accidentally printed Ben Franklin on both sides of his attempts at faking hundred-dollar bills. [But would *even a lawyer*] . . . try to excuse such inept criminality by claiming that the bungled bucks showed *immortal, greatest-technician-of-the-era BRILLIANCE*?" Swerdlow 1989 p.36 fallaciously explains-away (to his own, the MacArthur Foundation's, and the referee-challenged [fn 3] *JHA*'s satisfaction) Ptolemy's huge Venus inaccuracies by stolidly repeating his innocence (§N7) of how to find a quadratic problem's maximum, this time for Venus' elongations. (Awful details at Rawlins 2002V fn 20.)

[15] The same ratio for Hipparchos' solar observations (*Alm* 3.1) is merely about 3; so Ptolemy's indoor "observations" (leading to an outlandish ratio of 50) cannot be deemed anciently normal — as

(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 measures 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 positions[16] or (§B2) he has broken the law of empirical science by pretending to base theory on outdoor celestial "observations" actually computed, indoors on the sly, from or to fit the very same theory (Thurston 1994P §D & Rawlins 2002V §§B3-B5), such fakes then used to "prove" said theory, a (literally) preposterous sham-process as Delambre saw: §B2.

D2   To appreciate the grossness of the illegality here, consider its sheer enormity (as emphasized in solar, lunar, and planetary contexts throughout R.Newton 1977): for his 3 equinoxes, Ptolemy is alleging 1st-hand visual sightings of the Sun's ***center*** on the celestial Equator at times agreeing to 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 $-23'.0$/cy. (A rate consistently bi-miscomputed at CalTech's Swerdlow 2010 p.152, item 3.) So: when is Ptolemy's Sun correct? Obviously that epoch must be $137.547 + (100^y/cy)65'/(-23'/cy) = -145$, 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 mimick the Hipparchan PH solar model's big $0°.4$-amplitude annual *periodic* error. (Again: see error formula for Ptolemy's Sun at Thurston 1998A ⊙1.) 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 Arbela 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.lxvij-lxix) explained in detail how Ptolemy had fabricated his solstice and equinoxes by merely adding integral numbers of Hipparchan years to Hipparchos' observations of solstice and equinoxes. (Discussion: Thurston 1994P p.58; and Thurston 1998A §§A&S.) For example (by method of *ibid* ⊙2, reconstructing indoor calculation of the 139 AD Autumn Equinox): to recover Ptolemy's 132 AD Autumn Equinox (*Alm* 3.7), just add 278 Hipparchan years ($365^d1/4 - 1/300$ each, 6^m longer than

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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 same delusional Acquittal] reports several discordant with theory & each other: ‡3 fn 8.

[16] Ptolemy didn't have to look skyward to see theoretical data instead of real: his *Optics* fakes 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.)

actual $365^d.2425$ year then) to Hipparchos' $-146/9/27\ 00^h$ Autumn Equinox (*Alm* 3.1), and one finds $132/9/25\ 13^h46^m$. Ptolemy reports (*Alm* 3.7) $132/9/25\ 14^h$. All 4 of his solar data agree with such arithmetic, to the 1^h precision he displays for each of the 4 — the precision itself a revealing farce, since in all cases based upon Hipparchan cardinal-point times ***6-fold rounder***: each expressed to the nearest 1/4 day. (For historically valuable detection of yet another Ptolemy solar longitude fake see Thurston 2002S pp.65-66 & fn 14.)

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 (§D3) for Hipparchos' epoch; [3] Ptolemy "observations" tightly (§D4) and overprecisely (§D5) theoretical not empirical. No matter how obvious the implications of items [1]-[3], each has inspired neverevereverever-say-die[17] efforts at explaining them away. However, as in §B3 [b], we find no substantial connexion between chauvinists' several desperate and disparate[18] alibis (other than the common aim of rescuing Ptolemy), while by contrast all three oddities are mutually-corroborative of each other through the single simple theory that simultaneously, coherently, and fruitfully explains them: Ptolemy faked.

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

E1   At *Alm* 5.12 and 13, The Greatest Astronomer of Antiquity provides *and computes celestial positions using* his assumed geographical latitude L for Alexandria: $30°58'$ — an erroneous value swiped from Vitruvius 9.7.1 (probably based on observation by asymmetric gnomon, not transit circle). For c.8000 sites, Ptolemy's *Geographical Directory* (*GD*) lists, in Books 2-7, geographical latitudes L and geographical longitudes E east of the Blessed Isles (discovered at Rawlins 2008S §F, to be obviously the Cape Verde Islands), uniformly rounded to the nearest twelfth of a degree. At *GD* 4.5.9 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*.[19]

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[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 Graßhoff's analyses.

[18] From chats with Ptolemists over many years, the following alibis are recalled: [1] Ptolemy's $-65'$ solar error has been speculated as due to his having constructed his system before most or all of his solar "observations" (pretty dumb, since the system depended on the Sun: §D3), so he was stuck with the error and decided [see Ragep at ‡3 fn 9] not to re-do his whole scheme. (How is this a defense against a charge of knowing pretense? And plagiarism, since [§D3] the system's error is just that of Hipparchos' solar tables, 2 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 presuming that Ptolemy thought Hipparchos' observations were better than his own putative outdoor observations, so (§D1) he instead reported Hipparchos-accordant data. (Was it honest to commit this appropriation? — and without saying so, which makes it a theft.) [4] See also rocks and 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,

E2   Moreover, such an error (see math of *Alm* 5.12 and 13, or Rawlins 1994L eq.1) would carry exactly into the "Clean Dozen" unfudged and unused Ptolemy-contemporary stellar declinations δ reported at *Alm* 7.3 (the only honest *Almajest* star data from Ptolemy's era). These, however, show (Rawlins 1994L §F9) that the observer's error[20] in assumed geographical latitude L was $+4'\pm2'$, 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[21] anonymous observer who knew his L.

E3   Confirmation is achieved via statistical induction (Rawlins 1994L §F8) of the exact latitude $L = 31°1/4$ 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.

E4   At *GD* 4.5.76 Ptolemy lists the Alexandria harbor Pharos lighthouse's L as $31°05'$, blithely copying this false L from Hipparchos-Strabo (Strabo 2.5.39; Neugebauer 1975 p.1313; Diller 1984 fn 23; Rawlins 2009S fn 16). Question: Since his *Alm* had already (§E1) put Alexandria instead at $30°58'$ (consistent with *GD* 4.5.9's $5'$-rounded $31°$ value) why didn't **an Alexandrian**[22] **& allegedly-outdoor observer** notice he'd thus inadvertently stretched by ordmag TEN-fold the 7-stade-long embankment connecting Alexandria to its Pharos, an embankment explicitly **named** Heptastadion (επτασταδιω: Strabo 17.1.6)?

E5   So by carelessly[23] copying disparate Alexandria L data from Vitruvius (§E1) and Hipparchos (§E4), "astronomical observer" Ptolemy adopted — *simultaneously* — two L

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over decades, have never noticed either?! — which could have saved themselves a half-century of drip-torture serial-embarrassment, by recognizing the obvious right away and promptly moving on to careers of *open-ended inquiry* instead of sterile (fn 12; Rawlins 2009E fn 7), quasi-theological apologetics, with sacred-cowclusion-set-in-advance. In cement. The answer is revealed by another question: which route has been exclusively rewarded (fn 28) by the JHAD, throughout The Controversy, and which has been banished (§B2; fn 3), attacked with pseudo-science (fn 8), & slandered with fantasies (fn 35)?

[20] Excluding no stars when bivariate-least-squaresing the Clean Dozen produces the L-error cited, leading ultimately (carefully trace Rawlins 1994L §§F3-F9) to the observer's $L = 31°11'\pm2'$ (*ibid* Table 3) — consistent with Alexandria's $L = 31°12'$. Same process produces the Clean Dozen's epoch, 159 ± 8^y, in fine accord (*ibid* fn 45) with the 10th century *Suda*'s dating of Ptolemy to epoch Marcus Aurelius 1 (160/7/14). Previously unnoted vastness of *Alm* 7.3's leapfrog anachronism: the Clean Dozen δ were observed c.160 AD (as just shown) and then merged with the SickSix δ 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^y 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-outré 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 $0'.72$, Aristyllos $0'.18$, 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.

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

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

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

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

E6   Also revealing of Ptolemy's degree of empiricism is his astonishing listing of the Pharos (§E4) at exactly the same L (§E1) — $31°05'$ — as for his home Serapic temple at Canopus from where he had only to look down the Mediterranean coast after dark to see[24] that the 12 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: $12\cdot\sin30° = 6'$.) Further evidence that Ptolemy "doesn't seem to have allowed his eyeballs out at night" (Rawlins 1985G p.266).

F   IMPERVIOUS MERCURY

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

F2   But the difficulty for Ptolemy's loyalists is this: his *Canobic Inscription*, written some years before the *Alm* (as proven in the brilliant paper, Hamilton, Swerdlow, & Toomer 1987), listed precisely the same Mercury mean motion, but most of the other elements differed. So: how could the same mean motion have been empirically and mathematically based for both works, if the respective derivations involved discrepant elements? E.g., deriving the 139 AD position for the *Canobic Inscription* elements versus doing so using the *Alm* elements, produces results disagreeing by over $5°$. Yet The Greatest Astronomer of Antiquity gives the identical mean motion in both works, to six sexagesimal places, and this speed is anyway precisely computed not from the alleged observational base (angular-arc-traversed/time-interval) cited at *Alm* 9.10 but from the period-relation $52200°/16802^d24'$ found at *Alm* 9.3. [Full calculation of $5°$ discrepancy: Rawlins 1987 p.236-237.]
[It was specifically this fraud that most convinced van der Waerden Ptolemy was "a liar".]

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

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

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

[24] Check Rawlins 2008Q (§C1 & eq.23) for the Pharos' sea-level visibility-distance, about 20 nmi, or $1°/3$ 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 was 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)[25] from Marinos of Tyre. Instead, Ptolemy's vast opus provides (GD 1.4.2) but **one**[26] example: two longitudinally much-separated reports — 500^y old! — of the famous Arbela −330/9/20 lunar eclipse's start, saying it was seen there at 23^h and in Carthage at 20^h, thus proving that the 2 places are 3^h or 45° apart in longitude.

G3   However, Pliny earlier reported the same data very differently: 20^h (8 PM) for Arbela (modern embattled oil-city Irbil) & 18^h (6 PM) for Sicily, whose west end (big city Lilybaeum) was part of Carthage's empire, & of longitude anciently-known to be similar to Carthage's: www.dioi.org/j500.pdf cites D67&D131. Modern calculations[27] show that non-astronomer Pliny was quite accurate, while The Greatest Astronomer of Antiquity was amazingly wrong, over 2^h off for Carthage, 3^h off for Arbela. The former error nearly equals the entire actual 2^{h}1/4 longitude gap between the sites, and the latter error far exceeds said quarry. But the weirdest part is yet to come: Ptolemy's **own lunar tables** put the eclipse just about as much in disagreement with his reported times as modern tables do: 2^h Carthage and 3^h Arbela. How explain such an entertainingly disastrous fabrication? Start by consulting Pliny 2.72.180 on the −330/9/20 lunar eclipse. Reading the passage carefully, one sees that no numerical hour is given explicitly for the western apparition in Sicily, merely: moonrise ("exoriens"). By contrast, the Arbela time is given as the "2nd hour" after sunset, or about 20^h, which is the very time Ptolemy gives for the *Carthage* report. Why? Well, look carefully at the Pliny passage cited: by a fluke of grammar, "secunda hora" appears nearer in the sentence to "Sicilia" than to "Arbelam". This obviously suggests Ptolemy used Pliny or his source but (evidently unable to read Latin well) took Pliny's 20^h time to be Carthage's.

G4   But how did Ptolemy arrive at 23^h for Arbela? Since Gossellin 1790, it has been obvious that multiplication by an expansion factor (Diller 1984 §C5) had been applied by Marinos or Ptolemy or their source to a prior map's accurate longitudes, creating the oversize longitude intervals of the GD. Rawlins 1985G eq.15 showed that the expansion factors were either 7/5 or 4/3 in the region under consideration. Assuming that the earlier accurate map correctly put Arbela 2^{h}1/4 east of Carthage, then expansion by 4/3 would produce 3^h, the very gap — the very wrong gap — Ptolemy reports. I.e., typically for him — **and his defenders** — the conclusion was established ere the evidence was engaged.

G5   He simply added this 3^h to 20^h, thus arriving at his fantastic 23^h time for Arbela. *NB*: This solution adds powerful new evidence favoring the theory (still-foolishly-doubted: §G4; ‡1 §B; Rawlins 2008Q §J & Rawlins 2008S fnn 13&45) that GD fatefully corrupted an accurate prior map by expanding its longitudes by a factor of 30%-40%. Collecting §§C&E with the present case, we now have 3 separate Ptolemy double-false fakes on display here.

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

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

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

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

[27] The Battle of Arbela was fought at nearby Gaugamela and 11^d after the eclipse. We find actual Local Apparent Times of the −330/9/20 eclipse's umbral start: Carthage 17:43, Lilybaeum 17:52, Gaugamela 19:56, Arbela 19:58. So the Gaugamela-Lilybaeum difference in geographical longitude E is $\Delta E = 2^h04^m$; Gaugamela-Carthage, 2^h13^m.

rotates about the equatorial not ecliptic pole: as we can see from, e.g., the educational paper model Evans has helpfully disseminated), so (Rawlins 1982C p.361 & Fig.2) we'd find error waves of amplitude 1°/2 in the Catalog's latitudes β (cosine waves: *ibid* eq.4) and northern longitudes λ (sine waves: *ibid* eq.3). We don't. (Amusing details at Rawlins 1992V §§ C13-C15 & fn 31). See also the inspired findings of Graßhoff 1990 — which instantly converted dedicated and scholarly Ptolemist G.Toomer — as well as the perceptions of Duke 2002C, all of which combine to show that, e.g., errors in Hipparchos' stars are statistically quite discernable in the *Alm*'s, including a few ultra-giveaway cases where a star with an error of *several degrees* is found to have the same sized error, with the same sign, for both Hipparchos and Ptolemy.

H2   It thus became obvious c.1990, even to the most religious, that many Ptolemy stars were Hipparchan. However, no archon was ever going to admit in print the plain truth: the establishment had been blindsided by proof that its challengers had been right all along — that Graßhoff's test had now unexpectedly surprise-vindicated the long-loathed Tycho-Newton-Rawlins position that the Catalog was stolen. (The post-disaster spin of some was that the three pioneer skeptics had just regrettably not proven their case **clearly enough to be understood by** the judicious archons atop the American Astronomical Society's Historical Astronomy Division [H.A.D.] — surely a truth-determination criterion to live by — while Graßhoff had. Which translates as: [a] ashamed refusal to acknowledge that — until the truth hit them in all their faces —- believers had been too limited and predisposed to see anything significant in the same evidence from which skeptics had drawn the right conclusion years ahead of final proof; [b] denial of credit to unapproved first perceivers, according to a principle handed down to us from on-high, the *JHA* itself [quoted at Rawlins 1991W fn 127, emph added], "the first speculative occurrence of an idea is generally *far less significant* than its later emergence, possibly in other hands, supported by persuasive arguments.") The post-Graßhoff era has been especially fertile for indiscriminate fallback apologies by Ptolemy's (selectively) malleable modern choir, as the politically ambitious realized that the *JHA* would ever so gratefully[28] publish *anything* that muddied the clear evidential situation, in order to save archons from facing apt appreciation for decades of falsely denigrating now-vindicated scholars: simply pretend vindication either never happened or isn't 100.00000000%. Question: is anyone empathizing with the cornered defenders' needless pain here, caused by artificial extension of the Controversy? (Puts one in mind of equally needless ongoing misery from other kinds of poverty than intellectual — mass-agony likewise of insufficient concern to those who subsidize its perpetuity, to ensure their own perpetuity in office.) This is so literally *pathetic* — Chauvin's shade shutters his orbs in shame at what his legacy has come to. [1] Memory-hole-unapologetic for his original 1987 and 1998 arguments that Ptolemy probably outdoor-observed the whole catalog, Evans now just hopes that Graßhoff 1990 hasn't proven that *all*[29] stars were copied from Hipparchos. [2] Schaefer (2002) says the Yale Bright Star Catalog also grabs previous catalogs' stars, so what's the concern? However, both these defenses of Ptolemy's *integrity* plainly founder upon his claim of 1st-hand observation of *all* 1025 stars. And argument [2] is on the logical and ethical level of defending a bank-robber by pleading

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[28] Rewards handed out to those who attacked the R.Newton satan include *JHA* boardship (R.Newton 1991 fn 2) and a MacArthur for miss-man Swerdlow. (It's hard to find good help anymore.) Among other examples: maid-men Evans and Schaefer were elevated at *JHA* not long after their massive bungled 1998 and 2001-2002 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 disjunct mix of superposed shapes: circular (stars observed afresh) and elliptical (stars copied from Hipparchos). But the diagrams are instead just elliptical. (Even if otherwise, this would prove only that someone other than Hipparchos — not necessarily Ptolemy — observed the stars whose dots mapped circularly,)

that, well: doesn't everybody withdraw money from banks? The BSC does not claim 1st hand observation, while The Greatest Astronomer of Antiquity explicitly[30] does so 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 fn 177): the 5° gap which should exist between the antarctic circles[31] of Hipparchos' stars and Ptolemy's stars (due to their differing latitudes) does not exist. The 2 circles are virtually identical. Controversy over.

H4   Several Sagittarius (Sgr) non-dim stars were missed by Tycho, so Evans 1987 p.168 (like Evans 1998 p.272) tries to create 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 β, due to ancient Egyptian and Greek proclivity for expressing non-integers by using inverse integers: "unit fractions". (Cause of both excesses detailed at Rawlins 1994L §B4.) But the most common ending for the longitudes λ is 40′.

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

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

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

I3   Most critiques of Ptolemy's chicanery point primarily to the excess of 40′ endings (vs 00′ endings) in the Catalog longitudes λ, but (thanks to the 00′ ballot-box being [deliberately?] stuffed with the entire sample of rounded 55′s) the most shocking frequency-contrast is elsewhere (Rawlins 1992V §C22 item [e]; Rawlins 1994L fn 5): *the spectacularly greater number of 10′ endings than 30′ endings.* (Before Ptolemy added 2°40′ to Hipparchos' λ, these were 30′ and 50′ endings, respectively.) Looking naïvely at the tabular distribution, the fact that 30′s are the least frequent endings is bizarre, since 30′ is nearly the most frequent latitude ending, as it should be. Indeed, for the latitudes 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.

I4   Maintaining the Jekyllian pretense to being a genuine, respectable, *outdoor* astronomer, required the sneakiness of hyding the otherwise-glaringly-odd 55′ and 25′ endings (which Ptolemy's addition-thievery had produced from formerly 15′ and 45′ endings), by secretly rounding them to 00′ and 20′ endings, respectively. That is, the Greatest Astronomer of Antiquity deliberately and permanently destroyed data in a legendary work, just to cover his tracks in a theft. Thus, we cannot now tell whether a Ptolemy stellar longitude with a 00′ ending was 15′ or 20′ in Hipparchos' catalog; likewise for a Ptolemy 20′ ending, where we cannot know whether it was 40′ or 45′ for Hipparchos.

I5   So R.Newton (§I2) explained why longitudes λ are near-bereft of 15′s and 45′s (only[32] 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.

I6   Two other little-known extras regarding the Catalog: [A] Rawlins 1994L §§E4-E7 found statistically (at high odds) that the compiler of the Catalog's southern stars observed from a place where he had estimated his geographical latitude L at a value ending in 5/6 of a degree, consistent with the southern tip of Rhodos Island, Cape Prassonesi (latitude $L = 35°53′N$), but not with Alexandria's $L = 31°12'N$. [B] Shevchenko 1990 p.194 discovered for a specified half of the zodiac, stars' λ exhibited no particular excess of 40′s. Later, *DIO 10* (2000) fn 177 tested Gem-to-Sgr (roughly Shevchenko's range) and found that — excepting Sco, whose prime stars' β reach atypically far south for the zodiac — these stars' original Hipparchos 00′-excesses were not in ecliptical longitudes but in *polar* longitudes. The novel and insuperable impediment thus created for Ptolemy's defenders is found in the footnote cited.

I7   The root, of the persistence of the *embarrassingly*-long (given the evidence's imbalance) "debate" over the Ancient Star Catalog, is that sneakily (§I4) stealing ordmag 1000 stars is unambiguously, undeniably ***a scientific crime***, verifying the justice of the Newton book's Neugebauer-klan-hated, JHAD-enraging title, *The Crime of Claudius Ptolemy.*

I8   Some Ptolemists 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 PedaDodgical Ploy cannot excuse his explicit claim (§K1) of 1st-hand observation of all 1025 stars, a theft statistically lock-proven by Graßhoff (above, §H1).

I9   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 flagrantly consistent M.O.). But, again, among these, only our Greatest Astronomer of Antiquity ever stole a thousand stars — the factor that (as in §I8) separates the "mere" Fudger from the naked thief.

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[32] All 5 stars with 1°/4 longitude endings are ecliptical and are that rarity (like Tau informatae): not copied from Hipparchos. *DIO* found that these 5 oddballs' conjunctive sources (Rawlins 1992V fn 20 and Rawlins 1996C fn 108) were thrice lunar eclipses (Babylon, Hipparchos, and perhaps Menelaos) and twice Venus.

J CONCLUSIONS

J1   The *Alm* is an invaluable resource, our only connexion to much of high ancient mathematics. 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.

J2   The most educational observation we may end with, regarding the timorous state of the modern history of ancient astronomy community, is this: its fiscal rulership can read all that you have just read and, though finding not a digit out of place in the analyses, conclude or pretend that: [A] Ptolemy has done **absolutely nothing** dishonest, and [B] DR should continue to be non-cited[34] for [1] his witchcraft (fn 35) in co-hypnotizing scholars (fn 1) into realizing Ptolemy cheated, & [2] exposing the vile tactics of archons who'll never admit they were wrong to slander[35] R.Newton before even understanding his evidence.

[33] R.Newton 1977 (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 [!] predecessors . . . being obsolete, . . . ceased to be copied.") See Neugebauer 1957 p.145 & Rawlins 2008Q §K3. Neither van der Waerden nor 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 desired "confirmations" of his models. Basic confusions of those who see Ptolemy's derivative astronomy as primary: §M1 [b].

[34] 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 shunnee 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; fnn 1, 7, & 47 item [1]) cannot be openly discussed without fear of archons — or editorial submission to such (fn 1) — we've descended into knowledge-destructive sociopathology.

[35] Among *JHA*-circle herd-talk compliments toward R.Newton and his solid mathematical analyses have been the following: "incompetent" (A.Aaboe), "up a pole" (J.Field), "disreputable" (Toomer 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* ‡1 §C7 & ‡3 §§D2-D3). And see *Scientific American* at fn 52. If taking academe'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 engaging in rational academic discourse. Instead, we've had 4 decades of their dereliction and/or equally inexcusable non-comprehension (despite the author's vain 2002/10/2 request of stonewalling American Astronomical Society chief Milkey that he look into the conduct that has characterized the controversy), while since 1968, JHAD missionary Gingerich — soon after joined by Swerdlow — has (Rawlins 1994S §§B5-B8) lauched jihad against infidels and tried hermetically whackamoling all public doubt on Ptolemy (reincarnating those who burned books and sorcerers in the Dark Ages, to contain another heretical disease), privately contacting any forum or party or person who dissented or was about to: *Nature*, *Science*, *Scientific American*, Rawlins (1974), CSICOP, Martin Gardner (1978), Horace Judson (2004), etc — to assure them that *the skeptics* are dishonest crank incompetents, correctly confident that the non-specialist gullee would believe he need not check alleged supporting evidence of such eminent personages [‡4 p.87]. Apparently, the notion that a MacArthur Genius and a Harvard professor could themselves be either shall-we-say Limited (fn 8) or shall-we-say not-overstrictly-truthful (fn 5) or both, has heretofore seemed just too outré for non-scientists and writers (unfamiliar with the JHAD) to believe, thus popular media (if we naïvely assume power-secretaries' naïvete) continue to be near-100% buffaloed by our dedicated genii. (Rawlins [like even Gardner&Judson] was similarly fooled [1974/11/15, back when he wasn't yet into Greek astronomy], for over a year, by Gingerich saying Newton was just a crank; see Rawlins 1994S §C8.) Question-in-passing: how do scholars of the refined character and competence of Swerdlow and Gingerich keep getting to be eminent in the 1st place? Their JHAD is even now hoping to assert further Dean-Wormeresque Double-Secrecy [§N17] by acquiring Double-Peek fail-safe control over

K APPENDIX 1: GRUSOME TESTABILITY WARS

K1   Delambre 1817 2:284 was 1st to notice that, in Alexandria's 2nd century AD sky, some stars which transited a few degrees above the southern horizon, and were bright enough to have been recorded by hypothetical-observer Ptolemy, were nonetheless not in "his" Catalog — and, by-an-unfunny-coincidence (explored statistically in Rawlins 1982C), *all these unCataloged stars were invisible to Hipparchos*, who observed 5° north of Alexandria, so that his antarctic-circle (fn 31) of invisibility was radially 5° bigger than Ptolemy's (swallowing about 4/3 more sky). Automatically fighting the probable implication while unable to deny any facts, Evans 1998 p.272 resorts to the improbable (§B3 [a]), in order to set aside such simple antarctic-circle testing, speculating that because no previous mass-star-cataloger known to us had resided so far south as Ptolemy, there were no constellations to which he could attach stars in the 5°-wide strip of sky he could see but Hipparchos could not (and which no hypothetical early southern constellator had filled), so we must excuse Ptolemy — excuse him, that is, for not doing what The Greatest Astronomer of Antiquity himself[36] 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 Ptolemism's death-agonies: *alibiing one Ptolemy scientific crime too often requires positing another*. (Check out, for example, fnn 30, 37, & 42.) I.e., even if one accepts Evans' argument, it simply exchanges a charge of plagiarism against Ptolemy for a charge of lying.[37] But Evans' theory fails anyway 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 50ʸ!]— details at Rawlins 1994S §B13. After all: must protect even 'til-now-undefiled Thailand and the antipodean Land-of-Oztrollya [‡4 §A2] from the full truth about the integrity & ability of Ptolemy and his never-too-far-away Muffia public-relations organ.

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

[37] Similarly, a recent paper (Brandt *et al* 2014B; see also fn 20) tries to deny the certainty of Ptolemy's fabrication (from Hipparchan data) of any star declinations δ found in *Alm* 7.3. Various modern scientists have noticed that Ptolemy "proves" his false 1°/cy precession from his 18-star sample using the worst six stars (the "SickSix") and ignores the valid twelve (the "Clean Dozen") which would have accurately yielded 1°.4/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 theory — and then turned around to "prove" 1°/cy precession from said fakes. 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 Ptolemists fumble-lawyering 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 **ignorance** would Ptolemy (uniquely, even for him) acquire — steal — a small set of star-declinations obsolete *by a century* (fn 20) *while purporting that he observed them himself* (*Almajest* 7.3), and insert them into a set of contemporary declinations? **This is proposed as a central new theory in a DEFENSE of Ptolemy's honesty?** Innocently unanticipated by the paper's authors: the 2014 paper's proposed 57 AD date for the former is near the date which Peters & Knobel 1915 p.15 found for when the Star Catalog's mean longitude-error is null: 58 AD. But we know from independent evidences (§I) that the Catalog was stolen by adding false precession 2°40′ 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-deficient fabrication from Hipparchos? — an epoch that's nearly the same for Ptolemy's declinations-list AND his Catalog. **NB**: Ptolemy's SickSix fraud-mimicry in stellar declinations is especially obvious since his outlier Arcturus' δ conspicuously is correct for just over 50ʸ 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 "informatae" 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 ϵ Car [$m = 1.9$] is less than 6° from Cataloged δ Vel: PK886), despite being easily visible from Ptolemy-era Alexandria, (post-atmospheric-extinction magnitudes μ ranging between 3 1/2 and 5: Rawlins 1982C 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 limit[38] μ_o for capture. Even more peculiarly absent from the *Alm* catalog are α and β Gru. Both of pre-extinction magnitude $m \approx 2$ — i.e., of Big Dipperish 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 new[39] constellation. After all, [i] There already was a two-star constellation, CMi (PK847-848); [ii] Ptolemy was inventor of the new[40] asterism Antinoüs, which he formed c.130 AD from six[41] stars "around" Aql (Toomer 1984 p.357).

K2   Stars α, β, and $\delta 1$ Gru are missing from the *Alm* star catalog, though all were visible from Ptolemy's Alexandria ($\mu = 3.3$, 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 ι PsA (PK1021) versus 10° from α and δ Gru, and 7° higher than δ Gru 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 $\mu = 7.1$. 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: γ&β (PK76&77) 11°. Aqr: β&ϵ (PK632&636) 12°. Peg: γ&α (PK316&318) 17°. Hya: β&γ (PK916&917) 22°. So the no-available-constellations argument is slaughtered by [2] the obvious explanation for γ being the Catalog's sole Gru star, namely: of α, β, γ, and δ, only γ was visible to Hipparchos.

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the SickFive, whose weighted mean error vanishes c.59 AD — since he faked it to co-prove false 1°/cy 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^y. 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 goes null c.50^y after the LMO, should just-happen-to-also-be the lone Hipparchos star whose δ-error also goes null c.50^y later than Hipparchos. (To be exact, 52^y after Hipparchos; 56^y after LMO.)

[38] Hipparchos' μ_o was a bit dimmer than 5: §L3. For his deep south sky, virtually every identifiable star brighter than $\mu = 5$ appears in the Catalog. Note: this is about the μ (Rawlins 1993D §J4) 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.)

[39] Evans 1984 had argued for Ptolemy's originality with the equant, but only 3^y later is adducing his UNoriginality to squirm out of a religious paradox. See Swerdlow similarly at Rawlins 1992V fn 43.

[40] Not just the date argues for Ptolemy's Serapic temple being the source of Antinoüs' 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/cot.htm#rhhv for further analysis, plus Rawlins photos of Hadrian's Canopic temple replica (with poolside stone crocodile), as well as of a classical-era Antinoüs statue (Glyptotek, København).

[41] Has it previously been noticed that not one of Antinoüs' 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, ι, κ, λ Aql. (Three of the six λ endings are 40' or 10', vs two randomly expected, a slight hint that Hipparchos observed them after writing the *Commentary*.) The group was named for bereaved Emperor Hadrian's recently Nile-drowned teen boytoy (Rawlins 1992V 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.

L APPENDIX 2: THE MAGNITUDE SPLIT
(AND WHY WASN'T PLUTO KNOWN TO ARCHIMEDES?)

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

L2   A passing 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 look[42] as if the observer were at Hipparchos' latitude L — instead of where Ptolemy's

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[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' Rhodos Island $L = 36°$N. This requires assuming (contra Pickering 2002A §F) that Schaefer's modern Alexandria daytime [mean] 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 stars. Since Schaefer (*op cit*) astonishingly neglected consulting the stars in Hipparchos' *Commentary*, the paper doesn't even realize that (given the virtual identity [§H3] of the antarctic circles of the stars collected by Hipparchos and by Ptolemy) his argument inevitably requires that Alexandria's aerosols were much greater than Rhodos' — again, conveniently, by enough for pseudo-indicating a Hipparchan latitude for the Catalog's main observer. Dense turbidity proponents Schaefer and (less incautiously) Evans propose opacities that are obviously over-high for antiquity. Schaefer (*op cit*) choosing an opacity of 0.23 mags/atm happens to put the star γ Ara at $\mu = 6.7$ (Pickering, *op cit* §B2 independently verified by Rawlins): probability of Hipparchan capture $P < 1/1000$. The only way to make it remotely possible (for 0.23 mags/atm) that γ Ara could have been recordable by Hipparchos would be to adopt (instead of Rhodos City's $L = 36°.4$) the Rawlins 1994L §E4 Hipparchan position (§I6) for observing southern stars at Cape Prassonesi ($L = 35°53'$, vertical distance above sealevel $z = $ c.200m), making $\mu = 6.2$, $P = 1\%$; but Schaefer has never acknowledged that Prassonesi could've been Hipparchos' south-Rhodos observation post. (See discussion at Pickering 2002A §§B2 etc, regarding Schaefer's intelligent [if only slightly mitigating, in this case] argument that γ Ara's low P should be seen in the context of several other similarly situated stars [too few of which are of identifiably bright m], even while he himself remains impervious to the larger context of Pickering *op cit*'s numerous other strong, mutually-verifying clear-atmosphere proofs.) Still at Prassonesi: Evans' preferred 0.2 mags/atm makes $\mu = 5.8$, $P \doteq 6\%$. (But he is creditably willing to admit the possibility of opacity as low as 0.17 mags/atm, which would leave $\mu = 5.4$, $P \doteq 1/3$.) Schaefer's impressively-published case for an opacity which inadvertently worked against Hipparchos' recording γ Ara makes an even more impressive impact upon us when we learn that Hipparchos actually did record γ Ara: it's found at Hipparchos' *Commentary* 3.2.6 (which Schaefer was unaware of at this time, since he didn't consult that central work until Pickering told him about γ Ara face-to-face at the 2001 H.A.D. meeting). By contrast, γ Ara's visibility to Hipparchos is reasonable by Rawlins' 0.15 mags/atm opacity, which has the star's μ at 5.2, very near Hipparchos' capture-limit μ_o, with capture probability $P \doteq 1/2$. (See Hipparchos-capture probability-function by Rawlins 1982C p.363 — and its later independent 2011 confirmation at www.dioi.org/cot.htm#hpbb, explaining why Hipparchos counted precisely seven Pleiades.) Similarly, we check opacity from α Car's attested (Strabo 2.5.14) visibility to Eudoxos at Knidos ($L = 36°40'$). Rawlins' $\mu \doteq 3.2$, while Schaefer's 0.23 mags/atm makes $\mu \doteq 6$ 2/3. The entire thick-atmosphere line-of-alibiing was squushed by Pickering 2002A (the best paper, ever, on the atmosphere debate). This (*ibid* §D9) is the 1st adducement (for this case) of Eudoxos' sighting of α Car ($h < 1°$), also of Hevelius' recording of $\upsilon 1$ Eri (*ibid* 14) — both far too dim for visibility by Schaefer's opacity. Pickering (*op cit* §F) further demonstrates that bright stars were visible *on the horizon* in antiquity — most spectacularly by revelation of the hitherto-unconsidered fact that Hippocrates' and Ptolemy's achronychal rising/setting data for Arcturus and Saturn has to be referring to these objects' ancient visibility **ON** the horizon since achronychal effects *cannot even be defined at*

defenders argue he really, *really* might've been.

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

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

M1   How did too much of the academic establishment get sucked into promoting astronomical history's 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?[45] — but astrologer-mathematician Ptolemy was not a scientist. Not empirical.

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any other altitude than $h = 0°$: see the lucid and irrefutable discussion at *ibid* §F11. Further, thanks to an amazing ms-recovery by B.Goldstein, we now have the fact (Rawlins 1993D §L8) that Ptolemy *said in so many words* that 1st magnitude stars (pre-extinction $m \doteq 1$) 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 ($\mu \doteq 12$); and Schaefer's 0.23 mags/atm similarly entails ancients seeing to 14th magnitude ($\mu \doteq 14$). So: why didn't Archimedes beat Clyde Tombaugh to the discovery of Pluto?

[43] Define split-Vagueness V in §L3's list [a] as: dimmest Cataloged star's μ minus brightest nonCataloged star's μ. Testing Hipparchos' V (γ Ara vs ϵ Cru) at Cape Prassonesi (height $z =$ c.200 m above sealevel) for five assumed sealevel opacities: 0.14 mags/atm (negligible aerosols), 0.15 (Rawlins 1982C), 0.17 (Evans 1987, #2), 0.20 (Evans 1987, #1), 0.23 (Schaefer 2001): $V = 1/5, 1/4, 1/3, 1/2, 3/5$, respectively.
Compare sealevel-Alexandria Ptolemy's §L3 list [a]: V at 0.15 mags/atm, $V = 5/4$ ($\beta1$ Sgr vs α Gru). And for mags/atm = 0.23 , 0.3: $V = 4/5, 3/4$, resp (γ Ara vs α Phe in both cases).

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

[45] The half-century Ptolemy Controversy should have been over in half an hour — had all participants amicably cooperated at the outset and sat down together to compare data and enlighten each other. Had defenders realized early on, before positions hardened in ignorance of, e.g., the significance of Ptolemy's large error for Alexandria's geographical latitude L (§E1) and real 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 gooroos). Decades of ugly and harmful warfare followed. Again: all needless. But as with many wars, when it becomes obvious to most observers who's going to lose in the long run, there is a bloody period when

[b] Were Ptolemy's math proofs so admirable that it seemed incredible for him to have plagiarized data? — but (Rawlins 2003X p.502): what if he plagiarized the math, too? It was long believed that the *Alm*'s spherical trigonometry proofs were original — until 1901, when it was found that they were taken from Menelaos (c.100 AD): Pedersen 1974 p.73 n.9. [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 Reformation-germinating mistake of trying to defend by reason that which cannot be defended by reason. [d] Did damage to Ptolemy's sacred-grant-cow value trigger the shunning (§B) of R.Newton's valuable insights? With the *Almajest* 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 fudges, by deliberately (fn 46), falsely claiming that everybody-did-it (§M2) in antiquity — a distortion that's gatewayed a 180° inversion of truth for a range of ancient-science issues, as detailed at §N, below. Above option [d] was the most likely place for the original flame of rage at Ptolemy-skeptics to have started. Newton used to note that the last century has seen numerous charges of historical fraud in the physical sciences, but none produced a fraction of the ferocity of Ptolemists. [Wherever there's a weak, rationally-indefensible tenet, advocates are left with no other way to protect it than (e.g., Rawlins 2017C fn 1)] by suppression, banishment, indiscriminate argumentation, and circulation of way-overdone baseless or irrelevant personal denigrations against opponents. So when we see such phenomena we should sense said weakness. As a general rule that can save plenty of time and bother:

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

M2   Admittedly-non-peer-reviewed *Scientific American*'s Swerdlow-Gingerich-inspired premature "Acquittal of Ptolemy" (ScAm 1979), published in anti-Thoughtcrime horror at R.Newton's scientific 1977 exposure of Ptolemy's career of fabrication, could hardly have been more ill-timed (fn 12) or more extreme in fundamental-premis misunderstandings of Ptolemy in particular **and ancient science in general** (most of which survive immutably to this day among his remaining band of believers). It verbatim-echoed the already-echoed (§A) promotion of Ptolemy as the "greatest astronomer of antiquity". (Same modern clique sells Babylonian astronomy as primary science, too, though it's just as derivative: §N13.) In truth, Ptolemy was [a] the occultist author of astrology's bible, the *Tetrabiblos*, and chief mathematician for his Serapic religious cult at its Canopic temple; [b] compiler of derivative (§N20) Euklidesque "handbooks," perceptively designated thusly (see References below) by the first able translators of his *Almajest* (Manitius 1912-3) and *GD* (Stückelberger & Graßhoff 2006); and [c] used his mathematical talents to hustle occultism by faking the overexact truth of his Serapic sponsors' belief in celestial predictivity, presumably suggesting a comforting parallel superstition-superadvert for astrology's predictive efficacy in human affairs. Ptolemy's "Acquittal" promoted Swerdlow-Gingerich's idea of established-fact: it was the "established ethic of ancient science to report only those observations that best confirmed theory and to disregard [i.e., destroy] the rest" — without letting on that [*no ancient witness is cited for this "ethic"* (just modern genii)]. [1] So-called "observations" repeatedly in error by ordmag a **DEGREE** (see, e.g., §D ["Illegally Blind"], and especially at fn 47) obviously never happened in the 1st place, so there were never any Ptolemy observations to select among. [2] The durable Neugebauerian mantra that effectively-dishonest data-selecting was standard behavior for The-Greatest ancient scientists: [a] cannot survive 5 seconds of critical examination (§M3), and [b] is based on circularly taking astrologer

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the leader of the losing army begins also to lose track of priorities and will not give up until the enemy is knocking at the bunker door. During this final phase of real wars, millions die. For nothing. But the leader's vanity. In a hypothetical academic war, the whole sub-field could be made for decades to look foolish. For nothing. But, luckily, academe has no vain leaders. So it never happens.

Ptolemy as the quintessential or ultimate ancient scientist — knowingly[46] rejecting the inconvenient fact that his genuinely empirical predecessor Hipparchos, though also motivated by astrology (at least in the period −157 to −145), published theory-discordant data, solar (fn 15), lunar (*Alm* 4.11), and stellar (fn 47). The attempt to alibi Ptolemy by wrenching academe's view of ancient astronomy *to fit him* has caused as much damage to modern scholarship (§§M1&N) as Ptolemy visited upon ancient and (above, §A) medieval.

M3   "Acquittal" adds that ancient astronomers "were mathematicians who concerned themselves with proof, rigor, logic, and consistency rather[47] than with observational accuracy." Gingerich 1976 p.477 approvingly quotes Neugebauer 1975 p.108, "It makes no sense to praise or to condemn the ancients for the accuracy or for the errors in their numerical results. What is really admirable in ancient astronomy is its theoretical structure. . . ." See also Neugebauer on Aristarchos' data as non-empirical (Neugebauer, *op cit* pp.642-643; Rawlins 2008R §A1). How then did Aristarchos and Archimedes (*idem* & Rawlins 2012T §E1) find the solar diameter to ordmag 1′ accuracy? How did ancients 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

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[46] A month before publication, Rawlins informed *Scientific American* of the Hipparchan 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 crock that ancient scientists weren't accurately empirical, Evans 1987 reports that on 1981/7/16 he observed from Seattle the longitude of star λ 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 impossible 135/10/1 lunar report (rightly deemed fraud by R.Newton 1977 p.190), but (seemingly more relevantly) also similar to outdoor-Hipparchos' huge errors twice (*Alm* 3.1) when also comparing a star (Spica) to the Moon (during eclipses of −145 & −134). Pointing to all 3 large misses in the eclipse-star observations (by himself&Hipparchos), Evans triumphantly concluded: "No better demonstration could be wished" of 1° uncertainty in naked-eye observations, preaching that such Real-Science work "might temper [Ptolemy-skeptics'] judgements regarding the precision achievable" by ancients, not-very-implicitly asking: So, Mssrs. Newton & Rawlins — NOW what's so criminal about Ptolemy's one-degree-erroneous observations?

**Evans' and Hipparchos' three large outdoor errors clearly vindicated the Acquitters!
And all the many *JHA* experts who had for months vetted and refereed Evans 1987!**

Until Rawlins 1991W fn 288 and Rawlins 2009E §A & fn 7 applied the theory that Evans and Hipparchos had simply committed a sign-error in parallax when reducing each observation — a theory that decades later also proved just as neatly fruitful for Regulus' hitherto-inexplicably huge −35′ Hipparchan error (‡3 §B6). Recomputation showed (*ibid*) that **all four** ordmag-1° observational errors of Evans (Seattle 1981) and Hipparchos (Spica twice and Regulus) shrink to ordmag 1′. (I.e., all four errors were primarily those of reduction, not observation.) Warned of his sign error by Rawlins 1991W and in 1997 by Thurston&DR, while Evans 1998's text was still unfinalized, its pp.257-258 ever-so-subtly Memory-Holed his 1981 no-better-demonstration data and switched to a different eclipse (no observed data recorded: from 1977 Spokane, an eclipse never mentioned [Rawlins 2009E fn 7] during Evans 1987), continuing, while sticking to only Hipparchos' slips, the same Sermon-on-the-Muff (‡3 §B4), just reprinting his 1987 argument (Evans 1998 pp.256-259) header "HIPPACHUS [*sic*] AND PTOLEMY ON PRECESSION"] & 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* Assoc Ed [now Editor!]'s citational integrity (*DIO 8* 1998 p.2) & [2] of cultist disconnect between evidence & conclusion: **when the former collapses, the latter stands entirely undiminished**, an invincibility also durably evident throughout the revealingly flip-flop history (Rawlins 1992V §§C31-C33) of the Ancient Star Catalog controversy. Today, Evans continues (Rawlins 2009E §A2; *DIO 9.1* 1999 p.2) decades of evading Thurston's & Rawlins' questions on the matter. (In 1997 June, DR asked Evans face-to-faces. Evasion. DR then asked Evans for his office phone number so the two could confer. Evans refused. And *JHA*'s determination to shun permits Evans to face no consequences for such stealth. Other than 2013 appointment to *JHA* Editorship.) Like Gingerich (§A; fn 5), Evans cannot ever be shown wrong by non-club outlanders. [On the of-course-Disappeared "notes from that [eclipse] evening": ‡4 §B6.]

1994L Table 3.) How could Hipparchos measure all of his 3 eclipse-based star-longitudes (Rawlins 2009E) to similar precision? (Note, too, the 3 neatly-interconnected 1% hits hypothesized at §N10.) How were solstices fixed (§N7) to ordmag 1^h? Whence arose a Greek stade-length consistent with knowing the Earth's size to 1% (fn 49 below)? Was it just a series of miracles that all 4 surviving ancient collections of star declinations are (Rawlins 1994L §§F5-F9 & Table 3) consistent with each of the 4 observers knowing his latitude to ordmag 1′? From where (Martian visitors?) did the ancients obtain all 3 lunar months to (Rawlins 2018U §D) one part in ordmag a million or better? For the synodic&draconitic months: nearer ordmag 10 million! (Most of these accuracies were unknown before *DIO*.) *It would not have been possible for these measures to progress to such admirable accuracy, if researchers had merely tailored data to previous values.* Back in 1979, did Ptolemists ever take 5 seconds to contemplate such a self-evident & devastating point, ere committing to an obviously uncertain position so deeply that [fn 34] there could never again be a chance of turning back: with the courage of a lynch-mob, gang-smearing as an incompetent crank (‡3 fn 34 & ‡4 fn 2) prominent Johns Hopkins physicist R.Newton, whose analyses of Ptolemy employed math which historians-of-science couldn't even understand much less perform. Before such fateful investment, *did they even know* (fn 12) of the inescapable (§F2) Mercury inconsistency, or (§H1) the absent-error-waves test? ***Can Muffiosi show they ever even looked for an explanation of those remarkable millionth-precision lunar-period accuracies*** (‡3 §I10), *BEFORE committing themselves forever* (fn 9) decades ago to the above fantastic Neugebauer-klan notion that Greek astronomy was more theoretical than empirical? — and, in this connexion, to such a fragile jest as deaf-to-all-evidence promotion of an indoor faker (notorious as such among astronomers for centuries) as The Greatest Astronomer of an antiquity about whose outdoor astronomical empiricism they obviously understood a great deal (§N) less than nothing. Hopefully, these considerations will warn today's budding archons how a messtablishment — initially from preconception and careless science, then in heedlessly hot outrage at heretical challenge (Swerdlow at §B2, longtime *JHA* Editor M.Hoskin at fn 3), eventually ever more stubbornly and far-fetchedly as contrary evidence mounted (fn 50) — ultimately got itself regressively mired down ever deeper into a spectacularly ludicrous position, from which it has still not even seriously *started* to recover.

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

Those archons who for generations have controlled journals, conferences, and funding in the history-of-ancient-astronomy field 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 retards and fanatics of the history-of-science discipline, their plan could hardly be improved upon. (Which actually would be quite unjust, in that such men as Neugebauer, Aaboe, Britton, and others are brilliant despite their blindspots.) Think this too strong? Well, if you have the independence to actually read what follows in this section, you'll be *taking evidence before making up your mind*. (We trust this doesn't break a cult principle.) The specific delusions that have long been orthodoxy in this unreal field:

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

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

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

N4   Accepting that Greek astronomers were not primarily empirical (§M3; *DIO 1.1* ‡1 fn 24; Rawlins 2008R §A1 & fn 20). Among objections beyond the *a priori*: far too many extremely accurate ancient measures survive (§§M3&N1, fn 8 [3]).

N5   Because of own mis-signing of parallax-correction (fn 47), teaching in an Oxford University Press textbook that errors of ordmag *a degree* (exceeding the lunar diameter!), as repeatedly found by Newton in Ptolemy's alleged observations, were ho-hum-normal for ancient instruments, as Neugebauerians believe (e.g., §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' Cyclicities: Rawlins 2002B §H.

N7   When a ball is tossed upward at 0^s and caught downward at the same height 4^s later, most of us know it maxed at 2^s. Yet, from his own astonishing failure (R.Newton 1977 fn 20) to understand this junior-high maximum-height problem, MacArthur-Genius Swerdlow keeps asserting (with Evans' and Jones' evident assent: fn 11) that solstices could not be determined accurately, and likewise (fn 8) that outdoor maximum Venus elongations must've been so crude that Ptolemy was forced to compute them indoors. His argument (perhaps unique in all history-of-science scholarship): ***the time of maximum cannot be well determined because, near maximum, the object is hardly moving***. This sort of spectacular embarrassment is what happens when a history-of-astronomy crusade depends on those with inadequate gifts in positional astronomy, spatial relations, and common sense. (Assuming Swerdlow is not knowingly ladling nonsense to vulnerable archons too predisposed and sub-genius to recognize the prank.) Due to just such *JHA*-published pseudo-science, Ptolemists even perversely teach — complete with now-ironically Pompous sneers (quoted, R.Newton 1977 *loc cit*) at sub-*JHA* untouchables — that ancient equinoxes were more accurate (fn 11) than solstices, from their own unfamiliarity (e.g., *JHA* Editor-to-be Evans at fn 11), with [a] the instrumental and astronomical problems involved (R.Newton 1977 pp.81-82; and

Rawlins 2018U §F1, whose eq.10 quantifies for the 1st time the ordmag 1^h effect of deviation from quadraticity: §N19 below), not to mention [b] ancients' historically uniform choice of solstices not equinoxes for yearlength-determination. Have those who've been disbelieving Greeks' ability to measure solstices accurately (Swerdlow, Evans, Duke) noticed that the newly available papyrus *P.Fouad* 267A (§N19) has tried to enlighten them by directly surprise-testifying to an ancient solstice which was accurate to ordmag 1^h? Just-luck? (Like another trio of just-lucks at *idem*? Meanwhile, note that Duke not only wrongly doubts that Greek observations were sufficiently accurate for trustworthy solstices [*idem*], but agreeably if mistakenly proposes that Hipparchos' -134 solstice was 5^h off, when in truth its error was only 1^h: see van der Waerden at Rawlins 1991H fn 4; also Rawlins 2018U eq.10 & Table 3.) We can test the point: the mean systematic error of Hipparchos' equinoxes was 7^h (consistent calculational conclusion of Britton, Newton, and Rawlins: summarized at *ibid* §B4), while in spite of 6^h rounding, the errors in recoverable ancient solstices (one by Kallippos; one by Aristarchos, two by Hipparchos: *ibid* Table 3 & eqs.1&2&27&4) are $+3^h, 0^h, +2^h, \& -1^h$, resp, indicating that Hipparchos' rms solstitial systematic error ($1^h.6$) was more than 4 times smaller than his mean equinoctial systematic error. Unrounded ancient equinoxes doubtless had less scatter than solstices, but the latter obviously suffered smaller systematic problems (remember, too, that the ancients [needlessly] worried about [and corrected-for: ‡3 fn 97] the effect of several arcmin of solar parallax, 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.[48]

N8   *It will be convenient to here list in one place prominent instances, of the pile-on passion of the most elite historians-of-science, for trashing ancient empirical science*: Neugebauer 1975 pp.108, 284, 642-643. Gingerich 1976 p.477. ScAm 1979. Van Helden 1985 pp.6-7 & 168 n.8. 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 & ‡3 fn 8.) Another interlude: Ptolemy's *Tetr* 1.1 astrology-promo suggests he's fighting more resistance than historians-of-science know, "most events of 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.

N9   We recall how "Mr. History of Astronomy" (AAS-HAD *Newsletter* #51 Page One) dream-creates his idea of ancient realities. Gingerich 1976 p.477 on a temple-bound faker-mathematician geocentrist-astrologer out in kookburg Canopus (‡1 §U), who was just as skills-ineducable by his real-science world (‡1 §§E-F) as the JHAD cult is by its: "we can easily imagine Ptolemy surrounded by assistants and graduate students at the famed Alexandrian library." But on the most original genuine ancient scientist's coherent heliocentrism: trivial by *JHA* criteria (§H2 [b]), just a passing "splendid speculation tossed out during a vigorous discussion between the Alexandrian mathematicians"(www.dioi.org/sti56.htm).

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[48] Irony [due to yearlength-estimaters' dependence on truncated (Rawlins 2018U ‡2 §C1) prior solstices]: despite 4 known reliable solstices (Rawlins 2018U Table 3), no known ancient got an accurate yearlength. In 1977, Brigham Young Univ astronomer H.Kimball Hansen conceived a simple method ancients could've used: [1] Find a stable stone point on a hill which near an equinox casts a North-South shadow at apparent noon on a stable stone surface below, both stone locations being more secularly immobile than human equipment. [2] On some date around an equinox (no need to be just at one, merely when solar declination-motion is near-maximal), mark where the noon shadow is. [3] Note when it returns there 20 years later, and divide the interval by 20. The result, in just 20^y, will be several times more accurate than any yearlength known to have been adopted in antiquity (even though these were based on intervals an ordmag longer) all of which were seriously erroneous, for reasons (analysed at *ibid* §§C-D&Q) 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[49] hit-success of the spare atmospheric-refraction theory that explains and fits both ancient Earth-

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[49] Only 2 ancient Earth-circumference C values were widely adopted: Eratosthenes' (really Sostratos': Rawlins 2008Q) 256,000 stades, and Poseidonios' 180,000 stades later. Ptolemy 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. Hmmmm. 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, during *the 1/3 of a century* (summary and citations going back to 1982 provided at Rawlins 1996C fn 47) since *DIO* published this hyper-neat triple solution. (Are they even aware that navigators' familiar formula for the horizon's "dip" has been based upon identical 6/5-mathematics for over a century?!) These 2 *DIO* matches *exceptionally* are achieved *without fudging the stade at all*, but just by adopting the standard 185 meter value. [***Thus serving as the final, controversy-ending proof that 185m was indeed the true length of high antiquity's stade***.] Our achievement here may be compared to the controversy's endlessly wheelspinning metrological-solution literature: sniffing&sifting through ancient lore in search of hints of oddball stades, which of course existed all over the place before the Ptolemies presumably regularized the measure by defining their empire's royal stade at 185 meters, commonly miscalled "Attic", which we may instead (below) dub the "sexagesimal" stade. Unique in the centuries-long history of the debate, *DIO*'s solution is ***physical not metrological***. Again (§N10), note that this resolution matches all 3 targetted data within about ***one percent***: [a] Eratosthenes' C, [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-deflating surprise that the Pharos solution of the famous Sostratos-Eratosthenes C is geographical, not astronomical. [Speculation follows.] But reflation is effected when we realize that the unclever (but low-refraction) presumably-Kleomedean-astronomical-survey-based 185 meter stade implies $C = 216000$ stades (since the product equals actual $C = 40$ million meters) but 216000 is the cube of 60, hinting that (before Sostratos cleverly but wrongly found for $C = 256000$ stades, c.270 BC) scientific surveyors had obtained an accurate C to which Greek science had naturally applied ***standard Greek sexagesimal division*** to the Earth's meridians (the process' step 1 is even attested: ‡3 fn 111) to define the stade so that $C \equiv 60^3$ or 216000 stades. I.e., sexagesimalization (triple division by 60) of Earth-C 40 million meters produces a geodetically correct "sexagesimal" stade of 185 meters, fine for 600 stades/degree, but not for Sostratos-Eratosthenes' later-famous (but seriously-too-high) 700 stades (of 185m each) per degree. But: when did this hypothetical achievement occur? Traditional games at the Olympic stadium began before 300 BC, but when were the stade-long-footrace markers now found there (Engels 1985 p.298) established? (If athletes ran in various states' races, there must have been gradual standardization.) Dinsmoor 1950 pp.250-251 presents for 5 ancient stadiums the disparate lengths of the traditional 1-stade footrace, where the sole 185m one is also the only one (Athens, reconstructed +143: Olympic *Decline . . .* Chap.2 [Mod.Libr.ed 1:41]) that's post-Ptolemy I. In the Hellenistic world, where else than Egypt would terrain allow a long flat North-South arc, of ordmag 1000 km. Given that the Alexandria-Meroë arc's curvature is apt to a meridian circle of $C = 39870000$ m, a Strabo-attested 10000 stades interval (at 700 st/degree) signifies ancient knowledge (within 1 part in 100s), that the cities' latitude-difference is $14°1/4$, thus able reliably to indicate Earth-C so accurately that hypothetical surveyors' 185m conclusion was trustworthy to within ± 1 m. Was Kleomedes' famous Alexandria-Aswan legend a myth (DR's former opinion), or a remnant of Ptolemy I's post-conquest land-survey of his empire? (With similar possessiveness, less scientific William the Conqueror reckoned his own new booty in the Domesday Book.) This would be technically possible using theodolites (transit instruments) we know existed (consistent with the superior technology that effected the conquest of Babylon, which lacked such advances as theodolites and trigonometry tables), given Timocharis' accurate theodolite-based stellar declinations c.−300 (*Almajest* 7.3; Rawlins 1994L). Was Timocharis chief of the project? Hitherto un-noted credit: Timocharis knew Alexandria's $31°12'$ latitude *precisely* (*ibid* §F6), while neither of the other two later star-observing Alexandrian astronomers quite did so (*ibid* §§F7&F9). From solar observations (e.g.,

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size measures, as well as the standard stade, ***within about one percent in all three cases*** — instead too often clinging immovably to disgracefully-traditional yardstick-fiddling folly, arguing that Eratosthenes' Earth-size only *seemed* too high (he *musta* been measuring with a runty stade), a flagrantly *ad-hoc* theory which has never done better than one match, usually just approximate, at that. (Among those who promoted or respected such a conveniently-flexible-stade approach: M.d'Anville, A.Letronne, F.Hultsch, K.Müllendorf, E.Lehmann-Haupt, A.Diller, J.O.Thomson, P.M.Fraser, I.Fischer, C.Sagan, J.Dutka, A.Stückelberger. It should be gratefully noted that those who resisted this popular, endless *ad hoc* road to noplace include: P.Gosselin, E.Bunbury, O.Neugebauer, D.Dicks, G.Toomer, D.Engels, D.Rawlins, J.Berggren, & A.Jones.) Said preference for an eternally-unsatisfactory non-solution suggests (after decades) the possibility that the community cannot even understand the elementary physics of refraction's effect on Earth-measurement — this, despite pablum-level instructive clarification of the problem (Rawlins 1992V §§A5&A7) by taking it to the extreme thought-experiment case where horizontal light rays' curvature equals the Earth's.

N11   Rejecting for most of the 20th century spherical trigonometry's early existence, as proven by Diller's untouchable, uncitable, but unflawed 14-for-14 hit-record (‡3 Table 1; Rawlins 2009S Table 2) for his theory that Hipparchos' *klimata* were computed by spherical trigonometry. Yet, despite that remarkable, and (for this field) unparalleled fit-success, Diller's brilliant 1934 discovery remains — for [84]y now — unadmitted by a *single* member of the NOOOObody-steps-outta-line Neugebauer clique and the JHAD's present inheritors of its flawless cult-discipline tradition. Impressive. Instead, Jones 2002E just baldly, arbitrarily pollutes his competitors' long-standard data-base (about as subtly as at §N18, below) and proposes a scheme so crackpot that **he doesn't dare tabulate it** (a glaring central omission unnoticed by *JHA*'s breakfast-lunch [fn 3] refereeing) — since doing so would reveal it doesn't even fit his own fudged version of the data! (No other scholar investigating the Hipparchos klimata has failed to produce a table exhibiting his theory's fit-percentage: Diller, Neugebauer, Rawlins.) Given Thurston's and Rawlins' repeated observations, from 2002 on, regarding this paper's demonstrated amateurishness (www.dioi.org/biv.htm#dvck), destructiveness, and slyness, its non-withdrawal has by now become a conscious imposition. (Or, as with §N7 or §N19 or fn 50 [Farnese], how can we even tell whether or not we have here just another hoax-test upon *JHA* refereeing?)

N12   Despite considerations that have been obvious right along (e.g., Rawlins 1991W fn 53), too many scholars keep believing (*idem*) that sophisticated 3rd century BC Greek

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Philo's at Meroë: Rawlins 2009S §C), the latitude difference between Alexandria ($31°12'$) & Meroë ($16°57'$) was knowable angularly as 2 3/8 sixtieths of C. (At the later standard of 700 stades/degree, this is 9975 stades, only a quarter-percent short of the Strabo-attested distance of 10000 stades.) But, given the habitable Nile Valley's narrow sinuosity and the non-trivial longitude difference between Alexandria & Meroë: how would the survey team measure the 1578 km latitudinal N-S difference between the two cities, in order to divide by 2 3/8 60ths, to find C? If the proposed survey was thorough & scrupulous enough, we may speculate that this would've been possible, though quite laborious. (But: more laborious&expensive than Alexandria's non-speculative achievement of erecting&*maintaining* the Lighthouse?) Surveying had been a vigorous science in Egypt for over 2000y before Ptolemy I, as witness the Great Pyramid's precision (*DIO 13.1* pp.2ff). Indeed, Egypt's traditional latitudes near Giza-Heliopolis were better than C.Ptolemy's (Rawlins 1985G p.260). But Kleomedes hints at a direct N-S arc. Pondering this, we note: an arc-path due south of Alexandria, virtually along the $29°.9$ E meridian, could be measured *without being interrupted by the Nile or tough-grade mountain at any point* all the way south to Meroë's latitude (already measured by Philo: ‡3 fn 111) on that arc. Precisely reconstructing the numerical achievement: presuming measurement c.−300 of the 1578 km terrestrial arc south from Alexandria ($31°12'$) to Meroë's Philo-determined latitude ($16°57'$), $14°1/4$, the stade would have been indicated to be $1578000m/(14°1/4)/600 \doteq 185m$. [Note added 2018/4/24. DR's researches have ultimately realized a hitherto-unperceived 3rd BC century Greek-science dichotomy: adoption of Babylon's division of the circle into 360ths (degrees) for sky (Rawlins 2012T fn 3 & §E), but passing use of 60ths for Earth. Perhaps because the latter scheme neatly produced a unit pretty near equal a traditional stade, while the former yielded a unit c.10 times larger or c.6 times smaller.]

astronomers did their high-precision celestial work (*Alm* 7.3) by recording angles in the old pedagogical tradition of clumsy fractions of right angles, etc., a position recently undercut by the high-school-level discovery of the previously-unperceived fact that Archimedes' solar diameter was measured and bracketed in degree-fractions (Rawlins 2018U fn 4)

N13   Selling crude priestly Babylonian indoor astrology as "impressively accurate" (Jones 1991H p.118), and so brilliant it inspired Greek astronomy, according to Neugebauer [1975 p.622 believes in "the advanced state of astronomical techniques" in Babylon] and others of his persuasion (Rawlins 1991W fn 73; Rawlins 1996C fn 128) — even though the sole empirical datum traced in either temporal direction is Greek→Babylonian (Dicks 1994 fn 37). The greater antiquity of raw records from Babylon may say no more than that clay outlasts papyrus. Unlike for Greek astronomy, no record exists of how Babylon arrived at its naked celestial tables (Dicks *op cit* §C4; *DIO 13.1* ‡2 §H.) Clue: virtually all useful astronomical texts from Babylon *post-date its conquest by Greece*, and its periodic functions are not trigonometric ones (Greek astronomers had trigonometry from no later than c.130 BC) but approximations thereto, by zigzag or even step functions, suggesting (‡3 fn 120 below), though not necessarily proving, mere derivative degeneration from its conquerors' superior technology. (See also Rawlins 2018U §J4.) 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, 35°, [a] is found strictly in Greek records, not a word on *L* anywhere in cuneiform material, *another crushing blow to Babylonianist pretensions*, and [b] is too far north by 2°28′! — 148 nmi. Finally, while Greek planetary order was physical — Mer-Ven-Mar-Jup-Sat — Babylon's was astrological, Beneficent→Maleficent: Jup-Ven-Mer-Sat-Mar.

N14   Failure to learn anything from the first of *DIO*'s eclipse-cycle solutions (‡3 §I33), 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 luni-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 2009E fn 7.) This is also the 1st irrefutable evidence for ancient use of sidereal→tropical transformation of period-relations (Rawlins 1996C eqs.26-27), a process later extensively employed in *DIO 11.2*, which allows (here, in fn 8 [4]) reconstruction of Venus' accurate pre-blunder synodic motion.

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

[A] Without even being told, all positional astronomers instinctively know that the secret of ensuring high accuracy for a 2 event-based celestial period *P* is just to wait for a large enough number *N* of returns, ensuring a huge time-interval, t_1 to t_2, so that the error in deduced $P = (t_2 - t_1)/N$ caused by the errors in t_1 & t_2 is trivialized by the enormity of *N*. (How else could the ancients determine [*Alm* 4.2] the synodic month correctly to within well under 1 time-sec?!) [B] *Even Ptolemy knew enough* (‡3 fn 119) to use very long intervals when faking non-periodic arc/time estimates of solar, lunar, & planetary speeds. [C] The short planetary periods of *Alm* 9.3 are obviously not directly measured since they are [i] **not integral** (each has a remainder of a few degrees, from the cycle's imperfection), & (see item [D]) [ii] tropical, not sidereal. [D] Genuine, huge, observed integral **non-remaindered** planetary period-relations are listed in Ptolemy's *Planetary Hypotheses* (Neugebauer 1975 p.906 Table 15), mostly on the order of 1000^y, verifying to all but splice-dreaming JHADists that long cycles were recorded in antiquity. These vast planetary cycles are all listed by Ptolemy in *sidereal* years, which is JUST the type of cycle one obtains directly (no need for indoor splicing) from a centuries-separated pair of raw *outdoor observations* of stationary points near the same star, as explained by Neugebauer 1975 p.390, **producing period relations without remainders**, just as in *PlanHyp*. (Neugebauer *loc cit* also supplies centuries-long sidereal planetary periods for each planet: again, no remainders.) [E] There are many ancient attestations (§N16) to *direct* determination of long celestial periods, but no attestation (or purpose!) for stringing-together short ones to fake very long ones. *Ibid* p.555 produces an atypical ancient text that for Mars splices sidereal cycles of length 32^y (5° short of 15 synodic revolutions) and 47^y (4° beyond 22 synodic revs), to produce 79^y (1° short of 37 synodic revs) with the advantage of reduced (*but still non-zero*) remainder. (Neugebauer or source mistakenly renders the three day-remainders as degree-remainders.) But there's zero attestation for the hilarious idea that any ancient scientist did (or would expect to) construct a reliable 1000^y period-relation from such crude (low *N*: item [A]) short-time-base cycles. Nor could he expect to indoor-create, from short and **remainder-polluted** period-relations, a neatly integral (unremaindered) period-relation of the direct outdoor-obtained type which was already empirically available anyway (& *more accurate by an ordmag*) without such needless fiddling, & upon which (Rawlins 2003J) all *Alm* 9.3's short periods were ultimately based. [F] The *Alm* 9.3 Jupiter 71^y tropical cycle's superficially-inexplicable big remainder (while the well-known 83^y tropical cycle was available with a **50-times-smaller remainder**) proves its historical descent (Rawlins 2003J eq.40→eq.44) from an unremaindered outdoor-observed 427-sidereal-yr integral period-relation, not the reverse (a fiction made fact by Neugebauer 1975 p.391). See Rawlins 2003J §H4.

N16   Out of typically excessive and (www.dioi.org/thr.htm#bsvx) uninformed certainty that 13th century BC Babylonian observations couldn't have occurred, privately scoffing at & non-citing as utterly, *a priori*-ridiculous the only solution (‡3 §§I34-I37) YET discovered for (any, much less) ALL of the last 3 hitherto-unsolved anciently-adopted lunar motions ([A] System A; [B] draconitic; [C] Ptolemy's last lunisolar equation), namely: period-relation **ratios** from eclipse cycles exceeding 1000^y (all 3 stable, due to integral [or half-integral] anomalistic returns), with common integral factors removed, as at *Alm* 4.2&6.9 where factors 17&10, resp, are divided out to simplify the ratio. (Our long-cycle teaser at §N14 lacked such divisibility.) Consult esp. the *half-dozen* neat evidences and fits (Rawlins 2002H §§C3-C9) backing the theory that the draconitic month was determined by Hipparchos, by using the very same back-end −140/1/27 eclipse he'd used (*Alm* 6.9) when first applying (almost as accurately) the same eclipse-period method. (Debate-averse Jones privately produced a *single*-item retort [which insta-melted upon examination: Rawlins 2002H §D], while ignoring all 6 shockingly obvious positive evidences, possibly a non-scientist's receptivity to data contrary to invincible preconception?) These empirical RATIOS at last explain how ancients determined lunar motions (which Ptolemy&cuneiform express **as** RATIOS, after all), all accurate to 1-part-in-ordmag-a-million or better. The proposed method: [1] is bi-attested (*Alm* 4.2 and 6.9), while no other pre-100 BC method

is attested at all; [2] is the sole ancient method even nearly capable of such hyper-accuracy; [3] automatically spits out ratios; and, [4] inducing the three solutions without manipulating 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 (§M3) significance of the *Almajest*'s three lunar periods' high accuracy — but to then let shunnishment dictate refusal to cite the 1st solution (§N16) anyone has yet achieved, for **how** these periods were obtained?! That's non-citation with an *impressively* unanimous lockstep. . . . (Is there a prize we don't know about, for this special brand of perfection?) To summarize, cultists keep secret from *JHA* readers and the larger public BOTH: [1] the historically revolutionary empirical significance (§M3) of the inescapable fact that all three ancient lunar-speeds are micro-accurate, **and** [2] the eclipse-cycle source (§N16) of such accuracy. (Let *Animal House*'s Dean Wormer top THAT Double Secrecy.) And don't miss the saddest&sadist-masochist part: all disbelieving archons are religious Babylonianists, but their coherent disdain for target-heretics trumps even worship of Babylon, as their rabbotic shunning requires every single cringing cultist to forgo reveling in the *mathematical recovery of the greatest heritage from Babylonian astronomy*, not to mention **the earliest major scientific achievement by any civilization**: observation and *preservation for a millennium* 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-cited 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^u = 781^y case does survive (§N14 & ‡3 §I33). [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 Kallippic 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 revive lost antiquities? Were the earlier data extant, there'd be no inductive mysteries here. Who desires a field with no challenges, no advances beyond texts?

N18   Rigid, total, as-usual-unanimous, high-odds-oblivious rejection of 5 new data-match-based proposals central to the field: [1] Computing from his saros-based 4868^y Great Year, Aristarchos originated the "Babylonian" month, 29^{d}191°00′50″, decades before Babylon (‡3 §G4; or Rawlins 2002A eqs.4-8). [2] DR's 2001/6/27 British Museum lecture showed (*ibid*: eqs.9-11; or ‡3 §G5) Aristarchos applied to this the Metonic cycle (235 months ≡ 19^y), resulting in his Metonic "tropical" year $Y_{As} = 365^d 1/4 − 15/4868$ (fatefully off by 6^m, 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. 170^v (data at Neugebauer *op cit* p.601), τ ξ ε δ′ κ′ ξ β′ or 365 4′ 20′ 60 2′; if expressed as the continued-fraction 365^{d}1/[4 + 1/(20 + 2/60)] (Rawlins, *op cit*: eqs.12-13), this is $Y_{At} = 365^d 1/4 − 15/4868$, verifying above item [2]. All without altering any Vatican document number and much aided by Neugebauer 1975 p.602's perceptive interpretation of ξ as sixtieths. [4] The sidereal-year companion data, Aristarchos' τ ξ ε δ′ ι′ δ′ or 365 4′ 10′ 4′ from Vat. gr. 381 fol. 163^v (Neugebauer 1975 p.601), we write as continued-fraction 365^d + 1/[4 − 1/(10 − 1/4)] (‡3 §G2; Rawlins, *op cit* fnn 14-15), yielding sidereal year $Y_{As} = 365^d 1/4 + 1/152$ (good within a few time-seconds), again altering no Vatican-ms digit; and, again, a hit: the

interval from Meton's −431 solstice to Aristarchos' −279 solstice is just 152^y. [5] The difference between Y_{As} & Y_{At} **IS PRECESSION**, suggesting geomobilist Aristarchos' apt pre-Hipparchan discovery of it. His value is near 1°/century, a much-too-low false rate, later rounded to exactly 1°/century & adopted by Ptolemy. Having adamantly rejected all 5 of the above ideas, from the floor at the 2001 event, Jones 2010B (pp.21-22) won't cite any of the 5 even while covering the same documents. Unable to find fault with any of the foregoing propositions' math, Jones (*ibid* n.27) vandalizes their data-bases by (unlike Neugebauer, *loc cit*) arbitrarily stripping off all accents, never letting his readers know of the bare existence of *any* of the 5 proposed advances that the unstripped data encourage.

N19   Resisting unwelcome results from newly-translated papyrus *P.Fouad* 267A:
[1] The papyrus' vindication of the previously unknown Rawlins 1991W (§§K8-K9&M4) discoveries that Hipparchos sought a −157 S.Solstice and that he (twice: *idem*) used Kallippos' 365^{d}1/4-year solar motion. Both his −157 solstice (a different day: Rawlins 2018U §§L-M) & use of Kallippic motion are explicitly provided on the 1900^y old papyrus. (Kowal & Van Brummelen have enjoyed similarly unexpected years-later vindications, which the reader should look up, to share the felicity.) [2] Simultaneous solution (*ibid* §§K4-K5 & eq.25) of *P.Fouad* 267A's decade-old double-mystery: [a] Why did it specify daytime for the −157 solstice, though Duke's accurate calculation from its data got nighttime? [b] What exactly caused the odd remainder of its tropical year, 365^{d}1/4 − 1^d/309? The field was informed of these discoveries in 2015. Inert reaction: ‡3 fn 66.

N20   Confusing the almanacs and handbooks of extant derivative science — cuneiform texts (§N13) and *Almajest* (§M2) — with primary, which is not very extant. Possible practical reason: it's easier to raise grants for analysing existing works than lost ones, and it helps hype to push the former as central — though the probability is minuscule that ancient watershed-research astronomical manuscripts would be numerous enough (compared to handbooks: §M2) to have survived to the present. (Archimedes is the rule-proving exception in mathematics, but even his work on solstices [noted at *Alm* 3.1] is lost.)

N21   Carrying §N20's handbooks-as-primary-science transformation to an unsurpassable apogee, as the field's archonship improvidently[50] for decades banished anyone who objected to selling the clumsiest (§§C and G) faker in astronomical history to academe & the public as "**THE GREATEST ASTRONOMER OF ANTIQUITY**."

Again — we're not supposed to guffaw?

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

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B.Schaefer 2001. JHA *32*:1.

B.Schaefer 2002. Sky&Tel *103.2*:38.

B.Schaefer 2013. JHA *44*:47.

ScAm 1979. Scientific American *240.3*:90. Commissioned by ScAm Ed. D.Flanagan.[52]

Dmitri Shcheglov 2016. Isis *107.4*:687.

M.Shevchenko 1990. JHA *21*:187.

Strabo. *Geography* c.20 AD. Ed: Horace Jones, LCL 1917-1932.

S&G = A.Stückelberger & G.Graßhoff 2006. *Ptolemaios Handbuch Geographie*, U.Bern.

Noel Swerdlow 1989. JHA *20*:29.

Noel Swerdlow 1992. JHA *23*:173.

Noel Swerdlow 2010. At Jones 2010A pp. 151.

Tetrabiblos. Compiled Ptolemy c.160 AD. Ed: Frank Robbins, LCL 1940.

Hugh Thurston 1994P. DIO *4.2* ‡6.

Hugh Thurston 1998A. DIO *8* ‡1.

Hugh Thurston 2002S. Isis *93.1*:58.

Hugh Thurston 2002V. DIO *11.3*:91.

Gerald Toomer 1984, Ed. *Ptolemy's Almagest*, NYC.

Vitruvius. *On Architecture* c.30 BC. Ed: Frank Granger, LCL 1931&1934.

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

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[52] "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 and fn 8] trio cited in the piece he published [Swerdow, Gingerich, & V.Thoren], reflecting the kind of muttered slander (more at fn 35) created and spread behind backs by parties many of whom even today keep believing that if they can just preserve or salvage some sliver of doubt that Ptolemy faked, they are thereby not utterly convicted of the vilest brand of academic misbehavior, in their decades of gang-smearing those who were — the ultimate irony — on the side of truth right along. Which merely adds ethical incomprehension to scientific. And why would a party have ever in the 1st place resorted to slander & shunning & running instead of inviting debate, *IF* (§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 recalls DR's sardonic summary during NatGeogrSoc's 1989/12/11 doomed presser-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 finding a star's manybodies-induced tidal ellipsoid in 3 dimensions, www.dioi.org/gjr.pdf, *G.J.RoyAstrSoc 69*:265-271 (verified *Sky&Tel* 2000 Sept pp.14-16); asymptotic planetary perturbation-amplitude as function of relative distance, *MNRAS 147*:177-186; Great Pyramid orientation star 10i Dra, *Nature 412*:699 (2001/8/16); 1st simple zenith-to-horizon formulae for atmospheric refraction&extinction, *PASP 94*:363; solutions of Greek Earth-sizes' errors, Pharos' height, & stade's length (*Griffith Observer* 2018 Aug); closest pre-1976 Pluto-mass (*AJ 75*:856). (For his further scientific discoveries and historical reconstructions, see fn 109, back cover, & www.dioi.org/cot.htm.) Relevantly, *DIO* is also a veteran hoax-investigator: *NY Times* 1996/5/9 p.1, 1998/11/26 p.1, and 2009/9/8 science page.

Universities' science departments deserve to know the kind of mis-math (fn 13), herd-think (fnn 8&10), data-tampering (§§B-G), & idea-grabs (fn 10, §C8) too often passing for scholarship in prominent but joke-refereed (‡2 fn 3; Rawlins 1991W fn 6) & coverup-prone (fnn 10, 11, & 97) journals in history-of-science, a field rife with smearings (fn 8), shunnings (fnn 5&116; Rawlins 1991W fnn 171&173), threats (fn 109), & rejection of normal *science* (bizarre details: *idem* & §I26; fn 100) if favoring heterodoxy, with research-advances' acceptance *contingent upon whose clique the discoverer belongs to*. (Repellant examples: Rawlins 2017E §G3.) Further, there's mitey evidence that archons teach, value, or even understand (§§G5 & J1 [f], fnn 42&106) exploratory hypotheses' use, *tempered by Occam* (§I25, fn 33, ‡1 §A, ‡2 fn 49), to expand&refine knowledge. The result (p.45 & §§B5&C-G) regarding advances in ancient astronomy, is inevitably more destructive than constructive.

However incomplete, the following cronyology is a start towards top academic institutions' enlightenment re contemporary history-of-science's frailties. (**Even while** *DIO* values the field's finds [e.g., fnn 42&127&§I14, ‡2 §F2&fn 42], from which scientists have learned. Despite wan reciprocity.) **Mathematical scientists' scrupulous verification encouraged**.

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

Continuing the history-of-science cult's staunch tradition of exiling and/or gang-smearing such math-competent, even eminent intruders as van der Waerden, R.Newton, H.Thurston: despite physicist D.Rawlins' half-century of astronomical-history researches (samplings above & p.2), a staid version, www.dioi.org/qjo.doc, of the following please-clean-your-house paper (with amiable cover letter), was inflexibly (fn 100) spurned in 2017 by the **History-of-science** Society's *Isis* (ultimo US hist-sci forum), which refused to evaluate **its History or its Science**, while unable to deny its accuracy, relevance, or multiple demonstrations of *the most prominent historians-of-science ALTERING DATA* (esp. §§C-D&F-G), uncorrected-unretracted math-botches (§B4, fnn 27&97), dreadful science (§C5), even weird science (fn 2). (And see fn 4's conclusion, for the *Journal for the History of Astronomy*'s **DEFINITELY**-original idea of refereeing.) Not to mention shunning of competent heretics' scrupulously refereed research advances (§I), and systematic non-citation of the scientific-history journal *DIO*, though for over 25y it's been easily the most mathematically and astronomically competent journal in the science-history field, vol. 10 even highly-exceptionally co-published (with the University of Cambridge), long supervised by boards composed of that rare minority of scientifically able historians (e.g., astronomer-legends E.M.Standish, emeritus CalTech-JPL, & Chas.Kowal, late of STSI), so seethingly feared by the democratically-ruling majority, whose mathematical and ethical shortfalls *DIO* has

been patching-up for decades without the slightest discernable (positive) effect on the field. E.g., three cornered History of science journals cut contact with *DIO*, when, e.g. (fn 97), asked to print the embarrassing but unquestioned fact that their icon Ptolemy's four Sun "observations" were **FIFTY TIMES** closer to Hipparchos' 280y-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*y (‡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-tolerated to insistently-normative? Simultaneously with *Isis*' resistance to the below history, it was learned (see ‡1 here for links to all papers) that: [1] a 7y-old *DIO* discovery (Rawlins 2008Q fn 6) had been unattributedly published, www.dioi.org/cev.pdf, as *Isis*' 2015 LEAD article (*repair request repulsed*), and [2] *Isis*' pseudo-refereed final 2016 LEAD article had extensively attacked Rawlins 1985G (Greenwich Meridian Centenary paper) on ancient longitude accuracy (refereed *on its mathematical merits* by a panel of prominent *scientists*), calling such accuracy a "delusion" — due to the critic's own amazing delusions (fn 97): [a] Treating a *solar eclipse as lunar*, neither author nor *Isis even yet* realizing it sorta matters. (See hist.sci icon Neugebauer's able 1975 analysis at ‡1 §D; so *Isis*' cascading scientific innocence gauges hist.sci-decline since.) [b] Putting Spain into the wrong hemisphere. Rather than print *DIO*'s temperate Letter-to-the-Editor (‡1), www.dioi.org/islg.doc, *Isis* Ed. H.F.Cohen fled ("I will not read, let alone respond to, any further messages on your side."), ploy **unanimously endorsed by his 30 Adv.Editors**, www.dioi.org/isb.pdf, as *Isis* ducked refereeing the history or science of Letter or paper.

If this is the *top* of history-of-astronomy, one can imagine what's going on underneath. But, then, actually, one need not imagine, since scores of examples of the field's too-ordinary amusing scholarship are cataloged at www.dioi.org/jhb.htm, the oddest being "science" as credible as the Earth's East Pole (*Winnie the Pooh* Chap.9): [a] the 1976 *Dictionary of Scientific Biography 13*:321 discovery of the *Autumn Solstice* and [b] *JHA 22.2*:119's 1991 discovery of the *Winter Equinox*. See §§C-G for data-tampering by top pols, incl. the NYU Institute for the Study of the Ancient World's Director, fitfully brilliant Alex Jones.

Isis' 2017 coverup of its 2016 sham-refereeing disaster (p.8 here: 7 largely-obvious undetected errors, 2 of them crippling) is just the latest example of the level of equity, ability, and openness at history-of-science's most eminent&incestuous forums, which now exist in a state of such evidence-immunity and no-consequences self-rule that they have for a 1/2 century been **tragically & punitively insisting** (awful details: ‡2 §N8) on the very opposite of the manyways-obvious (§I10) truth of *an issue as central as Greek astronomical empiricism*, meanwhile becoming ever-more-incapable of self-righting the field's ship.

After decades of observing science-shy historians-of-science and watching limited mentalities (fnn 8&96) like careerists Noel Swerdlow and Owen Gingerich be elevated to an eminence that empowers their dementedly vicious smears (e.g., fn 34 & photos linked at fn 6) of those merely disagreeing with them, one may wonder whether historians' too-frequent encounters, with scientists appalled at also-too-frequent technical-goofiness by history-of-science archons, have led to a defensive pattern (gameplan?) of curling-up into a self-insulated world? (Classic turfish history-of-science-think quoted, Rawlins 1994S §C4: "We don't want the history of physics to be written by senile physicists.")

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

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

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

Muffia Cult's 84[y] 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:[1] [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 refereeing or such epistemic canons[3] of scientific evaluation as simplicity, minimal-premises, fruitfulness, and predictivity. Auto-rejection has been inspired by durable grantmagnet (Diller 1984 fn 26) orthodoxy that the famed ancient data-faking (§I8), 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'

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[1] New early spherical trigonometry date, 2nd century BC (fnn 16-17&24 below); the same era's 1″-accurate trig tables & 1″-accurate calculations (fn 37); 3rd century BC Greek scientists' use (fn 42) of order-of-magnitude (ordmag) and their adoption of degrees (fn 94). [Superscript glossary: §I32.] JHAD = cartel of *JHA* (*Journal for the History of Astronomy*, O.Gingerich principal editor for 40[y]) & HAD (Historical Astronomy Division, Gingerich long-dominant co-founder) of *the American Astronomical Society, whose Ethics Statement has just demoted* [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 (§I10; Rawlins 2017E) vs Hist.sci reverence for alibiing inaccuracy, to ameliorate Ptolemy's gross fabrications, e.g., ***inverting relation of theory&evidence*** (Ragep **crudely**: fn 9), & focusing on Greek "theoretical structure, erected in spite of the enormous difficulties that beset the attempts to obtain reliable empirical data" prominently quoted by Gingerich 1976 p.477; see fnn 8, 62, & 97 here. Neugebauer 1975 p.931 crowned indoor astrologer (Rawlins 2003X) Ptolemy "the greatest astronomer of antiquity", echoed verbatim by Gingerich 1976 [AAAS!] & Gingerich 2002. Since Ptolemy's *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-odds, perfect-hit solutions, which have the effrontery to contravene current orthodoxy, encourages vulnerability to adopting embarrassingly unlikely alternate theories, and thus (effectively) escaping into the miracle world of the supernatural, palming off — as valid scholarship — notions unworthy of a rational enterprise. For a JHAD-wayouthouse of ultra-outré occultisms, see here at: §§C11, D2&D3, E2, G7-G9, G11, H4, I22; fnn 12, 33, 44&45, 55, 68&69, 89.

[3] *DIO*'s principled approaches to knowledge are brought together below, at §J1 [g]. See, too, fn 10.

hugely contra-reality super-adherence to 280[y]-old indoor tables, is uncriminal since Greek astronomers were theorists not empiricists, who *suppressed — i.e., **destroyed** — data inconsistent with prevailing models* (fnn 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/dioind.htm), which has fitted to attested ancient data scores of new heretical reconstructions (many evaluated below, esp. §I, with selected links), meanwhile asking how ancient astronomers *copying predecessors* could advance to their surprisingly numerous but heretofore remarkably unappreciated high-accuracy Greek measures. (Below, compare §I10 [& ‡1 §D] to craniolithic cult-insistence on Greek inaccuracy: fnn 1, 8 [!], 69, 93, & §B4.) Also investigated: ahistorical myth of "theoretical" Greek non-empiricism & data-selection; Occamite resolutions of such problems as ancient Earth-measure by Pharos flame & double-sunsets; the method explaining all 3 Greek-adopted monthlengths becoming undeniably accurate to 1[s] or better (!); how all 3 hitherto-unsolved lunar speeds were based on classical-era use of 13th century BC Babylonian eclipse data (§I34); pseudo-Aristarchos' daily retrograde Moon & Archimedes' degree-use (both obvious, yet ***unnoticed for 2000***[y]: §§I1&I2 below); Ptolemy's celestial fakes; Hipparchos' elaborate and 1″-accurate calculations, effected by 1″-accurate trig tables; his use of spherical trig; Archimedes-admired pioneer in heliocentricity & ***spatial***-vastness, Aristarchos: P.Tannery's and *DIO*'s quadruply-verified (fn 88) reconstruction of his ***temporally***-vast 4868[y] Great Year, 1[s]-accurate monthlength, and pre-Hipparchos discovery of precession.

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

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

A1   In the contemporary history-of-ancient-astronomy subfield, numerous coherent, mathematically-copper-fastened, expertly refereed, but archon-offending progressive discoveries have appeared for decades, elucidating hitherto-mysterious ancient data. When these offenses cannot be undone frontally, certain careerist serial knowledge-subtractors, — compactly called the 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 unrefereed[4] articles that:

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[4] Wikipedia's article on the virtually unrefereed *Journal for the History of Astronomy* [*JHA*] actually claims the journal is "peer reviewed"! (See fn 109 below, also re Wikipedia's 2008-2014 war upon Rawlins' Wik-biography.) Meanwhile, the best-refereed journal in the field, *DIO*, is repeatedly, aggressively classed by Wikipedia as Unreliable (not deserving an article, with bio-references to Rawlins as "publisher" persistently suppressed), though neither Wikipedia's CSICOP-soldier administrators and associated threatening cult-vandals (repulsive details also at fn 109) nor *JHA* have in years of trying and seething, managed to find incompetent scholarship anywhere in *DIO*'s score of volumes. Meanwhile, by a contrast that would be embarrassing to honest forums, *DIO* has (without even having to try) discerned ***dozens*** of flagrant examples of prominently-published history-of-astronomy catastrophes that nobody refereed with care (or cared to referee): see fnn 50&86 below and above at ‡1's *POSTSCRIPT*, also www.dioi.org/jhb.htm, www.dioi.org/fff.htm#sxrg. Historians-of-science seeking ready buddy-publication (to convince their universities and funders that they're academically contributory) evidently *prefer such laxity vs being refereed scrupulously, since far more vitae get padded by indiscriminateness than by care.* Given the spectacular muff-frequency of the history-of-astronomy cult and its captive *JHA*, plus its distaste for wasting time on alien authors or on the bother of serious refereeing (fnn 97&100), *DIO* from its inception has dubbed this incestuous, ***self-perpetuating*** cartel: "The Muffia". (Considering the history laid out in the current paper, who could resist such apt appellation?) A secret of maintaining eternal muffery is *JHA*'s riggorous refereeing of sufficiently

[A] Noncite the heretical paper primarily targeted. ***Deliberately***: §§C10, D6, G11, fn 10, twice more at fn 121. (This pattern goes back at least to the prominent *Sky&Tel* attack on R.Newton by dutiful Muffiosa Janice Henderson 1976, without citing his name or papers.)

[B] Exhibit daring proclivity for the inherently improbable over the probable (even the most remotely of each: fnn 33&36&45), whenever seeking apparent evidential support for any cult-in-too-deep position threatened by new findings.

[C] Effect sterile destruction by demeaning *or even outright altering* (e.g., §D5 item [C]; §F10 fnn 25&85 [& see fnn 30&11&58]) the data upon which an offending discovery is based, decreeing that whatever version of said data was previously accepted (right up until rebel forces **fudgelessly** found the 1ˢᵗ reasonable potential resolution of them) has only 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 ever attaining cult–Acceptability. In-balance is the possibility that one or two or dozens of valid potential advances might languish indefinitely-unrecognized, wasted.

A2   Below, we analyse, in §§C-G, a flock of recent instances of such data-fudgery-for-orthodoxy, aimed at submerging competently proposed, compelling, but still-little-known heretical historical advances. We also append, in §I below, a score of potentially-heuristic examples of such advances where those, governed by agendas, shunning,[5] & cliques — ever-attended by denigration[6] of outlanders — instead (of resorting to data-alteration) just

archon-osculating 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 *prefereed*: straight-to-press unread (flagrantly, laughably clumsy *Centaurus* example written by *JHA* boardmember: fn 50 below), a practice additionally eased by *JHA* insistence on printer-ready (Rawlins 1991W fn 6) and wordprocessor (www.dioi.org/pm3.htm) submission. (Such conveniences are only exacerbating a trend already underway [as DR warned *JHA*, 38ʸ ago: *DIO 1.2* fn 6&§B4] whereby "editors" become little more than printers. Note *DIO*'s typical insensitivity, www.dioi.org/isb.pdf, in asking an editor to edit.) And it shows. Lucky for us, *JHA* refereeing's judicious deliberateness is self-extolled by proud 1970-2013 Editor M.A.Hoskin, www.dioi.org/pm3.htm, emph added: "*it is quite common for an article received at breakfast to be refereed during the morning . . . and the verdict sent to the author by lunchtime*." For about 60 cases of thus-inevitable *JHA*-published odd and/or miscomputed scholarship, see: www.dioi.org/jhb.htm, samples here at fnn 10-11&86. No like list of botches debuting in *DIO* has been or can be compiled. Maybe due to scrupulous *DIO* refereeing?

At least until afternoon tea.

[5] Shunning R.Newton: Gingerich 1990 p.364. Shunning DR: fnn 109&125 here. Hoskin 1983 blackball: *DIO 1.2* §§B2-B3 (1991). Shunning's reality acknowledged by Schaefer 2002 p.40 while carelessly relaying **nationally** in *Lie&Telescope* yet-unretracted previously-private false slander (original shunning-justification) that DR long pestered Hoskin with abusive letters; *DIO* urged observers to phone the libel's publisher, 617-864-7360, for copies of said persistent abuse *that is in fact pure fantasy*. What institute did? Or asked who'd let even real insult kill dialog for 1/3 century? [Gingerich's condition for 1999/7/3 UND debate: bar DR from the podium. Such establishment un-principledness guarantees: #1 response to this *DIO* will be fresh irrelevant smearing. Same timing at www.dioi.org/stb.htm#ffpd. (Like Assange, suddenly a "rapist" upon publishing forbidden data.) Purely punitive aim. (Rawlins 2000A fn 172: world's richest mud-mine? A controversy's last ditch.)]

[6] O.Gingerich, longtime head of Harvard's History of science Dep't, defames Ptolemy-skeptics, www.dioi.org/pm2.htm, in private communications. E.g., his 2000 referee-report to *Isis* innumerately 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/j43f.pdf, "Naked Came the Arrogance", Rawlins 1994S §§B5-B8; Gingerich, *loc cit*; and cult echo-slander sampled at www.dioi.org/j111.pdf, *DIO 1.1* ‡1 §C7; also see fnn 16&20, as well as *ibid* ‡3 §§D2-D3, vs ‡3's fn 7. Establishment-servant Gingerich's whackamole campaign to contain heresy is detailed at www.dioi.org/j43f.pdf, 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 sham, his *JHA* would hardly have gone decades printing just Gingerich's side of the Ptolemy pseudo-controversy, protecting readers from exposure to *DIO*'s too-dangerous evidence&reasoning,

impunitively ignore mathematically and logically solid but cult-displeasing findings for decades. This transparent behavior continues even despite 2002 national attention to "un-professional" acts characterizing the Ptolemy Controversy (ancient astronomy's hottest).[7] Are debate-averse conservatives justified in unprofessional contempt towards unprofession-ally uppity heresy? That question is explored below, along with the field's domination by dissent-intimidation & shunning, which has only solidified (fn 125) since 2002, perhaps from scientifically-shy historians-of-science hanging shy of scientific critics.

Which suggests several questions that may lurk behind superficial arrogance:

[1] *Are historians of mathematical scientists the only historians who fear their own subjects*, defensively driven to slanderously, if ironically, fake (fn 34, ‡2 fn 35) higher expertise?

[2] Are their depts like French depts staffed by profs who don't speak French very well? *And pretend it doesn't matter*. (It does: §I7; Rawlins 2017E §K2; Rawlins 2018V end.)

[3] Is this why too many historians-of-science cannot (e.g., §B4) admit mistakes (*as scientists routinely do*: R.Newton, B.L.van der Waerden, *DIO 11.2* [on cover!], S.Goldstein, etc) & can be 100% sure teamplayer fellow historians-of-science won't *ever* ask them to?

[4] Does that relate to the inverse: science-historians' endemic reluctance to acknowledge non-club-members' vindications? — a reflex which can reach such extremes (fn 17 & §F5) as to defy any known academic ethical code. (Outside sororities: Rawlins 2008R Epilog.)

B   Hiding Modern Empirical Data: Boomerang Irony & Lawlessness

B1   When today's Ptolemists are determined to justify what they already knew before "investigating" — the unscientific but cult-approved[8] deed of destroying data needn't be ancient. Apologists, for Ptolemy's "observational" ordmag 1° errors, claim it was normal for Greek astronomers' to compute outdoor "data" indoors. Or to fudge alleged observations to agree with positions that were "theoretical" (i.e., computed indoors — so how does fudging differ from fabrication?) and throw away any that didn't agree.

and (especially scary) competence.

[7] Schaefer *loc cit*. But how "hot" is a controversy where virtually all prominent print is on one side? The cowering side.

[8] Historians-of-science remain impenetrably loyal to the idea — which *DIO 1.1* p.10 fn 24 deems *mass-slandering of ancients* — that it was (inevitably?) normal for **crude** *Greek science* (§C14; Rawlins 2018U fn 3) to indoor-fake empirical data. Or (Gingerich 1976 p.477) to fudge such. (Same thing, when the value which a datum is fudged to agree-with is not observed but fabricated.) This Ptolemy-alibi concoction was promoted nationally (*though supported by not the slightest ancient attestation*) through *Scientific American*: inspired by its Editor, mentored by Harvard's Gingerich & MacArthurian Swerdlow, who (fnn 6&34) actually slander **OTHER PEOPLE** as con-men and/or kooks, even *while* hawking faker&astrologer Ptolemy to funders&public as an immortal scientist. (Items at ‡2 fn 8, or www.dioi.org/jhb.htm#dmcq, assist in weighing Swerdlow's qualifications for such judgements & his capacity for determining which scholars to condemn and/or banish. Likewise for Gingerich at Rawlins 1987 n.35 & Rawlins 1996C fn 66.) The article (ScAm 1979) states *as fact* the baseless *speculation* that ancients ignored (destroyed) data discordant with the theories they *inherited*. (Explicit in fn 9.) As *ScAm* was vainly informed before publication, Hipparchos reports observations clashing with theory, even with each other (*Almajest* 3.1, 4.11, 6.9): here, in fn 97. (Oxford University Press was equally vainly warned of problems with the largely useful textbook, Evans 1998, by phone and 1997/7/17 letter.) In spite of massive — though disgracefully-long-unrecognized — evidence of high-accuracy Greek science (overkill-detailed here at §I10, more at Rawlins 2017E), the ubiquity of the modern history-of-astronomy cult's insistence on its eccentric fantasy of ancient astronomy as non-empirical, data-forcing, disorganized, inaccurate, and/or observationally clumsy *prominently persists to this day* (fn 13) and is obvious from the sheer volume of without-exception-fallacious & pseudo-scientific effusions arguing it & its variations. Besides earlier in this note, see: compact string of citations in above Summary, as well as fuller and more specific listing here (& even fuller above at ‡2 §N8); fn 1 (on Neugebauer 1975 & Gingerich 1976); below fn 62 (on same & Evans 1998); §H4 (on Jones 2002E); Rawlins 2018U §§B2-B3 (on HamSwerdlow 1981, Swerdlow 1989, & Van Helden 1985); §B above (on Evans 1987 & Evans 1998); Rawlins 1991W fn 99 (on Swerdlow 1989 & Graßhoff 1990); Rawlins 2002V §§I2-I11 & fn 57 (on Gingerich 2002); below fn 97 (on Shcheglov 2016 Dec, the latest).

B2   In 1987, in order to justify the modern Ptolemist vision of antiquity, *JHA*-Editor-in-progress J.Evans published an unexceptionally polite, technically pathetic, but politically brilliant Step-One towards becoming a Muffia Maid-Man by assassinating R.Newton's credibility — anticipating full well the boost he'd achieve towards his ultimate Editorship by attacking *JHA* Editors' *bête-noire*-Newton, i.e., telling 'em what they wanna hear, regardless of the cost to truth and to the reputation of one of ablest scholars ever to grace the field. (And regardless of whether Evans' paper was valid. All that mattered to *JHA* was the attack's teamwork-contribution to the pretense that Newton was as crazy as its cult's unanimous goosestep-slander was insisting: fn 34. Which is why the parties soon proven right [‡4 §B4] about Ptolemy's Star Catalog theft were exiled, while those who were impenetrably wrong were elevated — the most-impenetrable 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 adducing three instances of grossly erroneous outdoor placement of a star's position, from measurement of its angular elongation from the known-position Moon at mid-eclipse: Evans' own 1981 Seattle observation of the star λ Sgr — the record of which has since disappeared without explanation — and two ancient observations of the star Spica vs the eclipsed Moon (*Almajest* 3.1) by Hipparchos in −145 and −134. The errors were all ordmag 1°: respectively, −40′, −33′, +33′.

B3   After in 1991 *DIO* showed (fn 10 below) these were not observational errors at all, a 2009 Rawlins paper detailed[9] the precise explanation which shows that Evans' three data ultimately support his conclusion's opposite.

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[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, when the defendant has already for at least 1200[y] been known to have simply, clearly, uncomplicatedly, high-schoolishly, arithmetically indoor-computed his four alleged "observations" of the Sun: §I8 below. A textbook case of politics overwhelming reason — as it has, for ordmag a century of history-of-science's ubiquitous, *naked promotion-for-grantprofit of a known scientific criminal*. (See, e.g., ‡2 or Pedersen 1993 p.559's justification of Ptolemy's massive indoor plagiarism of Hipparchos' stellar coordinates, after initially denying it at Pedersen 1974 p.258 by assuring us that Ptolemy had too much "integrity".) Further history-of-science contributions to ethical philosophy are announced from the field's heights by NYU's A.Jones (who knows Ptolemy faked science [‡2 fn 2], but ranks true *history* [vs JHAD status] in canny political order): the Jones-edited 2010 Springer volume *Ptolemy in Perspective* (CalTech [!] 2007 conference, arranged by CalTech's Swerdlow), is prefaced by Swerdlovian prose, "Among the SCIENTIFIC authors of the Greco-Roman world, none gives us such a strong impression of writing for posterity as Ptolemy. . . . no reference to himself except as an OBSERVER, scholar, and theoretician . . . . Nor is there anything meretricious in Ptolemy's efforts to give his SCIENCE a public face. . . he . . . made astronomical OBSERVATIONS [vs below at §I8] between the mid-120s and the early 140s of our era" (caps added). The same Jones-edited collection calls Ptolemy's fakes "observations of the Sun" with mere "errors" (Swerdlow 2010 p.151), adding that Tycho "took the observations . . . of Ptolemy seriously" (*ibid* p.154), though Tycho deemed Ptolemy a thief&fraud and so dumped his fake data, ***epochally discovering accurate precession thereby***: ‡2 §A; Rawlins 1993D fn 141. In this same *CalTech* collection, we're told (Ragep *op cit* p.126, emph added), in a typically (‡2 fn 18) condemnationless history-of-science clique "reply" to the fatal-for-*scientists* revelation that Ptolemy indoor-faked allegedly outdoor data: "But let us look at this another way. Ptolemy decided *not to tamper with* the year[length] he had inherited from Hipparchus" (the very datum used to fake all 4 of his solar data): the historian-of-science here acting as if *a faker's tampering with data is the scientific&ethical* **equivalent** *of real scientists' tampering with theory when improving same*: just two O.K.-options for resolving theory-vs-data conflicts! *Consider the revelation*: this **CALTECH-SPONSORED** expression of inside-out-science is considered the epitome of DEEP non-judgemental thinking, in the history-of-science commune, where no one in authority seems able ***even to tell a real ancient scientist from an occultist fraud***. And see ‡1 §§H&T on the field's robo-brushoff of Ptolemy's connexion to astrology, believing *in reckless defiance of the awful truth* (§I11) that only unenlightened, "paradigm"-insensitive scientists could suppose it reflects negatively on him.

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

> Among the gymnastic hysterical-astronomy pratfalls enlivening *JHA*'s hefty (64pp!) James Evans double-lead-paper attack [Evans 1987], upon (then-minority) Ptolemy-doubters, was Evans' lordly illustration of [skeptics'] dumb overestimation of ancient [observational] accuracy . . . . [Evans *op cit*] n.50 (p.275) presents his own non-telescopic (cross-staff) 1981 July 16 Seattle observational determination of the longitude of a star (λ Sgr) by using a lunar eclipse (as Hipparchos had) [measuring the star's angular distance from the Moon when [it was] 180° from the Sun's already-tabulated position] — which after Evans' reduction produced a longitude erroneous by −2°/3, thus according to him (*idem*) showing that the huge errors in some ancient observations were so ordinary that such were a poor basis for learning anything about ancient science [i.e., condemning Ptolemy's gross errors]. As further examples, Evans specifically mentions (*idem* & p.235) Hipparchos' two hugely disparate Spica data [also eclipse-based] . . . which disagree by over 1°. He then draws for us a [Ptolemist] lesson (emph added): "***No better demonstration could be wished*** of the uncertainty attached to the method" of fixing stars' longitudes by eclipses. However, when instructor Evans ***repeats the very same sermon*** (on Hipparchos' eclipse-star errors) 11[y] later [in manyways-valuable J.Evans, *History and Practice of Ancient Astronomy* (Oxford: Oxford Univ., 1998)] p.259 ("This shows the size of the possible errors in ancient measurements of absolute star longitudes"), he slyly deletes mention of his formerly prominent 1981 eclipse-star measures — which shows that (during the 1987-1998 interim) Evans had read [the 1991 revelation][10] . . . that DR had discovered

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[10] A 1999/4/2 Evans letter boasted of ashcanning *DIO 8*, allegedly not reading it (& its p.2 exposure of his citation-integrity's consistency; also fn 127 below). [Equally honest Peary dodge at Bryce 1997 p.602.] Groundwork for further bibliographical sins? E.g., Carman & Evans 2015, www.dioi.org/cev.pdf, prominently published *as original*, **without attribution**, Rawlins 2008Q fn 6's discovery, www.dioi.org/je01.pdf, of a parallactic explanation of Eratosthenes' Earth circumference 252000 stades, which coverupper *Isis* is refusing to even correspond on much less undo (‡1 here), while Evans hides from his obligation to own up re who 1st discovered the theory Carman&he published as their own. [Recall: J.Bode appropriated "Bode's Law" though it was actually 1st published in a footnote to a non-astronomical work by J.Titius.] Specifically, Carman & Evans 2015's proud Pb-paper's central equation, Eratosthenes' Sun-distance $S_E = 102^r$ [102 Earth-radii] (fn 42 below), was already discovered & published for the 1st time 7[y] earlier at Rawlins 2008Q's waystation eq.9, which *Isis*' leashed authors couldn't see past. (To connect to a new world of Greek scientists' ingenuity & precision which *DIO*'s paper delights in exploring: curious readers will enjoy sharing this journey into the previously unknown, far past where Carman & Evans 2015 stopped.) Incredibly or typically, the authors reveal to readers no hint of: [A] Airbending's effect on geodesy (§I25 below; Rawlins 2008Q). [B] Pseudo-Aristarchos' ["p-A"] factor-of-four giveaway slip (§I2 here). [C] Archimedes' contradiction of p-A (*idem*), saying Aristarchos' Sun-diameter was 1/4 of p-A's (correct half-degree vs ridiculous 2°), which C&E certainly knew about since it appears on the very Archimedes page cited (for other cause) at Carman & Evans 2015 n.1. (Shades of other knowing archonal non-citations noted here at fn 121.) [D] And p-A's lunar 3° parallax's follies (§I2; Rawlins 2008R §C1). [E] Also p-A's ***daily-retrograding Moon*** (*idem*), caused by lunar distance 1/3 of reality. (Which Carman & Evans 2015 p.9 claim "would not have seemed outrageous" for the time!!!) [F] The p-A solar disk's angular area was 14 times reality. [G] Multiplying 40800 stades by 2π yields 256000 stades (fn 42 below), the very *C* found 36[y] ago from Eratosthenes' Strabo-relayed Nile Map. (Carman & Evans 2015 n.10 cites the map but not its centrally deduced 256000 stades.) [H] *Ibid*'s innocence of the ancient context: intelligent Greek astronomers rounded the solar distance (in Earth-radii) to powers of 10 (fn 42).
I.e., classic JHAD immunity to Greek scientific progress & modern historical progress, cult-obediently (fn 8), heroically repelling wave after wave of such. Evans insists (above top line) he won't read *DIO* (with its Rawlins 2008Q §C1 catalog of all five pseudo-Aristarchos farces), and none in this cringing

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 *DIO* scored & "premier" *JHA* bellyflopped . . . . Contra Evans, neither his own nor Hipparchos' problems were observational. Both simply miscomputed . . . valid observational data by using invalid math: ***the wrong sign*** for their parallax corrections . . . . the [1981 Seattle] longitudinal lunar parallax p_λ was virtually $1°/3$. . . . the sign mixup would naturally cause an error of . . . $-40'$, [&] the laughably impossible [vs here in fn 12 & §I10 #1] "observational" longitude he reports is indeed (Evans 1987 *loc cit*): "too small by about $40'$". (Typically, Evans has had no comment since, despite [Rawlins . . . face-to-faces] & Hugh Thurston [by letter] directly bringing the matter to his attention.)[11] 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[12] ascribed to the best ancient . . . observations.

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field has a word to say in criticism. Parallel Evansiana: despite denial of reading *DIO*, Evans' clumsy unannounced sly-try 1998 eclipse switch (unsubtle details: fn 11 below, or ‡2 fn 47) shows he'd read Rawlins 1991W fn 288, *DIO*'s detection-revelation of Evans' 1987 parallax-miscue, ever-uncited by him during 3 decades of hiding from publicly facing this central demonstration of his cult-engendered fallibility. Also ever-Evans-uncited: *DIO 3*, www.dioi.org/j301.pdf, **the standard critical edition of Tycho's 1004-Star Catalog**, Rawlins 1993D (fruit of *DIO*'s 7 years of scrupulous investigation & math-reconstruction: 1987-1994): flagrantly deliberate citation-avoidance in Oxford U.Press' Evans 1998 pp.271-272 & n.28 thereon (p.459), 5^y after *DIO 3* appeared. (Will scholars not following suit offend The Editor's obviously hyper-tender sensibilities?) Is the Tycho catalog shunned because published by a journal correcting an Evans mistake he pretends was never made? (*But doesn't say so.* [Indeed, doesn't say anything.] *And no historian-of-science asks*. A field ruled by fear for decades. But note that B.Schaefer has admirably broken ranks on *JHA*-shunning of *DIO 3*.) More Evanscience appreciated in Rawlins 1992T §§H1-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 aptly.) Intermittent listing at www.dioi.org/jhb.htm#vjcr, of ten further examples of ***deliberately-ignored*** (recall 1999/4/2 letter, above) revelations of undeniably erroneous but never-retracted Evans scholarship. See also *DIO 8* ‡4 fn 4 on the unsubtly-arbitrary & inadvertently non-empirical→empirical argument at Evans 1998 p.72, ultimately adopting (non-citationally) yet another DR original discovery (§I4, *DIO 1.1* ‡7 §C1; Rawlins 1991W §R9, fnn 263&272; Rawlins 2008R fn 17): Aristarchos' 87° half-Moon elongation as not precise but a lower limit. And don't miss www.dioi.org/jhb.htm#cdqm, on Evans twice copying J.Dreyer's prose without quotation-marks. Irony: *DIO*'s Tycho star catalog & the differently-important Hipparchos parallax-sign discovery, were both triggered by Evans' own mis-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* fnn 207&292; to Duke, §C14 above & Rawlins 2012V fn 22; to the whole Muffia at Rawlins 1991W §S3.) Evans' modesty precludes him from citing any of these thank-yous. Compare to *DIO*'s rule of always correcting its errors — for both integrity and refusal to mislead readers: www.dioi.org/err.htm, as well as below at §I14 & fnn 98&110, also *DIO 1.1* ‡4 §A2 (1991) & *DIO 11.2* (2003) front cover & p.30. JHAD's perverse reaction to the contrast neons its priorities for all to see. And eyeroll.

[11] Rawlins 2009E fn 4: "Both inquirers were told by [now *JHA* Editor] Evans that he would look into [*DIO*'s correction to his eclipsed-Moon-vs-star experiment]. But he never communicated what he found. Except by implication" when in 1998 (weird details: *ibid* fn 7) he *1984*ly suppressed [***without notice to readers***] his 1987 paper's 1991-*DIO*-outed observed&bungled 1981 eclipse-star measure, then [emulating infamous con-man Dr.Cook: www.dioi.org/jl03.pdf, §C8] nervily subbed, into the same ***Greek-accuracy-demeaning Muffadvocacy***, a nonmeasured 1977 eclipse instead. *Historians*-of-science know of this conscious *historical* distortion. None objects. None felt it bore on Evans' 2013 apotheosis to *JHA* Editor — the #1 political office in the history-of-astronomy field. Kult über alles. With predictably-degenerate Evans history-of-science surfacing yet again in 2015: fnn 10&42.

[12] Rawlins 2009E fn 5: "E.g., Rawlins 1982G p.263 & n.17 [dissed by Swerdlow, rejected by 1st

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B5   (Do years of not admitting parallax-mismath by now equal a kind of data-alteration?)

B6   The above-quoted 2009 article added: [i] detailed proof of *DIO*'s 1991 contention that the 2 superficially awful-looking Hipparchos lunar-eclipse star-placement measures cited above were accurate to ordmag $1'$ if his parallax-corrections were correctly signed; [ii] further, if Hipparchos' hitherto-unexamined only other eclipse ($-140/1/27$) was used to fix nearby Regulus, undoing the very same parallax-sign-error shrinks the same apparent ordmag $1°$ error (common to all 4 mis-longitudes) down to just a few arcmin ($7'$, mostly rounding error), yet again.

[Note: All data are subject to trivial error from ordmag 10^m uncertainty in that era's ΔT.]

B7   Exact data (Rawlins 2009E fn 22): *JHA*'s acceptance of the unchallenged record leaves -145, -140, -134, & 1987 errors of, respectively, $-33'$, $-35'$, $+33'$, $-40'$. *Idem* shows that removing proposed parallax-sign confusions, as well as accounting for the serious systematic errors of the solar orbit Hipparchos used for his estimation of mid-eclipse time, leaves errors in empirical Moon-vs-star gaps of, respectively, just $-2'$, $+7'$, $+1'$, $+2'$.

B8   The former amounts are obviously less credible than the latter, when set in the context of Hipparchos' other known observations' mean single-datum scatter (fn 12): $0°.1$ for 3 lunar-limb-vs-Sun measures, *Almajest* 5.3&5; $2'$ for 17 solar equinoxes on Rhodos (mean's traceable error $7'$, mostly non-observational: Rawlins 2018U §B4), *Almajest* 3.1; $5'$ for 17 stellar declinations (mean's error $0'\pm1'$: Table 2 below), *Almajest* 7.3.

B9   The ore-refinement findings by *DIO* for the three star-vs-eclipsed-Moon data raised by Evans have shock-vindicated *DIO*'s longtime position that Greek scientists were empirical and accurate — and the Regulus case constitutes classic coherent theory-fruitfulness.

B10   Has Evans produced his 1981 written record, to refute *DIO*'s 1991-2009 stimuli? No, he keeps hiding it (fn 127 below); and colleagues' cooperative averted gaze (fn 11 above) is consistent with the dreary theory that they prefer their own flubs be granted like silence, in return. Mutual consequence-free lawlessness.

C   SphTrig's Debut: A.Diller's #1 Discovery Mobbed by Half-Fits

C1   Expressing them in stades north of the Equator, Strabo preserved a dozen-plus Hipparchan geographical latitudes L corresponding to what ancients called "klimata".[13] narrow

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referee Toomer], Rawlins 1985G *passim*, [Rawlins 1985H.]" Pro-Greek-competency: *DIO 1.1* (1991) ‡1 fn 24. Hipparchan measures of lunar limb-vs-Sun separation (*Almajest* 5.3&5), mean error $0°.1$: Thurston 1998A ⊙11. For Rhodos equinoxes, Hipparchos' scatter (mean single-datum error) was $2'$: fn 70 here. With error $0'\pm1'$ (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 $0°.4$), the above-§B's exact before-correction-vs-after data are found here at §B7 (or at Rawlins 2009E fn 22). Evans' refusal to recognize that *DIO*'s analyses have ordmag-shrunk all 4 of his and Hipparchos' longitude errors (each from $1°$ to $0°.1$ or $1'$: chance odds ridiculous) implies that he suspects scientifically-irrelevant dark magic, behind treasonous dirty-tricks [‡4 fn 48], & unprecedentedly insufferable quadruple-lèse-majesté.

[13] Familiarity with klimata is vital to understanding the disgraceful ordmag $1°$ inaccuracy of Ptolemy's geographical latitudes. For the ***purely astrological Hipparchan*** cause, see, e.g., here at ‡1 §R, or Rawlins 1985G pp.260f. Both sources analyse astrological evidence consistent with the self-evident theory that organized ancient scientists had corresponded for lunar eclipse local-time comparisons (accuracy limited mostly by ruling-fineness of sundials&astrolabes used for timing eclipse-start&end), to find longitudes to a mean accuracy of ordmag a degree (§I11 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-1st-cited prime hallucinator. Shcheglov tries to prove that ancients were so incompetent that eclipse-based longitude-differences, as reported in Kleomedes&Pliny, were "badly overestimated". But see *DIO*'s 2017/3/20&4/1 Letter-to-the-Editor (published here as article ‡1), which dishonest *Isis* refuses to print or even evaluate since it shows that *Isis*' editors & referees didn't notice that Shcheglov achieves his denigrations of ancient competency

Table 1: Diller Sph Trig Proof: Hipparchan Longest-Days in Hours ⇒ Latitudes in Stades

Klm	Longst Day M	L Calctd Sph Trig fn 14 eqn	Round Nearst Degr/12	Conv Stads §C3	Round Nearst 100 St	Strabo	Princ Insttt Neug	NYU *JHA* Jones
Eqtr	12^h	0°	0°	0	0	0	*1500*	
Cin	12^{h}3/4	12°36′23″	12°7/12	8808	8800	8800	*10200*	8800
Mer	13^h	16°35′04″	16°7/12	11608	11600	11600	*12800*	11600
Sye	13^{h}1/2	23°59′43″	24°	16800	16800	16800	*17600*	16800
EgL	14^h	30°33′49″	30°7/12	21408	21400	21400	*21800*	*21300*
Pho	14^{h}1/4	<u>33°31′04″</u>	33°1/2	23450	23400	23400	*23700*	23400
Rho	14^{h}1/2	36°15′25″	36°1/4	25375	25400	25400	*25500*	*25300*
Hell	15^h	41°07′34″	41°1/6	28817	28800	28800	28800	*28700*
Mas	15^{h}1/4	43°16′44″	43°1/4	30275	30300	30300	30300	*30200*
Pon	15^{h}1/2	45°15′40″	45°1/4	31675	31700	31700	*31600*	*31600*
Bor	16^h	48°45′50″	48°3/4	34125	34100	34100	34100	34100
Tan	17^h	54°14′53″	54°1/4	37975	38000	38000	38000	*37900*
SBr	18^h	<u>58°12′31″</u>	58°1/4	40775	40800	40800	40800	*40700*
NBr	19^h	61°04′56″	61°1/12	42758	42800	42800	42800	*42700*

constant-L strip-regions extending east-west around the Earth, sharing the same longest-day M — which is mathematically[14] 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*[15] 2.6. And phenomena are tabulated according to klimata at *Almajest* 2.8 for rising-times, at *Almajest* 2.13 for parallax.) In 1934, the eminent philologist Aubrey Diller made two connected, unexpected, epochal discoveries: [1] all Hipparchos' klimata were computed via spherical trigonometry[16] (which in 1934 scholars doubted was available as early as the 2nd century BC), [2] using an obliquity[17] unattested but the most accurate anciently adopted: 23°2/3.

C2   *Mufftypically seeing 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 ironically hilarious mathematical mis-steps: details in fn 97 below.
From H.Buckle *History of Civilization in England* 1873 (1:318-320): In medieval times "the credulity of men had reached a height which seemed to ensure to the clergy a long and universal dominion. . . . A book . . . sanctioned by [the most eminent] judges" recorded that the Carolingian hero Roland fought the Moors' towering goliath Fenacute to no-decision until he "engaged his adversary in a theological discussion. Here the pagan was easily defeated" and, thus confounded, was quickly slain by the sword. When, despite being repeatedly informed of contrary evidence, our era's equally eminent whistleblower-resenting **SCIENCE** journals (‡2 fn 11) dissentlessly trust for decades Mennonite Jesus-hugger Gingerich's insistence that his fellow occultist&courtAlmaJester was The Greatest Astronomer, of an antiquity on whose ingenious empiricism (§I) Gingerich remains invincibly clueless, can we regard contemporary academe's forums as any less deliberately mythmaking-for-the-cause than those of accurate history's prior Dark Ages?

[14] $L = \arctan[-\cos(7.5M)/\tan\epsilon]$ (L in degrees; M in hours); *Almajest* 2.3, Neugebauer 1975 p.38; further sample klimata tables, *ibid*, pp.706-736.

[15] There are 2 scrupulous modern translations of the invaluable *Almajest*, aka *Mathematike Syntaxis*. German: K.Manitius, *Handbuch der Astronomie* (Leipzig: Teubner, 1912-1913); English: G.Toomer, *Almagest* (London: Duckworth, 1984; NYC: Springer, 1984; Princeton: Princeton Univ., 1998).

[16] Diller 1934.

[17] Diller fits' perfection easily verifiable here at Table 1, or at Table 2 of www.dioi.org/jg03.pdf, Rawlins 2009S. *No JHADist agrees*. Yet the truth is plain to all but those controlling discourse, lockmawed even after 5/6 **of a century** of invincible non-innocence.

seriously",[18] proposing his own typically Babylonian-in-the-woodpile arithmetical solution fitting only about half the data, claiming commonality of Hipparchos' klimata with primitive arithmetical schemes, an *idée fixe* also mis-applied by him to Pliny's circuli (fn 90).
[Reliable test, by which one may discern a scholar confident in his creativity & ability: reacting to aliens' successes not with jealousy and-or destructiveness, but with genuine collegial appreciation. How many JHADsters have? Not zero, but too close to it.]

C3   Rawlins tabularly[19] and satirically noted obedient shunning of Diller's theory by every one of the Neugebauer-mob's altarboys[20] (for 5/6 of a century now), though it fit roughly twice as many data as Neugebauer's, while *DIO* introduced[21] into Diller's analysis the following irresistible improvements (Table 1 here): [a] Ancients' standard 5′ rounding of L and — after conversion to distance north of the Equator, at 700 stades/degree (Strabo 2.5.7) — applying customary 100-stade rounding of said distance, accounting for which converted all Diller's near-hits to on-the-nose[22] hits. [b] Finding several further Hipparchos-Strabo klimata unknown to Diller, which turned out to fit his proposal (not Neugebauer's) in every case: **SEVEN-fold fruitfulness** (Diller 1984 §D3), showing[23] that while Neugebauer's theory is indefensible, Diller's fit Strabo's data perfectly[24] for each&every klima: 14-for-14.

C4   See step-by-step calculations, left→right, in Table 1, where Diller-*DIO*'s values (col.6) match every attested klima (col.7). (Neugebauer's [col.8] or Jones' [col.9] don't. Both scholars' thesis-killing misfits [italicized in Table 1] are a *MAJORITY*.)

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

C5   Enter soon-after-*JHA*-boardmember&NYU-tenured A.Jones' prominent 2002 *Journal for the History of Astronomy* [*JHA*] brief,[25] in which Strabo's data — previously agreed-upon by Diller, Neugebauer, and Rawlins — were abruptly decreed, on the authority of Jones himself, to be henceforth considered unreliable, requiring re-do according to his judicious perception of the situation, as he rejects all three men in favor of his own new theory. All klimata data at issue were obviously from a single Strabo table. But, exceptionally well-read in the literature, Jones compared sources, noticing that 2 or 3 Strabo klimata seemed c.100-stades-discrepant vs corresponding (*non-klimata*) data in other works

[18] Contra Hipparchos' access to sph trig, Neugebauer 1975 p.734 n.14 decrees Diller's solution an "absurdity". Woodpile & arithmetical-Babylonian: *ibid* pp.305-306&334. Commonality: *ibid* p.306.

[19] Rawlins 1994M Table 1, augmented in later printings, as previously unnoticed Strabo klimata surfaced, each unfailingly consistent with Diller's theory.

[20] Most Neugebauerians received the 1st Diller-Rawlins klimata table (Rawlins 1994M p.56). All ducked, with an unwillingness to acknowledge Diller's vindication (or even a minim of merit) that was unabashedly, unblemishedly unanimous, even Britton: *DIO 16* p.2 (2009). (See also Dicks 1960 pp.192-194, written ere Dicks escaped the Neugebauer cult.) Neugebauer 1975 p.334's half-fitted 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 insight 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) when his shunned discovery was "rescued 45 years later [*most of his life having passed*] by a phone call from a stranger [Rawlins] in San Diego." See Boltzmann parallel at fn 108.

[21] Rawlins 1994M fn 10 and Table 1.

[22] Table 1 col.3 = Diller 1934 theory's unrounded L, nonfits (ere col.4's 5′ rounding) there underlined.

[23] Rawlins 2009S, www.dioi.org/jg03.pdf, Tables 1&2.

[24] Diller 1984 Table 0. The 14 klimata: Equator, Cinnamon Country, Meroë, Syene, Lower Egypt, Phoenicia, Rhodos, Hellespont, Massalia, Pontos, Borysthenes, Tanais, South Britain, North Britain.

[25] Jones 2002E. Scattered: p.17 n.9. Jones' cited Strabo 2.5.38 Alexandria gnomon ratio 5:3 is just a common textual *alteration*: the original Greek is 5:7, which E.Honigmann&Neugebauer realized (uncited by Neugebauer-protégé & eulogist Swerdlow 2010 p.151) wasn't a gnomon shadow-ratio at all, but the Alexandria klima's shortest:longest-day ratio, m:M (Neugebauer 1975 p.336); ignored (favoring Neugebauer 1975 p.101 n.1) by Jones *op cit* n.3, but the H&N idea's fruitfulness is independently confirmed via Carthage *GD* latitude (Rawlins 2009S fn 35): where same m:M mixup with shadow-ratio (in same Strabo 2.5.38) caused 1000^y of north-Africa latitudinal map-distortion. (Rawlins *op cit* §F4; similarly durable longitude-disaster for Arbela eclipse: Neugebauer 1975 p.938.)

of Hipparchos. Jones does not merely pollute Strabo's klimata with these alien data from scattered other works of Hipparchos, but uses them as an excuse **to shift the entire dozen**, suggesting Strabo's data could have been anciently corrupted by addition of a constant, $A = 100$ stades (meanwhile Jones consistently miscomputes Syracuse' 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 entire. Obvious point against the significance of Jones' disparate data-injections: he well knows[26] that Hipparchos changed adopted parameters over time.

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

C7   Jones also-traditionally alters[28] the *Almajest*'s text for Hipparchos' Marseilles latitude, from $L = 43°04'$ (consistent with $43°1/12$ of the Ptolemy *Geographical Directory* [*GD*]) to $L = 43°01'$, in order to reconstruct (using Eratosthenes' ϵ) 2nd century BC Hipparchos' Jones-speculated indoor calculation-invention (for unstated reasons, and counter-chronologically) of Pytheas' well-known longago (c.-300) solstitial noon gnomon ratio, $\frac{41\ 4/5}{120}$ (whose precision argues it was an outdoor[29] datum); and, to force the speculation's success, Jones begs tolerance of an odd-but-convenient Hipparchan miscalculation,[30] yet another *ad hoke* wrenching of ancients' data. [While rejecting Table 1's normal roundings!]

C8   Jones' promotion[31] of such jigsaw juggling seeks at least a half-share of Diller's discovery. Worse: by fantastically alleging that Diller used invalid data, Jones (Neugebaue-rianly: §C2) lodges his half-fitted theory — unvetted (§C10) and untabulatedly-whimsical — as SUPERIOR to Diller's ultimately-perfectly-fitting one. Though willing to reference Diller's 1934 paper (with 2 nonfits of 11 listed klimata, until *DIO*'s 1994&2009 upgrades: §C3 [a] & fn 34 end), shunsoldier Jones dutifully, consistently refuses to acknowledge the bare existence of Diller's 1994 vindication: *DIO*'s initial near-perfect 12-hits-out-of-13 table[32] (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* ‡3 update distributed in 2009, where the ultimate blemishless-fit perfection (Table 1 here) of Diller's 14-for-14 victory is too irrefutable to deal with. Except by fleeing.

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[26] Jones' 2000 *Encyclopedia of Astronomy&Astrophysics* Hipparchos article, or Tihon 2010.

[27] Jones *loc cit*. Syracuse miscomputed: Jones 2002E n.10. He also controversially if traditionally altered a key Alexandrian datum: fn 25 here. His highly Creative obliquity-speculation: *op cit* p.16. His preference for equinoxes over solstices for L: Jones *loc cit* (spotted by Thurston); and below fn 96 vs Britton 1992 p.29. Non-Hipparchan Alexandria L: Rawlins 2009S fn 30. Obliquity ϵ found concurrently with L via solstices: *ibid* §§F2-F3 & eq.8, and *Almajest* 1.12.

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

[29] *Ibid* Summary [1a] and Rawlins 2009S §F4.

[30] Jones *loc cit* requires Hipparchos rounded 41.713 (41;42,47) to 41 4/5 (41;48), though 41 2/3 (41;40) is rounder & almost twice as nearby. F.Ragep 2010 p.128's Jones-promo sees no-problem here.

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

[32] Initial Diller-Rawlins table of Strabo data compared to Neugebauer's: Rawlins 1994M p.56 [aptly augmented in later renderings, as noted at fn 19], *JHA*-uncited for 24^y now, but noted by Thurston 2002S p.67, and by van Brummelen 2009 p.65. Final 13-for-13 version of Diller-*DIO* klimata table: www.dioi.org/vols/w50.pdf, Diller 1984 Table 0. The table's subsequent 14-for-14 expansion: here in Table 1 or www.dioi.org/jg03.pdf, Rawlins 2009S Table 2.

C9   JHAD'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   *JHAD-unrealized*: Hipparchos-Strabo's data have unexpectedly **SPECTACULAR** sensitivity to even the most minuscule[33] 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] crutch-resort to a constant-shift) accords with all 14 data. (Superior even to the mathematically best-fit solution found by least-squares, which fails at klima 14^{h}1/4.) Jones' 2 premises, [i] Hipparchos' ϵ was Eratosthenes', and/or [ii] Strabo's data need alteration by $A = 100$ stades, overkill-wreck any chance of Jones ever fitting his klimata data. A difficulty he was innocent of, only because he didn't know how[34] to run the required least-squares (a disability seemingly near-universal among historians-of-science: fnn 34&100) — much less the multivariate version. We have attempted enlightenment by devising a simplified method of estimating bivariate-probability loci (fn 33 here), comparing the klimata analyses of Neugebauer & *DIO*: www.dioi.org/sta.htm#xmxw. Even accepting one of Jones' two premises & using least-squares to adjust the other unknown, several non-fitting klimata ensue, as emphasized to him by phone immediately upon his 2002 publication. The reality is rendered particularly obvious by his failure — unique to this controversy — even to supply a table of data. (Diller, Neugebauer, & Rawlins all tabulate.) This, because neither his ϵ nor any other can fit his own fiddled version of the data. *JHA*'s intensive breakfast→lunch refereeing (fn 4) inexplicably never noticed the omission of a Jones table, or his non-citation of the undeniably better-fitting 1994 Diller-*DIO* table well known to him via *Isis* (Thurston 2002S) & direct communications (www.dioi.org/biv.htm#jphn). Question: why persist for 16^y to nonretract & actually promote (Duke same at §D5) a pet theory so inferior (fn 33) *Jones knows he can't even tabulate it without revealing its worthlessness*? Given his long evasion of this obligation, we've tabulated his theory for him, in Table 1, where all non-fitting latitudes (Princetitute or *JHA*) are *italicized*. The crusher (fn 33): *Neugebauer 50%-misses via 4 unknowns, Jones via 2*; *but Diller-DR 100%-hits with just one*. No legit debate here — Just jokers hiding, Princetitute&NYU self-disgracing, **revealingly sure** academe won't act.

C11   Since 2009 April (when Diller's last apparent non-fit unexpectedly was found to accord), Jones has kept contending over his shoulder that the data are to blame for

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[33] Sensitivity: caption to Rawlins 2009S Fig.1, graphing Strabo's 14 klimata (Equator & 12^{h}3/4-19^h). *DIO* method of estimating multivariate-probability loci: www.dioi.org/biv.htm#xmxw, comparing Neugebauer vs Diller-*DIO*, shows that above Table 1's col.5 "Conv/Stads/§C3" (for Diller's $\epsilon = 23°40'$, $A = 0$, with L rounded to nearest $5'$) counter-intuitively fits the data (col.7: "Strabo") over 25% better than ϵ&A produced by least-squares (where also klima 14^{h}1/4 fails). Neugebauer 1975 uses 4 unknowns (Rawlins 2009S fn 7); Jones, 2 (*ibid* §J3). Do shunners note **Diller needs only one** (ϵ)? — to produce column 6 ("Round/Nearst/100 St"), the Diller-*DIO* theory's predictions which anyone (but clenchjawed Muffiosi) can see exactly match column 7 ("Strabo"), the attested Strabo-Hipparchos data, via 2 tiny, normality-predetermined (§C3 above) roundings. As these 2 standard nudgelets inject the finishing-touches, converting Table 1's near-fit col.3 into perfect-fit cols.4→6, who but a cult-priest could see ultimate theoretical col.6's exact matching to attested col.7 as but a demonic illusion by the forces of darkness, requiring heroic interventive exorcism by the farces of dorkness? Specifics useful to the competent follow, for all 14 klimata. Derived from $\epsilon = 23°2/3$ & $A = 0$, Diller's numbers (col.3) fit Strabo's data (col.7), with squares-sum $S = 88'^2.9$ [or 88.9 arcmin-squared]. Applying bivariate least-squares finds $\epsilon = 23°39'\pm2'$ & $A = -10\pm25$ stades, & improves to $S = 82'^2.9$. But for nudged col.4 (Diller-DR) vs col.7, $S = 62'^2.2$. Fitting col.6 to col.7, $S = 0'^2$. With 13 klimata, for Jones' $\epsilon = 23°51'20''$ & $A = 100$ stades (col.9), $S = 540'^2$, less than Neugebauer's (col.8) S. (Unrounded stades & S for both men: www.dioi.org/biv.htm#dzqs.) But Jones only fits 5 of 13 data vs Neugebauer's 6, so cultish-cling to nonadmission of Diller's triumph takes *JHA* from bad to worse.

[34] Similar non-use of least-squares by ubiquitous MacArthur-Genius Swerdlow — who, in terror that academe will read revelations (www.dioi.org/jhb.htm) of his bumblings — smears competent scientists as cranks: *DIO 1.1* ‡3 §§D2-D3; like *ibid* ‡1 §C7. See Rawlins 1992V §§C11-C14 on curve-fitting (vs Evans' notably accurate comparable curve [regardless of subsequent misuse]). For impossibility of Jones fitting the fiddled: Rawlins 2009S §I3. Elimination of last Diller-Rawlins non-fit: *ibid* eq.3.

his theory's doubtless-illusory woes: *they* are the unreliable party, having committed the offense of disagreeing with the theory of the most authoritative expert (here, in fn 86), adding that Hipparchos' trigonometry tables are suspect of a parallel disloyalty. Meanwhile, DR contends that the glad & enlightening opposite is recommended — both for Strabo's klimata data[35] and for Hipparchos' trigonometry tables — by Table 1's 14-for-14 fit, cited above (fn 24). Jones is doing a convincing imitation of one who imagines those astronomical-odds-defying 14 perfect hits merely[36] constitute a paranormal or religious miracle, with no significance or status in his people's idea of the real world of science, where Occult's Razor slices an illusion like a 14-hits-out-of-14 table completely out of that special bubble, consigning it to the Orwellian-Goebbelsian flames it deserves.

C12   Note: it's been obvious since [the Rawlins 1994M investigation] that 1″ accuracy is crucial[37] for the 18^h 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[38] (1991) trigonometric *DIO* reconstruction of Hipparchan lunar orbits, discussed below, in §D and fnn 38&39. *NB*: We now have consistent confirmatory *double*-evidence for Hipparchan 1″-accuracy: [a] Table 1's 18^h klima, as just noted (fn 37). [b] Below fn 46's neat hits for attested $e\&r$.

C13   Ironically, D.Duke's rejection (fn 69 below) of that reconstruction unintentionally highlights the Hipparchan trigonometry tables' accuracy. It just seemed incredible to Duke that *DIO*'s analyses (précised at §D here) of Hipparchos' *Almajest* 4.11 eclipse-trios could possibly be reliable, from sensitivity to tiny uncertainties. Yet we have multiple-verification of those analyses' validity, because Hipparchos' calculations (fnn 24&52 here) & trigonometry tables were more accurate than previously believed by Duke or anyone else (including DR, before 1991 testing). So his&others' now-punctured doubts thankfully just emphasize the shocking newness of the discovery of Hipparchos-era 1″-accuracy computation & trigonometry tables: [1] the two eclipse trios (customarily called "A"&"B") confirmed each other by both producing Hipparchan orbits based on Kallippic motion (period $365^d 1/4$) [2] new papyrus testimony vindicated in 2005 (§F4) both of the novel inductions of *DIO*'s 1991 analyses: [A] Hipparchos' search for a −157 Summer Solstice, [B] his contemporary passing adoption of Kallippic solar motion.

C14   None of these confirmations could have succeeded unless Hipparchos' trig tables were indeed (as already indicated: fn 37 below) accurate to 1″, as later were Ptolemy's (*Almajest* 1.11). Specifically, if *DIO*'s 1991 elicitation of lunar eccentricity e from Trio A had computed with a key trig function off by 1″ (from slightly unreliable trig tables), Hipparchos would have found other[39] than $e = 327'2/3$, the correct value, which is attested (§D1 below) & is found to agree (§D3) with calculation via 1″-accurate trigonometry tables. To repeat (§C12): [i] the Diller-klimata table's 18^h entry (here, in Table 1 & fn 37) and [ii] *DIO*'s eclipse-pair-based orbit reconstructive matches, both consistently establish the 2nd century BC as the earliest date we know 1″-accurate trigonometry tables existed. Concluding this section's revelations, of sph trig & 1″-accurate trig tables & calculations, *3 centuries before Ptolemy*: we recall the mentality that long ago locked-in establishment-wisdom here, Gingerich 1976 p.477 in *Science* (!), blaming his hero Ptolemy's huge errors on (caps added) "CLUMSY mathematics invented only a generation earlier" (‡2 §M1 [b]).

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

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

[37] At the 18^h klima in above Table 1, $58° 12'31''$ rounds to matching $58° 1/4$; but $58° 12'29''$ wouldn't.

[38] Rawlins 1991W §N11[a] eqs.12-20.

[39] *Ibid* §N14. Further deep thanks (not necessarily facetious) to our loyal royal cavilliers: here at fnn 10, 51, 71, 73, & 98; also ‡2 fn 42, and www.dioi.org/jb12.pdf, "Gratitude to Opposites" p.10.

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

D1   As recounted in *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[40] to recover these numbers, wrongly assuming (like *Almajest* 4.6&11 and modernly D.Duke) that Hipparchos had used Ptolemy's sophisticated mathematical procedure (*idem*). Toomer combined this attractive and seemingly plausible theory with a more speculative one: proposing Hipparchos' use of a 3438-based trigonometry table (as used later in India), presumably[41] figuring that the crude proximity of 3438 to Hipparchos' R values (above) was meaningful and that the R were not fixed at the outset but occurred during the math development and were never normalized, hitherto-unheard-of procedure. But ultimately Toomer couldn't match any of Hipparchos' 4 numbers.

D2   *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[42] solar distance of 1000^r, as follows:

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[40] Toomer 1973; Duke 2008W p.286 also assumes Hipparchos used Ptolemy's deft trio method.

[41] Rawlins 2012V §I4. Vs conservative assumption of outset-fixed orbit radius R: Thurston 2002S p.60. For Trios A&B, Toomer's & *DIO*'s fits are compared for all 4 parameters at Rawlins 1991W §P2.

[42] Ancient ordmag-rounded Sun-distance estimates: Rawlins 2008R §§D-F. Sun-distance as historical origin of order-of-magnitude: Rawlins 2012V §D. Reconstructed distances: [a] $S_E = 100^r$ Eratosthenes (Rawlins 2008Q eqs.6-13; nearly same at Carman & Evans 2015); [b] $S_H = 1000^r$ Hipparchos (§D2 here; Rawlins 1991W eqs.22-24; Rawlins 2008R eq.12); [c] $S_A = 10000^r$ Aristarchos & Archimedes (*ibid* eq.15, which §E shows was *empirically* verified as a lower limit in antiquity). Obviously-heliocentrist Poseidonios also proposed at least 10000^r: *ibid* §F2, *probably on solid observational grounds*: *ibid* §E4. **Wise Greek realization that the Sun had such tiny parallax that its distance could be but crudely estimated** was obviously reflected in ancient scientists' repeatedly ([a]-[c]) setting the ratio of solar distance to Earth-radius at a power of 10. [Interlude for exceptions: [1] Hipparchos tried a variety of solar distances, at some point halving his 1000^r solar distance to 500^r, thus solar parallax 7′, later the inverted basis of $3438'/7' = 491 \doteq c.490^r$, Swerdlow's valuable & original discovery, *ibid* fn 39. [2] Arab astronomer Al-Battani foolishly used 1146^r [$180\cdot60/3\pi$], plainly overexactly computed [*idem*] via round 3′ parallax. [3] *Almajest* 5.15 has non-empirical 1210^r.] It is a reflection of the state of current history-of-astronomy and of history-of-science that the History of science Society's *Isis* (toppe history-of-science journal) so failed to understand such a simple and fundamental aspect of Greek empiricism that it could publish as its Pb paper for 2015 an analysis co-authored by Evans (fn 10 above), Editor of the *JHA* ("premier" history-of-astronomy journal according to Schaefer 2002 p.40), deliberately-narrow-focus-arguing that Eratosthenes had a solar distance of 102^r (fn 106) — a transparently overexact value — rather than 100^r, as realized here at Rawlins 2008Q (eqs.9-12) through common sense, antiquity-sense, & consistency with Eratosthenes' long-available actual (Rawlins 1982N) Earth-circumference, 256000 stades. What does it say about the exploratory open-mindedness of history-of-astronomy's current #1 archon that, when he figured Eratosthenian solar distance = $2\pi\cdot4080000$ stades/252000 stades = 102^r, he allegedly never thought to explore-test by dividing (into same numerator) the 2 alternate C-candidates: Kleomedes' famous 250000 stades (yielding solar distance 103^r: Rawlins 2008Q eq.8), or DR's provably-known-to-Evans-but-uncitable 256000 stades, which yielded 100^r *within 1/10*th *of 1%* (*ibid* eq.11). To obscure his 1987 mismath (§B above), Evans has also *for 27^y now* refused to withdraw his claim that he [& Hipparchos *twice* — **MOST**-atypically (§B8)] mis-saw the Moon outdoors by *more than its own diameter*, & will keep on (fn 11) ducking (like *Isis*: fnn 13&100), trusting that academe lacks the integrity to reveal

$R = 1000^r \cot 87° = 52^r 24'$, or, in 60^{ths}, $3144'$ — thus matching[43] Trio A's R (above). Now, a common slip (ancient & modern) is confusion of unit-fraction (inverse integer) & arcmin, since each is signified by a prime-marker; so if we test the hypothesis that a Hipparchan-school computer later misread $52^r 24'$ as $52^r 1/24$, we find, in 60^{ths}, $3122' 1/2$ — exactly matching[44] Trio B's R (above). Unable to counter the math, centrist pols (faces eternally, irrevocably invested into shun: fnn 125&127) have, during the decades since 1991, had no reaction to this *minimal-premises* double-match of both $3144'$ & $3122' 1/2$, besides implicitly contending[45] (by pushing incompatible fudge) that this is all PURELY COINCIDENTAL. Any wonder the above Summary was forced to confront the supernatural? — see fn 2 here.

D3   *DIO* also found double-consistency with its theory that Hipparchos' calculations [1] had used eclipse-pairs[46] not trios (the trios-approach has never yielded unmanipulated data-matches) & [2] had sought only one unknown, eccentricity e (or epicycle-radius r), not apogee-at-epoch A_o or mean-longitude-at-epoch ϵ_o. Thusly computing $e\&r$ produced $327' 39''$ & $247' 30''$, respectively, each a neat match to the above (§D1) corresponding attested Hipparchan data: 327 2/3 & 247 1/2. (Finding no mathematical error, Duke regards these matches, too, as just ANOTHER spooky double-accident.) H.Thurston & John Britton carefully verified all the 1991 paper's supporting math & recommended publication. In 2005 soon-after-*JHA*-boardmember D.Duke defied those recommendations (despite Toomer's honest acknowledgement[47] of his 1973 speculation's dubiousness), resurrecting-recycling it by altering — **explicitly** altering — Toomer's numbers, to ensure its success by inventing teleologically convenient calculations.   DR, recently expanding[48] his 1991 orbit-reclamations, compares *DIO*-vs-Duke simplicities:

that History-of-astronomy's present Ultimo Archon mis-signed his 1981 parallax-correction (§B4), but won't admit so *since silence helps keep alive the JHAD sacred cow of Greek inaccuracy*. So far, his gauge of academe's honesty has proved perceptive. Ability-consistency of *JHA* Editor Evans' record here (faithfully carrying-on the exemplary tradition of 1970-2013 Editor Hoskin's grasp of mathematical astronomy: www.dioi.org/fff.htm#ffcy): [i] 1981-1987 failure to sign parallax correctly, and [ii] 2015 failure (on solar-distance) to understand that if Greek observing accuracy was on the order of 1° as Gingerich insists (Rawlins 2018U fn 3), and *as Evans echoes* (item [i] or §B4 above), then since Eratosthenes' parallax is given as ordmag 1° ($0°.56$ at Carman & Evans 2015 p.14), solar parallax's uncertainty *was ordmag 100% of its size* [like ±1 §E]. (Rawlins 2008R §C5 argues that Aristarchos suspected an even higher ratio.) So only non-scientist pols like Ptolemy or JHADists wouldn't realize that the *JHA-Isis* 102^r distance's ordmag-1% (!) precision is way-larfably too exact — *when uncertainty is 2 ordmags looser*. All this reveals an even looser grasp of *Greek astronomy* (& elementary mathematical astronomy) by two of the world's most deliberately-eminent history-of-science journals. Among famous classical Greek astronomers, only faker Ptolemy insisted on solar-distance numbers of such naïve wayoverprecision, e.g., *AlmaJest* 5.14-16, which Hartner 1980 p.26 justly deemed a "fairy-tale" (like R.Newton 1977 p.198); yet another case where seeing Ptolemy as typical of his era, or even its dominant Greatest, has warped history-of-science's perception of actual science in antiquity.

[43] Rawlins 1991W eq.23.

[44] Rawlins 1991W eq.24. Confusion of arcmin & unit fractions: *ibid* fn 251; Neugebauer 1975 p.166 n.3, & p.729 n.15; Thurston 2002S p.60. Only *DIO*'s theory (or an incantation?) explains the odd but Muffia-uncited circumstance (Rawlins 2012V §I5) that *Almajest* 4.11's two R *differ by less than 1%*. For Toomer and Duke, that must be yet-**another** longshot-coincidence.

[45] Pure chance: Duke 2005T, http://onlinelibrary.wiley.com/doi/10.1111/j.1600-0498.2005.470204, and see fn 68 below.

[46] Pairs: Rawlins *op cit* §N7f. This should be obvious since [a] *Almajest* presents the data in pairs, after all!, & [b] pairs-analysis yields matches to the trios' attested elements, while other approaches don't. Inducing that A_o and ϵ_o were pre-assumed, not sought: Rawlins 2012V §J. Pair-calculations' matches of $e\&r$ to Hipparchos' attested values: *ibid* §§F2&G2, & Rawlins 1991W §N14, & Thurston, *loc cit*. (Doubling double-occultism: implicit in Duke 2005T.) Summaries in Thurston *op cit* pp.60&66-67.

[47] Toomer 1984 (fn 15 above) p.215 n.75; Dicks 1994 fn 42. Recycling: Duke *op cit*.

[48] Augmenting 1991 analysis: Rawlins 2012V §§C-G. *DIO*-vs-Duke contrasts: *ibid* §K2 (or here at §D4). Reconstructions: Rawlins 1991W §§N7-N15, & above in fnn 41, 43-44, &46. Round-element cornucopia: *ibid* eqs.8&9, and §§K9-K10, M4, N10.

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

D5   By contrast, Duke: [A] like Toomer, calculates R first for Trio A, then reverses course[49] to satisfy Trio B, which doesn't work, anyway, unless an extra variable d (hitherto not in evidence, in Ptolemy, India, Toomer, *DIO*, or elsewhere) is arbitrarily brought in to rescue the situation; [B] finds no round elements; [C] alters extremely precise numbers like $51°30'23''$ and $8°48'28''$ to instead become extremely precisely $51°19'37''$ and $8°44'08''$, respectively, though there's no resemblance of ere&aft, or any independent justification of fudgeries so shamelessly explicit, besides riggorous issuance of The Right Answer. All to smother *DIO*'s natural-flow-multifit coherent solution under a pillow fluffy with special assumptions&tampering, resurrecting the spirit of co-subtractor Jones, above (in §C) & below (fn 85). Like Jones (§C10), Duke has refused DR's request to withdraw the paper.

D6   The non-manipulated Rawlins 1991W fourfold-fit reconstruction (above, in §§D2-D3) is never cited throughout Duke's prank (did hypothetical referees even know of it?), **though his paper originated as a challenge to that very reconstruction**, vying for a *DIO* prize, www.dioi.org/pri.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[51] to "correct" any of Hipparchos' calculations in order to draw coherent results from his data, so a historically new conclusion[52] emerged:

Hipparchos' purely mechanical computations are dependably flawless.

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

E1   *DIO* shows (fn 56 below) Hipparchos' -157 Early solar orbit "EH" was succeeded by his -145 prime-years' orbit "PH", in turn replaced by his -134 Ultimate orbit "UH". He adopted EH&PH when computing lunar elements from long-earlier lunar-eclipse Trios A&B (§§C13&E2, fn 55). In 1977, physicist Robt.Newton detected[53] a hitherto-unsuspected 1° error in Trio A's 3rd eclipse, warning that data-restoration here is demanded (fn 91 below) or "incredible" consequences will flow from analysis. The orbital elements derived by warning-rejectors Jones and Duke inadvertently double-confirm[54] Newton's "incredible" prediction. Like Jones, Duke does not notice Newton's 1° warning, & deliberately (§D6) acts as if the carefully-refereed (§D3) *DIO* paper that did heed it does not exist, though it was unsuccessfully challenged (§D6) by Duke to arbiter Thurston & (fn 32 above) précised in *Isis* in 2002. Duke's paper independently computes[55] best-fit $e\&A$ of the Trio B-accordant EH orbit, & of the inevitably weird unrestored-Trio-A-accordant orbit, as if original, though

[49] Jones 1991H likewise treats Trios A&B quite inconsistently: Rawlins 1991W fn 209. Ptolemy rigged numbers to "verify" parameters: Thurston 1998A pp.3, 13, eq.1, perhaps (§D5) inspiring the approach found in §D4 item [C], among other ploys hereabouts.

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

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

[52] *Ibid* §A3.

[53] R.Newton 1977 p.119; consenting to warning: Rawlins 1991W fn 206.

[54] *Ibid* §M3 discusses the problems which Jones 1991H — Duke 2005T & Duke 2008W later following — encounters from ignoring Newton's 1° perception.

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

all 4 solutions had appeared earlier in the very *DIO* paper he is non-citing while trying to exile & replace it.

E2   Nonciting Newton's and *DIO*'s analyses protects readers from learning that, after application of Newton's unexpectedly productive-predictive 1° correction, Trio A is satisfied by unzany elements, which turned out — as discovered 14^y later — to be half from the EH Sun orbit (already known from Trio B) the familiar prime PH Sun orbit. Unless utterly coincidental (as Duke and Jones judge reasonable) or an evil miracle, the result reveals, that, at the time of his Trio A calculations (PH's −145), Hipparchos' adopted Sun orbit was transitioning from EH to PH, so he temporarily retained EH's eccentricity and Kallippic mean motion (until their new PH tables were computed & prepared), while immediately adopting PH's zero-point & apogee (neither needing tabulation). 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 *Saturday Night Live*, where ChurchLady's Faith-Based epistemology at last unmasks who's behind sinister *DIO*'s outrageously incredible, still-accumulating concatenation of impossible accidents: *could it be — SATAN*?!

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

F   Solar Orbit Reconstructions and Fruitfulness

F1   Hipparchos' −145 Prime PH solar orbit (§E1) is famous, because adopted by Ptolemy (*Almajest* 3.2&6) & still worshipped by Julian over 500^y after creation. But *DIO* recon-structed[56] two other Hipparchos orbits: his −157 Early EH orbit (§E1) & −134 Ultimate UH orbit (*idem*), each seriously differing from PH & previously unknown. (**NB**: Hipparchos' use of a late non-PH orbit was presciently induced a half-century ago before anyone by Britton 1967 pp.45-47.)

F2   Discovery of Hipparchos' final UH orbit arose from calculations,[57] refereed and supported by Thurston[58] and Curtis Wilson,[59] based on realization[60] that cuneiform text

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(eccentricity & speed) that had to wait for adoption 'til tabulated were naturally those that temporarily stayed EH, while the 2 swiftly-adopted PH elements (zero-point & apogee) were constants thus needing no tables. Perfect manifold correlation-confirmation? Or more *DIO* witchery?

[56] Rawlins 1991W §K9 & Rawlins 1991H eqs.13, 17-18, 28. Prescience: Britton 1967 pp.45-47, noted at Rawlins *op cit* §H2. Hipparchos (& Ptolemy) cited by Julian 1:429 (in "Hymn to the Sun").

[57] Hipparchos' ultimate improved data (−142 Autumnal Equinox, −134 Summer Solstice) caused his orbit-recalculation, thus shift from PH to UH: Rawlins 1991H §§C6-C13.

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

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

[60] Rawlins 1991H eqs.1-31. Babylonian astronomy specialist Britton helpfully added that *DIO*'s estimated date, −100±35^y (*ibid* eq.9), fit BM55555's writing style.

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 & §F9.

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

[1] Why the Aristarchos −279 and Hipparchos −134 Summer Solstices are the only[61] two among the twenty-eight solar records[62] of *Almajest* 3.1&7 where Ptolemy (who abhorred discrepant data) suppressed the hour — which we'd never have known, absent Babylonian cuneiform text BM55555.

[2] Hipparchos' final three calculated positions[63] of the Sun at *Almajest* 5.3&5 generally conflict with PH, but are all consistent with UH. (Jones appears to accept this analysis.)

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

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

[62] See, e.g., the bizarre attempt at Neugebauer 1975 p.284 (followed by Evans 1998 pp.273-274 & n.32, etc., contra *ibid* p.209, as noted below, at fn 127), to claim that Ptolemy was a BETTER observer than Hipparchos, oblivious to their relative errors, random & systematic (Rawlins 1999 §E — the section of this paper which was suppressed by *JHA* Editor Hoskin, without showing error of any sort). This joke-inversion is based merely on roundings in Hipparchos' semi-popular *Commentary* which are cruder than for his regular longitudes (*Almajest* 3.1&7.2) or declinations (*Almajest* 7.3). Neugebauer 1975 pp.642-643, deems Aristarchos' data nonempirically faked (similarly Evans 1998 p.72) vs Rawlins 2008R §A, sardonically at §A3, condensing the most unexceedable of JHAD fantasies (emphasis in original), "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 did, fixed the monthlength and sidereal year 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/jb24.pdf, *DIO 11.2* [2003] ‡4 Appendix 2 §L8.) 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 [‡2 §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' consenses, a hypothesis suggests itself which is consistent with this grotesque historical-distortion-by-historians-of-ancient-science: in a grant-grubbing era, public history's 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) wanted to promote, go-along-conservative Ptolemy's Aquinialy-enormous pandering to power-institutions and their brainwashed victims, ensured his works' massive preservation — thus, historians-of-science today can make a living by writing theses and holding conferences on a fat corpus. Revolutionary pioneer Aristarchos, by fighting the same institutions in honest and principled defense of emerging valid but upsetting scientific perceptions, ensured his works' near-extinction, so there's virtually nothing (explicitly — for those who cannot induce beyond the texts) left for moderns to fiscally exploit. Isn't the history-of-science profession inspirational?

[63] In the 1991 May *JHA* Jones 1991H p.117 claimed it's impossible to find a Greek orbit that satisfies these Hipparchan data, though all 4 elements of such an orbit had already been published by *DIO* (sources: fn 56 above).

by him from UH, unbeknownst to Ptolemy); however, the 2nd alone agrees (by chance, as it happens), though the underlying mean longitude he lists for it is discrepant by 5′ vs PH — even while tellingly agreeing[64] **to the arcmin** with UH.

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

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

[6] UH's epoch, the −127 Autumnal Equinox, follows Meton's sacred −431 Summer Solstice by exactly 304[y]1/4, so 16 or 2[4] such intervals just equal the 4868[y] "Great Year" of Aristarchos. And perhaps of Hipparchos himself: if the latter invented a version of the 4868[y] cycle at 1778021[d] (not Aristarchos' 1778022[d]: §G5 below) *it embedded an astounding quintuple of geometrically expanding cycles*.[65] Previous analyses never got past the 1st cycle of the five, e.g., Swerdlow 2010 p.174.

F4 From fitting EH to eclipse-Trio B, *DIO* mathematically induced (Rawlins 1991W §§K4-K9) in 1991 that Hipparchos' earliest Sun orbit, EH, [a] used a −157 summer solstice, [b] adopted Kallippic solar motion, 360°/365[d]1/4 for Trios A **AND** B. Findings [a]&[b] were both previously unsuspected. But, 14[y] later, papyrus *P.Fouad* 267A was examined by A.Tihon (paper 1st presented: Peking 2005) & was found[66] **to explicitly verify** 1991

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[64] Thurston 2002S pp.65-66. Error-amplitudes compared: Rawlins 1991H §§F1-F3. Waxing crescent Moon: *ibid* §§G1-G2; Shevchenko 1990's priority: Rawlins 2009E §E1. UH orbit epoch: Rawlins 1991H at eq.28. Proposed Hipparchan 1778021[d] variation on the yearlength of Aristarchos' 4868[y] Great Year: Rawlins 2002A (its fn 17) presented 2001/6/27 at British Museum conference, "Under One Sky" — condensed version published simultaneously in conference proceedings (2002).

[65] Quintuple succession of doublings (Rawlins 2002A at fn 14's conclusion): 304[K]1/4 (1[d] difference between Kallippic & Hipparchan calendars); 608[K]1/2 (1st return [360° advance] of saros cycle); 1217[K] (1st return of Sun); 2434[K] (1st return of Moon); 4868[K] (integral 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 1st interval yields, very nearly, sidereal (152[y]), Kallippic (76[y]), & Metonic (19[y]) cycles, where the 4868[y] Great Year encompasses about 2[8] of Meton's (Easter) 19[y] cycles. Details: *ibid* fn 17.

[66] Tihon *op cit*. The papyrus' solstice-day −157/6/26 (correct) seriously differs from Hipparchos' original false indoor-computed S.Solstice (−157/6/28), as reconstructed at Rawlins 1991W §K8, a point precisely resolved when Rawlins 2018U §K5 discovered both [1] the solstice's hitherto-unknown hour, 18[h] (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[d]1/4 − 1[d]/309. On 2015/4/8, the community was alerted to all this by email to a participant — and to the posting of *DIO* volume 20, containing the 2015 version of Rawlins 2018U presenting these solutions, plus the 1st formula ever developed for finding solstice observations' small ordmag-1[h] systematic errors (from Earth-orbit eccentricity), *ibid*: eqs.10-13. Not to mention *DIO 20*'s lead paper, with its important fresh discovery (§I1 below) of Archimedes' 3rd century BC use of degrees. There has been no engagement on any point as yet, except for a somnambulist-refereed *JHA* paper, Duke 2008W, which (at its pp.293-294) doubts Greek observational accuracy by centrally confusing systematic error with random error, causing misfire 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 *JHA*'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 §D3-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 CalTech in 2007, timing which suggests the possibility that the JHAD perceived danger right away: the nightmare of general recognition of the foregoing triple-miraculous papyrus-vindication, of a banshee's paper which had also satirically-advertised such typically-refereed *JHA* discoveries as the Velikovskian 360[d] yearlength by Duke's *JHA* co-boardmember Jones (Rawlins 1991W §G9,a *DIO 4.1* ‡4, 1994) — so the usual bungled lead-paper

discoveries [a]&[b]. (Tihon has further shown that, c.−150, Hipparchos experimented with previously unknown versions of solar motion, epoch, & precession.)

F5 Reaction to *DIO*'s vindications has been less than inspirational — but valuably revealing, nonetheless. While still under the influence (fn 59 above) of the late Curtis Wilson, Jones graciously assented[67] 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] 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[h].

F6 *NB*: These *DIO* induction-predictions aren't side-issues. They are central to understanding the early years of Hipparchos' evolution from amateur-observer-astrologer into an immortal empirical scientist. And subtractors have been uniformly oblivious to a central steel connexion, revealing his original resort to calculating not observing his earliest, grossly erroneous Summer Solstice in −157, indoor-computed using the obsolete Kallippic calendar: this is the most conspicuously odd building-block of the lopsided EH orbit, accounting for most of why EH's *e&A* were so flagrantly awful: $e = 3^p 1/4$ (vs $2^p 1/2$ PH, & $2^p 1/10$ actual), $A = 44°$ (vs 65° PH, & 66°1/2 actual).[68]

F7 In 2008, Duke, in yet another unrefereed Pb paper for *JHA* (whose board he had earlier silently joined while *DIO* Editor), scales new pinnacles of ancient-empiricism-denial, as he tries razing the entire basis of Greeks finding accurate orbits (such as PH&UH), claiming[69] that their equinoctial solar declinations' error averaged c.15′, nearly equal to the angle from solar center to limb!

F8 This is but a jawdropper case of confusing systematic error with random. The Duke paper's Table 1 displays admirably well-computed times of Hipparchos' equinoxes, whose errors are clearly sprinkled ordmag 0°.1 positively&negatively on either side of zero. Undoing Duke's historian-usual (‡4 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)[70] which corrupted all these equinox

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denigration (equally well-refereed: see challenge here at fn 70) was adduced to head off that hideous eventuality. (And without even citing Rawlins 1991W, the very paper whose thesis is being trashed! — a wise precaution, to prevent anyone from checking anything — also without mentioning that *idem*'s math has been endorsed by various experts, specified at Rawlins 2018U fn 10.) No surprise. For a shun to keep working (fnn 116&125), such intermittent commando operations are simply *standard-maintenance*. And, unlike for a military attack, you can completely screw up, but — to your own outlander-resenting mob (§J2) — *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 Hipparchan eclipse trios [analysed in Rawlins 1991W] are worthless), to evaluate its credibility or that of the multiply-misbegotten case brought forth against Greek accuracy. (See Rawlins 2018U §B4&N and fnn 10&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., Thurston 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.

[68] EH&PH elements compared at Rawlins 1991W: §K9 vs §K10. Duke 2008W pp.293-294 calls *DIO*'s reconstructions not "conclusive" due not to *DIO* errors in the underlying math but because said math is too "sensitive", implying (fn 66) that §F4's double-vindication by papyrus was merely spooky-lucky. These inexcusably (esp. §D6 here) citationless attacks are met in fn 37 above, and indeed had been anticipated decades ago in Rawlins 1991H §H3 & Rawlins 1991W fn 205.

[69] Duke *op cit* pp.284-289 (vs fnn 70&96 here). Solstices' immunity from refraction, etc: Rawlins 2018U §G1. Without sneaky *DIO* black-magic, odds against EH's three chance-hits within 20[m] of the three distinct cardinal-points: 8000-to-1 (*ibid* §N4[d]).

[70] Systematic error +6′1/2 or 7[h] Rawlins *op cit* §B4. Same results years earlier: Britton 1967 p.24, R.Newton 1977 p.78, Swerdlow (Rawlins 1991W fn 280) accurately confirmed by Swerdlow 2010

observations by that amount on average (while not affecting his solstices), an ordmag higher than his actual $2'$ *random* solar-declination single-datum rms error (or scatter).

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

F10   Though fully aware of inconveniently-existing *P.Fouad* 267A, the same Duke paper nonetheless pretends that *DIO*'s now-papyrus-confirmed predictive hit-[a] & double-hit-[b] (§F5 above), are ENTIRELY ACCIDENTAL — occultist shades of himself and Jones (fnn 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 5.3&5 — which Rawlins 2012V calls successively Trios A, B, and C — as do the EH→PH ("Frankenstein"), EH, and UH orbits, respectively; *AND* [ii] whose underlying cardinal points (Vernal & Autumnal Equinoxes and Summer Solstice) uniformly hit upon Hipparchos' standard $1^d/4$ precision — dawn, noon, evening, midnight — as all 9 cardinal points for EH&PH&UH (not to mention *P.Fouad* 267A: fn 66 here) conspicuously do. Duke has been publicly challenged (fn 70) to produce his alternate orbits. Nothing has come forth.

G   Aristarchos' Yearlengths, Pre-Hipparchos Precession, & Pre-Babylonian Accurate Monthlength.
History-of-science Archon's Talibanishment of Evidence.

G1   The mystery of the superficially-nonsense ancient yearlengths[71] 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: τ ξ ε δ′ κ′ ξ β′ and τ ξ ε δ′ ι′ δ′, or 365 4′ 20′ 60 2′ and 365 4′ 10′ 4′.

G2   Taking the numbers *exactly as they stand* and allowing signage-flexibility,[72] Rawlins in 1980 treated[73] both Aristarchan 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 60^{ths}, Rawlins saw that the 1st expression could be viewed as $365^d 1/[4 + 1/(20 + 2/60)] = 365\ 1/4 - 15/4868$, a classic Metonic "tropical" year, quite

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p.199. Invented *Almajest* $-134/6/26$ noon solstice: Duke 2008W Table 1. Challenging him to produce 1/4-day-cardinal-point Hipparchan orbits with (§F11 & fn 69) distinctly alternate elements: Rawlins 2018U §N2. Awareness of the *P.Fouad* 267A papyrus that confirmed *DIO*'s uncited §F4 discoveries: Duke *op cit* n.9. For the $-145/3/24$ 11 AM equinox observation (*Almajest* 3.1) of the refracted Sun, measured on the large κρικος (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^h & accounting for refraction, there was a $1'$-$2'$ error of observation, some of even that perhaps from over 100^y of the ring's settlement (§I10 & fn 122 below). See Rawlins 1982G fn 17 & Rawlins 2018U §B4.

[71] Yearlengths tabulated: Neugebauer 1975 p.601. Scholars' unproductive decades of disagreements: sources at http://link.springer.com/book/10.1007/978-90-481-2788-7, Jones 2010B p.21 n.28.

[72] Discussions: Rawlins 1999 §B5 & Rawlins 2002A fnn 13-14.

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

[74] Mathematical details: e.g., *ibid* & Rawlins 2002A fn 15 & eq.12. Both *JHA* referees W.Hartner & K.Moesgaard recommended publication of Rawlins 1999 — Editor Hoskin's written acceptance in principle 1981/9/17. (Advertised as forthcoming in *Isis* 1982 March, under altered [*JHA*-viewpoint] title. Cited as in-press at Rawlins 1982G [*Isis*] n.14.) Some of paper's results relayed supportively by Moesgaard 1983 p.57 & n.14, citing original title. Credit for 1st perception that ξ (60) indicated sexagesimal notation belongs to Neugebauer 1975 p.602. For Rawlins 1999's ultimate fate, see fn 97 below, and *DIO 1.1* p.11 fn 25, 1991.

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close[75] to the known (also seriously false) tropical yearlengths of Hipparchos & Ptolemy. The 2nd expression suggested $365^d 1/[4 - 1/(10 - 1/4)] = 365\ 1/4 + 1/152$, differing but ordmag 10^s from the actual sidereal year then (fn 114 below).

G3   Both results' implicit periods, 4868^y (Great Year) & 152^y (2 Kallippic 76^y cycles between iconic Meton's and Aristarchos' S.Solstices, -431 & -279, respectively), are among the EXTREMELY[76] few numbers long known to be relatable to Aristarchos, and the difference between the 2 induced yearlengths **IS** precession, the very discovery traditionally mis-ascribed to Hipparchos. Said difference is close[77] to $1°$/century, which presumably later influenced Hipparchos to treat $1°$/cy as a lower limit, though Ptolemy eventually adopted $1°$/cy exactly (*Almajest* 7.2-4). Note that Aristarchos is the only astronomer on the Vatican mss *cited for two different yearlengths*, obviously suggesting precession. As the 1st astronomer we know was a public geomobilist, he is an apt candidate for true discoverer of Earth's precessional wobble.

G4   To measure the Moon's mean motion & apogee, ancient scientists wisely chose (*Almajest* 4.2) the 4267 month eclipse cycle for its $126007^d 01^h$ interval's felicitous near-constancy (due to near-perfectly-integral return in 4573 anomalistic months), regardless of ecliptic position. That interval's tiny inconstancy-amplitude[78] of c.$1^h/2$ guaranteed the deduced monthlength's accuracy to one part in ordmag 10 million. (Divide 4267^u [4267 months] by $1^h/2$ to see this; the result is merely an upper bound on the better accuracy attainable by round-the-zodiac averaging.) *DIO*'s exploration of the 4267^u cycle vindicated Ptolemy's oft-doubted contention that it was the historical source of the ancients' highly ac-curate monthlength M, commonly miscalled the "Babylonian month". Rounding at the 10s-place of the 2nd sexagesimal term (as we find on cuneiform texts: fn 80) yields the M attested at *idem*: $126007^d 01^h/4267 = 29^d 191°00'50'' = 29^d 31'50''08'''20'''' = 29^d 12^h 44^m 03^s 1/3 = 29^d.530594$, correct (even today!) to a fraction of a timesec. Aristarchos' 223-month saros expression (*idem*) will, if divided[79] by 223, yield $M = 29^d 12^h 44^m 03^s.2$, which agrees with above "Babylonian" M, to one part in tens of millions (fn 81 below) — decades before its first known appearance in Babylon, which favors his pre-Babylon authorship of M, as does the *chronologically ordered* Vatican mss' long-overlooked listing of Aristarchos prior to anything Babylonian. TWICE. [See www.dioi.org/j913.pdf, Tables 1&2.]

G5   We know[80] that *Almajest* 4.2's saros of $223M$ agrees[81] to 1 part in 24 million with *idem*'s deceptively-round-looking saros expression, $18^K + 10°2/3$ or $18^K + 4/135$ (where superscript K signifies Kallippic Years of $365^d 1/4$ each) — which, times twice 135, so

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[75] Metonic "tropical" year: *Almajest* 3.1; Rawlins 1999; Tihon *op cit*; origin 1st rightly suspected by T.Mayer in the 18th century; later by, e.g., Swerdlow; cause of ancient tropical yearlengths' large common error traced by a stimulating paper, Moesgaard 1983, and by Rawlins 1999 §D4 & Rawlins 2018U §P7.

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

[77] *Ibid* fn 14 shows that, whatever one's sign-choices for the latter digits of the Vatican mss' Aristarchan expressions, implied precession will still be near $1°$/century, Hipparchos' lower limit (Rawlins 2018U §K5), verified by Tihon *op cit*.

[78] The 4267^u cycle's crucially&conveniently trivial inconstancy: 1st roughly quantified by Rawlins 1996C fnn 18&56, *en route* to verifying it's the empirical source (*Almajest* 4.2) of ancients' discovery of the key relation $251^u = 269^v$. Note revealing Muffiotic inversion at Toomer 1984 p.176 n.10.

[79] *Ibid* eqs.5-8. Aristarchos' $4868^K = 1778037^d$ Kallippic Great Year simultaneously ensures integrality in days, months, Kallippic years, saroi. Paper delivered 2001/6/27 at the British Museum conference "Under One Sky" a condensed version of which appeared simultaneously in the conference's 2002-published proceedings. Aristarchos' Great Year recovered by P.Tannery in the 19th century (fn 76 above and Heath 1913 pp.314f).

[80] *Ibid* eq.8, with the deft capstone-contribution, pointing out the conventional rounding-point, thanks to the long-experienced advice of John Steele and John Britton (Rawlins 2002A §A8), neither concurring with our conclusion.

[81] See www.dioi.org/jb11.pdf, Rawlins 2002A §A6.

precisely produces day-integral 1778037^d, Aristarchos' Kallippic Great Year of 4868^K — results which yield:

Discovery #1: The Vatican ms' Aristarchos-marked year, 365^{d}1/4 − 15/4868, is certified as Aristarchos' by its denominator's match to his 4868^K 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[82] 235^u = 19^y, we next arrive at a vindication for the kind of exploratory hypothesizing (§J1 [f]) that can occasionally move knowledge ahead: 4868 years of 235*M*/19 each equals Aristarchos' Metonic Great Year, 1778022^d (15^d less than his Kallippic 4868^K Great Year). Dividing by 4868 to find the cycle's yearlength[83] produces 365^{d}1/4 − 15/4868, perfectly matching the figure (Discovery #1) independently found (§G2) two decades earlier from Vat. gr. 191 fol. 170v: *classic predictive success*. Nonetheless, Muffiosi typically *refuse to cite* (e.g., §G11 below) the confirming evidence. Jones even goes out of his way to *destroy* (§G8 below) evidence for Aristarchos' multi-obvious possession (c.280 BC) of a monthlength accurate to a fraction of a time-sec, the better part of a century before Greek-conquered Babylon is known to possess such (c.200 BC) — possibly, just possibly, because pan-Babylonianists have made a living contending or pretending that the origin of such wisdom and precision is to be found only in the cuneiform tablets of a plainly inferior, scientifically-unsophisticated *and trigless* civilization (fn 120; ‡2 §N13), which their cult has become permanently, undeterrably, in-love with?

G6   We can also merge both cycles found from the Vatican mss (§G2) by noting that 152^y is virtually 1/32 of 4868^y, 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}1/4 + 32/4868 = 365^{d}1/4 + 1/(152+1/8), or about 365^{d}1/4 + 1/152 (which of course matches §G2's Vat. 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^y, roughly a degree per century.] Summarizing: the two continued-fraction solutions we found to be embedded in the Vatican mss' data, were derived by him from his Metonic Great Year of 1778022^d & his Sidereal Great Year of 1778069^d, resp. The latter's obvious parallel to the former just adds to the astonishing multiple-vindication[84] 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:[85] how to alter evidence, to undercut a paper that doesn't, and how to trash into chaos coherent&untampered inductions which twice accurately extract cycles (4868^y & 152^y) 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** wrecks the data-basis for Rawlins 1999's refined, precious, precise inductive journey from Vatican mss to [1] ancient science's ingeniously constructed (Rawlins 2002A §A) & modern induction's 4-way (fn 88) *re*constructed 4868^y Great Year [itself from superaccurate *M*: §§G4-G5, www.dioi.org/jb11.pdf, eqs.5-7], & [2] Aristarchos' sidereal year 365^{d}1/4 + 1/152. How does such holy warfare differ from the Taliban's? [Query *en-passant*: **would ancient astronomy historians accept NYU-InstStudAncWorld Director Jones' deleting all accents from the mss of the** *Almajest*?!] Like-Talibanish is his authoritarian **justification** for across-the-board wipeout: decreeing *accents on Greek* **mathematical** *data are* **destroyably** *worthless* **in HIS expert judgement**.

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[82] See Moesgaard 1983 or Neugebauer 1975 pp.354f.
[83] Derivation at Rawlins 2002A, www.dioi.org/jb11.pdf, eqs.5-13.
Dozens of similar *DIO* vindications are collected at www.dioi.org/vin.htm.
[84] *DIO*'s exact confirmatory hits here are akin to R.Newton's also-years-later fruitful success: §E3.
[85] Jones 2010A p.21. Counterhexing by eliminating accents: *ibid* n.27.

(Which expert mathematician[86] 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 manuscriptic vandalism. (Though see Rawlins 2000A ⊙ 13.) I.e., it's the kind of originality that explains why pioneer Jones adorns *JHA*'s elite Board of Advisory Editors.

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

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

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[86] 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 (Velikovsky's 360^d/year calendar). [d] A trio of longitudes (e.g., *Almajest* 5.3&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 §A [1994] for 360^d/year recognition.) [e] Hipparchos' Alexandria city geographical latitude *L* was 31° (Jones 2002E p.16, contra fn 27 above & Neugebauer 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°01′ (*ibid* p.17, contra the text [fn 28] and Rawlins 2009S fnn 40&41). [h] The Diller-Rawlins 14-for-14 fits, in an *ultra*-sensitive case (fn 33 above), are *yet 14* **MORE** *magic accidents* (fn 55 here). [i] Hipparchos didn't use celestial tables (Jones 1991H p.120; discussed at Rawlins 1991W §E4), contra direct testimony of 2005-revealed papyrus *P.Fouad* 267A. [j] Hipparchos' Syracuse was 200 stades from where Hipparchos placed it (fn 27 above). For one with a trifle less than an entirely perfect judgemental record, to issue overrule-decrees dispensing with coherent solutions, in favor of his own incoherent ones, might suggest nonsurfeits of appropriate humility and caution.

[87] Like confusion: fn 28, or Rawlins 2009S §H. Rummaging: Jones 2005 pp.21-22; fnn 24&27-28.

[88] Keep in mind that we have mutually-confirmatory *quadruple* evidences of the 4868^y Aristarchan 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 down to hohum-mere *triple*-confirmation, he might yet swing observers to sharing his occultist rejection of Tannery-Heath-DR reconstructions.)
[1] Censorinus' testimony (fn 76 above, and item [2]).
[2] *Almajest* 4.2 saros or exeligmos expression (fn 81 above); developed at Neugebauer 1975 p.603, where 1,21,8 is sexagesimalese for 4868.
[3] Vat. gr. 191 fol. 170v→365^{d}1/4 − 15/4868 (fnn 72-73 above.)
[4] 4868·29^{d}31′50″08‴20⁗·235/19 ≐ 1778022^d, thus (fn 83) matching the remainder-numerator of item [3]'s Great-Year yearlength 365^{d}1/4 − 15/4868 (§G5).
(See parallel fnn 83&84 above. And carefully check the galloping successes reviewed at Rawlins 2002A, culminating in its eqs.10-13.)

H   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-chauvinist tamperers have, with dedicated persistence & Creativity, tried misprojecting[89] onto valid data (above §§C-G) that commit the crime of being inconvenient to prevailing Muffia preconception.

H2   Neugebauer classified the seven "circuli" of Pliny 6.39.211-218 as a primitive "arith-metical"[90] scheme, deeming their superficial inaccuracy to be supportive of his contra-reality (fn 8 above) insistence on "the absence of any scientific organization in antiquity", a con-viction which he thought helped (along with his reasoning as covered in fn 1 here) to exonerate accuracy-challenged Ptolemy. But the 1st — and still only available — coherent solution (fn 91 below) ever devised for the circuli indicated they are no more arithmetical than Hipparchos-Strabo's klimata (which Neugebauer deprecated similarly: fn 18 above), proposing that Pliny's circuli are instead trigonometric (as are Hipparchos' klimata) and a case where it is reasonable to test[91] 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. Exper-imentally shifting all circuli M by the same simple round constant,[92] $1°$ (or 4^m), finds gratifying consistency with the same accurate Hipparchan $23°2/3$ obliquity discovered by Diller and insubstantially & cementally rejected by Neugebauer&Jones (fnn 18&25, resp).

H4   Rawlins' 1984 paper (invited for the Greenwich Meridian centenary Longitude Zero Symposium), featuring its $1°$-constant-shift solution (fnn 91-93 here) of the circuli, has never been cited by Jones, though his own pale constant-shift ploy for the Hipparchos-Strabo klimata (§§C5-C10 above) is either suspiciously or parapsychologically similar, with the difference that Rawlins' constant-shift solution for circuli turns chaos into coherence, while Jones' constant-shift for klimata is subtractively designed to accomplish the very reverse, leaving such a poor fit to the data that he doesn't dare even tabulate them (fn 34 above). Jones mentions Pliny's circuli but simply calls them "crude"[93] (in the Neugebauer tradition, fn 18 above), never telling readers that Rawlins' restorative-correction procedure — which is just as elementary and trivial as his own failed (fn 34) klimata-echo of it — reveals a trigonometric scheme whose cleverness is crudeness' antithesis. Nor does Jones' circuli-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

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[89] Conversely, our tamper-projectors staunchly spurn restoration for either of our cases here where its need is screamingly obvious: Trio A's 3rd 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 mere déclassé sorcery.

[90] Pliny 6.39.211-218. Neugebauer 1975 p.747. No ancient scientific organization: *ibid* on pp.367, 667, 748, 938; recent History of science Society 20 pp echoooooooooooooooooooo: Shcheglov 2016.

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

[92] See Rawlins 1985G, comparing eq.11 vs eq.12, where an ancient scholar's well-intended but uncomprehending $1°$-shift-of-M-data is explained at p.263. All of the circuli shadow data are expressed in feet except for the Rhodos 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 "vnciae" [inches] thus 105 corrupted to "c vnciae" (100 inches), as we now find in Pliny. Once this is realized, and other data are checked, it's obvious that 105 is the common denominator to all seven shadow ratios, (but Rome) & is key to the equation (*ibid* eq.11) that originally generated the Pliny circuli (ere ancient alteration to eq.12).

[93] "Crude": Jones 2002E fn 11. Or clever? See restored circuli's smoothly and flawlessly sphtrig-tracking curve, graphed in Rawlins 2009S Fig.1, in hollow dots: close proximity to the curve of the dark filled dots (Diller-Rawlins klimata values), over the restricted Mediterranean range (much smaller than Diller's) for which the circuli were designed.

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

H5   Our next chapter presents dozens of new perceptions, all rigorously JHAD-uncited, possibly due to the history-of-astronomy cartel's relative ranking (§J1) of cult-discipline vs knowledge-advancement.

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

I1   Though numerous scholars have doubted 3rd century BC Greek scientists' adoption of Babylon's degree-division of the circle, it was unexpectedly revealed[94] in 2012 that Archimedes' original unprocessed measure of the Sun expressed its diameter sexagesimally as in the range between $27'$ and $33'$, later conventionally published in his *Sandreckoner* as rightangle/200 and rightangle/164. Hard to believe (given the ultra-simplicity of the math): no one has noticed this for the last 2000y.

I2   For centuries, scholars have accepted *On the Sizes and Distances of the Sun & Moon* as by Aristarchos of Samos, **numbly missing the significance of Archimedes' accurate contradiction** — by a factor of four — of *Sizes*' farcical[95] fundamental empirical data ($2°$ solar&lunar diameters!), & in spite of its ludicrously contra-reality requirements that: [a] total lunar eclipses last 12^h (Neugebauer 1975 p.642), & [b] for Mediterranean observers, the Moon (at distance c.20 Earth-radii) visibly moves in-**REVERSE** among the fixed stars every day around culmination. Item [b] earns a truly special place in the Bizarrity-Hall-of-Infame, by the astonishing fact that, again: no one has noticed this for the last 2000y.

I3   In the 27y since publication (Rawlins 1991P fn 6), no modern scholar has yet cited *DIO*'s discovery of the elementary cause of §I2's 4-factor-overestimate: pseudo-Aristarchos misinterpreted Aristarchos' lunisolar diameter of 1/15th of a "part" or μερος of the zodiac as: 1/15th of a zodiacal sign. But μερος (meros) was just an ancient unit = circle/48 = $7°1/2$, of which 1/15th exactly agrees with Archimedes' correct half-degree testimony.

I4   Aristarchos' famous $87°$ half-Moon elongation wasn't empirically a precise figure, as moderns (and seemingly Hipparchos: §D2 above) assume, but a lower-limit.

I5   From tiny solar declination-motion near solstices, uninformedly sneering[96] amateur-astronomer-Ptolemists (incl. MacGenius Swerdlow: ‡2 fn 8) doubt ancient solstices could

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[94] Rawlins 2012T, www.dioi.org/jk01.pdf, §E (prior opinion: *ibid* §B1), DR's discovery of obvious sexagesimal source perceived c.30y after his contra-consensus 1983 Dec *Isis* Letter (& Rawlins 1991W fn 53) arguing 3rd century Greeks used degrees. Note Neugebauer 1975 p.590's just caution.

[95] *Sizes*' 5 farces & Aristarchos' 1/2-Moon-elongation $87°$ lower limit: www.dioi.org/je02.pdf, Rawlins 2008R §§C1-C3 & fnn 17&29. [By proportions, Aristarchos-Archimedes' shift (Rawlins 2008R fn 37 vs eq.15) from Sun-distance 1000^r to 10000^r altered limit to $89°.7$ (nearer actual $89°.85$).] For "part" (μερος or meros) ≡ circle/48: see Neugebauer 1975 pp.652&671, or Rawlins 1991P fn 6.

[96] Those wondering whether Swerdlow&Gingerich are primarily scientists or careerists, cannot miss their amusingly indiscriminate alibis for Ptolemy's fakes, at www.dioi.org/jk02.pdf, Rawlins 2018U §§B2&B3 and fnn 2&3. (And Duke at fn 69 above, vs Rawlins *op cit* eqs.5&21 Table 3, & §§E-J. Alleged solstice-inferiority: fn 27 here, Evans 1998 p.206, & Swerdlow 1979 [Phi Beta Kappa] p.527. Noel Swerdlow, though occasional valued discoverer [e.g., fn 42], is a prime contributor to *DIO*'s catalog of *JHA* scientific larfs: www.dioi.org/jhb.htm.) Besides *scientific* indicia (*see ‡4 §B3* on Swerdlow's & Evans' innocence of equal-altitudes, the obvious ancient solstice-finding method), there are unambiguous *historical* points they're equally (and, as purported historians, less excusably) unaware of (unlike non-politicians such as Toomer 1984 p.12 & Britton): **all known ancient scientists found yearlengths via solstices not equinoxes**: Meton, Euktemon, Kallippos, Dionysios, Aristarchos, Hipparchos, BM55555. And these doubters of ancients' solstice-accuracy (plus fn 27: Jones) are now confronted with recently recovered papyrus *P.Fouad* 267A, testifying to a Hipparchos solstice accurate to ordmag 1^h (‡4 fn 20 [C]). Preferring solstices over equinoxes for year-length-determination becomes

be good to ordmag 1^h, which invites equinox-vs-solstice accuracy-comparison (‡2 §N7).

I6   Rawlins 1991H found the Babylonian yearlength on famous Astronomical Cuneiform Text 210 was (§F2) based on well-known Greek solstices ($-431/6/27$ & $-134/6/26$), the 1st datable transmission of an orbit-element between Babylonian & Greek astronomy, going Greece→Babylon, thereby gutting the Muffia's holiest tenet (§I37&fn 120).

I7   Aristarchos&Archimedes put minimum solar&stellar distances at 10000^r&10000 AU, resp, **because** humans can see (as no historian-of-science seems aware) to c.1/10000 rad. Rawlins 2017E §K2: "For advancing history of science, knowing **science** matters."

I8   Ptolemy's allegedly-outdoor 4 solar "observations" (132-140 AD: *Almajest* 3.1&7) averaged over 1° error (*not even 1 shot encroached anywhere on the solar disk*) and were **fifty times** nearer Hipparchos' 3-century-old indoor tables than to outdoor reality (as known for 1200^y: F.Ragep 2010 p.121): undeniable but also (for over 30^y) *JHA*-unprintable.[97] Despite null ancient attestation of their speculation, Ptolemists insist (fnn 8&127) this is because ancient scientists kept only observations agreeing with theory. (I.e., they cheated.)
A. How could Ptolemy's solar "data", all off by *a degree* happen in the 1st place, outdoors?
B. How did it happen that Hipparchos' Sun observations' average disagreement with reality were merely two or three times their disagreement with his tables, vs Ptolemy's fifty?
C. How could Greeks *just copying predecessors* achieve their many accurate discoveries? (E.g., Greek vs real ancient monthlengths, Rawlins 2017E §§B2-B4: **startling proximity**.) However, most scholars (A.Pannekoek, R.Newton, Y.Maeyama, J.Brandt, & P.Zimmer among the welcome exceptions) reject ancient high scientific accuracy (fnn 1&35 here), undeterrable 2016 subtractor D.Shcheglov even (fn 13 here) calling it a "delusion."

I9   Aristarchos discovered precession 150^y before Hipparchos: §G3.

I10   Rawlins' order-of-magnitude estimates, of too-unheralded Greek accuracy (‡1 §F): 1′ for lunisolar diameter; 1% for moon distance; 1^h for solstices; 1′ for equinox taken on Alexandria's ring (fn 70 here; *Almajest* 3.1); 10^s for sidereal year (fn 114 here); 1′ for observer latitude (see fnn 39&100 here and ‡4's Table 1 & fn 38); 1′ for 500-mile North-South arcs on Earth (fn 111); 0°.1 for star declinations (fn 100); 0°.1 for lunar-limb-vs-Sun gap (fn 12 here); 0°.1, even 1′ for star-vs-eclipsed-Moon gap (§B7); ocular error 1′±1′ (fn 97 here) for Hipparchos' mean equinox, with 2′ scatter (fn 12); 1% for Earth-circumference-measure precision (fn 108); 10^m for lunar eclipse-prediction (fn 97 here); 1^m for lunar eclipse observation (Rawlins 1985G pp.258&265); 1′/century for mean motion of Mars (and maybe Venus); 1^s for all three adopted months (synodic, anomalistic, eclipse), each correct to better than one part in a million. Historians-of-science like Shcheglev show no sign of awareness of any of these Greek achievements.

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obvious from, for instance, Britton 1967 p.29. More generally, §I8's key Obvious Question C jokifies fn 8's Swerdlow-Gingerich-*Scientific-American*'s Ptolemy-exculpation-theory.

[97] On 1982/7/17, ever-Ptolemy-worshipping *JHA*, unable to argue facts or math, nonetheless belatedly excised Ptolemy's sensational 50-to-1 indoor-vs-outdoor ratio (§I8) from a projected Rawlins paper (fn 74 here; precensored text at Rawlins 1999 §E), along with nonselector outdoor-observer Hipparchos' parallel ratio of just 2-to-1 or 3-to-1. (Even that tiny ratio reflects just that his tables were founded upon his own slightly imperfect solar data. His UH tables' eclipse-prediction accuracy was ordmag 10^m: Rawlins 1991H eq.32.) The 7′ mean error of Hipparchan equinoxes is mostly not due to eyeball inaccuracy: when one removes the effects of his presumed solar parallax and (like fn 70 above) the errors from refraction in the Sun's zenith distance, and his instrument's setting-tilt from polestar-refraction, there's only 1′ (±1′) unaccounted-for: see Rawlins 2018U §B4. Mars (and Venus?) mean motion accuracy c.1′/century: Rawlins 2002V fn 26. Contrast to fn 1 here, and to the umpteenth fruitless metrological analysis of Eratosthenes' Earth-circumference, Shcheglov 2016, www.dioi.org/shc.pdf, which massively contends that high-accuracy Greek longitudes are mythic, his entire assault *upfront*-promoted by History of science Society (and post-protected by HsS stonewall-noncorrection), ultimately undone by his crucially confusing a solar eclipse with lunar and falsely putting Spain into the eastern hemisphere (and China's Xi'an & Luoyang into the western). Yet another history-of-science journal's all-too-common Pb-paper duffer-science: details of these latest *Isis* disasters can be found here at ‡1 (§D), *DIO*'s Letter-to-*Isis*' hiding-since-receipt Editor H.F.Cohen.

I11   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 naïvely 4/3-stretched map of Ptolemy's 160 AD *GD*, the *Geographical Directory* (often called just *Geographia* or *Geography*), finding that Greek mean longitude error was well under 1°, thus indicating that ancient scientists had cooperated in comparing (as recommended by Hipparchos: Shcheglov 2016 n.7) local times of lunar eclipses at even far-distant sites (fn 105). E.g., the unstretched 42° longitude-difference between Carthage and Persepolis is correct to ordmag 1%. **NB**: The distorted remains of accurate ancient geography in Ptolemy's *GD* were ruined in *two widely separated stages*, and in two very different ways (*but sharing an* **astrologer**-*source in each case*): [1] Latitudes were semi-*randomly* wrecked by Hipparchos (contra Rawlins 2009S fn 18): his discrete tabulation of them (*GD* 1.4.2) for professional reasons. (See sources here at fn 13 for details; Hipparchos was publishing tables [Tihon 2010's valued direct revelation] which served 3 astrological traditions simultaneously, sidereal (or anomalistic), 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 astrologer Ptolemy's ignorant stretch of correct eclipse-based longitudes by 7/5 or 4/3 (‡1 §§F&M).

I12   By successive approximations, mathematician Hugh Thurston in the 1940s detected previously un-noted evidence that Ptolemy had fabricated planet-orbit elements by backwards-calculation, later publishing the analysis in *DIO 4.2*, www.dioi.org/j426.pdf, ‡6 (Thurston 1994P). Its logic, though obvious, remains uncited by historians-of-science.

I13   For decades, historians-of-science argued (e.g., fn 18) against Hipparchos' possession of spherical trigonometry, a position now indefensibly obsolete: for a pioneering all-inclusive compendium of four plain evidences (& fn 103 here) that Hipparchos had full spherical trigonometry, see www.dioi.org/cot.htm#tvvc.

I14   Rawlins long insisted that the *Almajest* Mars mean motion was based on ratio 152145°/329621^d. Inspired by Duke's skepticism, Jones in 2002 discovered it was based on a different cyclic ratio that Rawlins had carelessly neglected, residing, ironically, in the *Almajest* itself. In the same article, Rawlins gave a similar explanation of *Almajest* Jupiter mean motion, equally false historically, reflecting even less-excusable overconfidence. The correct cyclic Jupiter solution was simultaneously realized by Jones and Duke. Rawlins faxed double-congratulations to Jones the hour he heard of the finds, announcing them in 2003 on *DIO 11.2*'s cover, establishing for years a $1000 prize for each overturning of himself (www.dioi.org/pri.htm), thus retroactively sending $2000 to Jones (no reply).

I15   Generalizing from the bases of mean motions of the planets, the Moon, & (partially) the Sun, *DIO* created a General Theory of Ancients' Cyclicities, proposing it was preferred ancient method to found mean celestial motion upon empirical integral-return ratios.[98]

I16   It's long been assumed that Hipparchos rounded the time of dawn or evening to the 1/4-day, even near solstices. But, in 2015, *DIO* showed that Tihon's papyrus is consistent with Hipparchos using exact time for each when gauging yearlength, thus revealing (fn 66) the origin of his hitherto-unexplained 365^{d}1/4 − 1^d/309, which Tihon was 1st to reveal.

I17   In 2002 K.Pickering discovered[99] that original locations of dozens of erroneously placed stars in the *Almajest* 7.5-8.1 catalogue can be reconstructed (occasionally via spherical trigonometry), but it was repeatedly found that repairs could only work with coordinates from Hipparchos' era, not Ptolemy's; sixteen years later, Ptolemists have yet to produce a collection of such reconstructions based upon the opposite theory, leaving the independent implication that the catalogue was observed by Hipparchos, later plagiarized by Ptolemy.

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[98] See www.dioi.org/pri.htm, for *DIO*'s longtime $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 2003J §§G&H, cover, & fn 21. (Both men agree to Rawlins' period-relation solutions for Mercury, Venus, Saturn.) "General Theory of Ancients' Cyclicities": Rawlins 2002B §H. Non-planetary cyclicity examples: *ibid* & Rawlins 1996C.

[99] Pickering 2002C.

I18   In 1982, Rawlins sought the epochs E, as well as the errors x in the observers' assumed latitudes L, and their standard deviations, for all 4 known ancient stellar-declination observers — Timocharis, Aristyllos, Hipparchos, Ptolemy's anonymous observer — through bivariate least-squares testing upon these observers' star-declination data.[100] In 1994, ex-

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[100] Rawlins 1994L §§F3-F8, where nulls show Timocharis (known observations c.−300-271), earliest of Alexandria's 3 observers of extant star declinations by surveying instrument, alone knew his exact L. Later studies get virtually the same E for each observer. Maeyama 1984 finds thusly, but instead of *DIO*'s bivariate least-squares (Rawlins 1994L Table 3 results), he independently finds observers' epochs E and latitude-errors x by double-monovariate, noncalculus trial-and-error, and graphs — gauging E's standard-deviation by eyeballing, and x's by confusing it with that for a single datum. More recently, in the mathematically-challenged *Journal of Astronomical History & Heritage* [*JAHH*], Brandt *et al* 2014B, www.dioi.org/bzj.pdf, says most of Rawlins' geographical latitudes L "are close to our values". But their L are merely assumed, so *DIO*'s *JAHH*-requested 2014/8/26 referee report, www.dioi.org/jau8q.pdf, asked that the paper notice *DIO*'s entirely original 1994 nulls-method of *non*-assumptively determining L (Rawlins 1994L *loc cit*), a discovery neither available ***nor even cited anywhere else — a quarter-century later***. A few referee-recommended corrections were fortunately applied by *JAHH*, e.g., Aristyllos-epoch's standard-deviation (compare www.dioi.org/bzj0.pdf, p.5 to final www.dioi.org/bzj.pdf, Brandt *et al* 2014B p.331). But the request for showing how L finally could be found (not guessed) was, among others, not met by *JAHH*. See ‡4 §C16 below for the weird details. Also and more importantly unmet was the *DIO* referee's urging the Brandt *et al* 2014B paper to take note that Ptolemy arithmetically reduced (*AlmaJest* 5.12-13) his transit "observations" to declinations using a seriously false Alexandria latitude L, from Vitruvius 9.7.1 (plagiarism unmentioned at Swerdlow 2010 p.151), $L = 30°58'$ ($\arctan 3{:}5$; above, in fn 25), erroneous by −14′, which obviously is much-too-large for a regular outdoor observer, and also is in 17′ conflict with latitude $L = 31°15'$ which is indicated (by nulls) to be the virtually-correct value that was adopted by the stars' actual observer. Brandt *et al* 2014B's standard deviations σ were allegedly calculated bivariately; but, actually, after each E's was found monovariately, L's "accuracy" was found by averaging the remaining residuals, original but invalid procedure, making L's uncertainty too small by an ordmag, as referee-specified. *NB*: Had Brandt *et al* 2014B done the reverse, finding L's σ first, and then again meaned leftover residuals to get the other variable's (E's) "accuracy" similarly, the result would have been informationless zero! (See ‡4 §C11.) There are further oddities. *Ibid*'s eq.1 sets O−C equal to C. The paper's O−C graphs are rendered C−O. Two strange *JAHH* attempts to undercut skepticism by splitting Ptolemy's 18 stars differently from R.Newton (and, by-the-way: Ptolemy and Pannekoek and DR — i.e., **every**one else), innocently omit those low-declination-speed stars which are most-informative regarding L (dropping them simply because least-informative for E), and then, *average the rest — improperly weighted* (‡4 §C17 below). Summation: [1] In 1982 Rawlins' pure bivariate least-squares gave accurate values simultaneously for E and for x, with each's σ, also providing single-datum σ. [2] For the last [36^y], no historian-of-science has ever duplicated that complete solution. [3] Upon its receipt by *Centaurus* in 1982 and by *JAHH* in 2014, both journals instead published solutions seriously mistaken in part (due not to miscalculation but to invalid methods). Coincidentally, their authors defended Ptolemy, while Rawlins insisted on citing long-journal-suppressed evidence he didn't observe his suspect *Almajest* 7.3 declination data, namely: adopted-L's clash with reality AND with these declinations (see just above, here in this note), evidence crucial to the controversy, known for forty years, but (as also in fn 97) never-ever found in centrist journals. Same for Ptolemy's solar observations' 50fold closer adherence to old tables than to the real sky, re which three journals have severed correspondence (1983-2016) rather than print. In 1983, *JHA* lawsuit-threateningly cut contact over the 50-factor issue (Rawlins 1991W §B, 1991; *DIO 6* ‡3 §I, 1996; Rawlins 1999 §§E-F). In 2015, faced with DR's request to print the same 50-ratio, likewise-cornered *JAHH* unilaterally ended contact with *DIO*, www.dioi.org/oww3l.pdf, and in 2016, so we see from www.dioi.org/isb.pdf, *Isis* Editor H.F.Cohen suppressed the same 50-revelation by cutting *all* correspondence — including in regard to *Isis*' 2015-2017 unattributed (fn 10 here) appropriation of a *DIO* discovery and an undeniably twice-miscomputed (fn 13 here) 2016 attack on DR — on the irrelevant excuse that www.dioi.org/qjo.doc, our gentler version of the present paper (a separate matter from *Isis*' two **lead**-paper academic offenses, 2015-2016), was insufficiently self-censored. (Since *DIO* can't read editors' minds to do censorship to their exact tastes, *Isis* was invited [www.dioi.org/isb.pdf, like *JAHH* before it] to edit-out whatever it liked from the paper's political discussion, with the *DIO* promise of no complaint since we'd asked for such assistance. *Censorial journals instead inefficiently demand the author keep guessing what needs eliminating in order to finally satisfy "editors" too scared [or lazy] to edit*; ‡4 fn 13.) The intent of

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

Obsrvr	$E \pm \sigma_E$	Adop L	Its Error x	Actual $L \pm \sigma_L$	σ_o	σ_r
Timoch	-294 ± 11^y	$31°12'$	$-1'.8 \pm 2'.7$	$31°13'.8 \pm 2'.7$	$\pm 9'.0$	$\pm 8'.8$
Aristyll	-258 ± 10^y	$31°15'$	$+1'.0 \pm 2'.7$	$31°14'.0 \pm 2'.7$	$\pm 6'.1$	$\pm 4'.2$
Hipp	-131 ± 05^y	$36°08'$	$+0'.2 \pm 1'.2$	$36°07'.8 \pm 1'.2$	$\pm 5'.2$	$\pm 5'.0$
Anon	$+159 \pm 09^y$	$31°15'$	$+4'.4 \pm 2'.0$	$31°10'.6 \pm 2'.0$	$\pm 6'.0$	$\pm 5'.6$

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 astrologers' geographical tables' ordmag-1° L-errors; see ‡1 §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^y in E or even half of 1′ in L was effected.

I19   The main Hipparchos observatory's deduced[101] latitude, $36°08' \pm 1'$, was just-recently supported independently by 2016 realization of the mutually confirmatory reality of his computing reliability (§D3) together with his trigonometry tables' accuracy (§C14) which jointly bring 1″ precision to a 1997 exploratory finding[102] of brackets for observatory-latitude, $36°00'22'' < L < 36°09'09''$. 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 reconstructed[103] by:
[1] Thurston-inspired spherical-trigonometric transformation of the *Almajest* star catalog's southern ecliptical data, restoring originally-observed equatorial coordinates, then
[2] testing the declination data thus recovered, to learn which L produces (in implicit zenith-distances) the dramatic, R.Newtonian excess of 00′ endings expected[104] for raw 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

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such exilings is to intimidate pushback-scholars into silence. And, in current grant-hunger-corrupted academe, it nearly always works. Nearly. None of these three *purportedly-refereed* journals (vs above & fnn 4&97) encourages non-closeted investigation of *DIO* accuracy or of their own behavior. None of *Isis*' bunkered editors' emails on what ultimately became www.dioi.org/qjo.doc, nor *Isis*' 2017 March "referee report" ever mentioned the paper's history or science or mis-math or factual errors. Not what really matters anymore in history-of-science. (After demanding the paper's muting, Cohen finally sent an earlier unmuted version to his referee, ensuring the negative verdict he sought.)

[101] Table 2 here.

[102] Rawlins 1997A eq.1.

[103] Rawlins 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' observervory of the *Almajest* star catalog, see Pickering 2002A; also, importantly, Duke 2002C.

[104] Excess of 00′ endings (crucial fractional-endings test) discovered by R.Newton 1977 p.247. Cape Croisette: Rawlins 2009P §B.

ordmag 1° accuracy, before Ptolemy's fateful stades/degree scale-shift[105] (§I11) 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^y later, all 3 were checked[106] against Eusebius' long-neglected Eratosthenian Earth-radius $r = 40800$ stades, and 256000 was the only one of the 3 that fit this r. (How will Carman & Evans 2015's authors explain not mentioning this match, *to 1 part in a thousand*, when their own cited sources show they knew of 256000? See fn 10 above.)

I23   It was simultaneously found that Earth-radius r was the empirically primary datum, consistent with being based on Sostratos' non-astronomical Pharos method (§I24), which directly[107] 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 6/5 too high, while Poseidonios' $C = 180000$ stades, the other anciently standard Earth-size, is exactly 5/6 too low. It is an Occam-*DIO* dream-come-true to perceive that since horizontal light rays' curvature = 1/6 Earth's, atmospheric refraction would cause observed errors in C of 6/5 and 5/6, respectively, for two simple, clever, low-physical-labor never-leave-home methods[108] of measuring the Earth: the Pharos-flame method (6/5) virtually attested by Pliny 2.65.164; and the also-physically-easy (& obvious!) double-sunset method (5/6). So *DIO*'s refraction theory at once satisfies both Sostratos&Eratosthenes&Hipparchos' C (6/5 high), as well as

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[105] Rawlins 1985G p.265, taking an idea due to (*ibid* n.22) Gossellin 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 (‡1 §F). Or by 4/3 via Poseidonios' $C = 240000$ stades→180000 stades. Proposing Greeks organized to compare eclipse observations (fn 13): Rawlins *loc cit*, vs fn 90 above & Shcheglov 2016. Cape Verde Isles, westernmost known land, chosen as Marinos' 0° longitude (www.dioi.org/j501.pdf, §A5), to kill longitude sign-muffs like those (fn 97) cursing Shcheglov 2016. [Speculation 2018/7/15. Marinos = pseudonym, like "Ptolemy"? Or map-title from maritime Tyre?]

[106] $C = 256000$ stades 1st induced from a Nile map's latitude intervals: Rawlins 1982N pp.212, 214, 216-217; Rawlins 1985G p.259; Thurston 2002S p.66. For which C fits Eusebius' $r = 40800$ stades: just multiply r by 2π !: 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.

[107] Rawlins 2008Q eqs.2&21&28; eqs.14-15&17-18 for r as Sostratos' direct empirical measure.

[108] See *ibid* §A4 [a] for the Pharos-flame method and Pliny's semi-attestation of it. Double-sunset method: Rawlins 1979 or *Scientific American* 1979 May. Interval between times of sunsets seen from Pharos' top&bottom exceeded a time-minute, unmissably-enormous alert and gauge of C's size. (Elementary illustration-by-extremes that different results ensue for flame vs sunsets: Rawlins 1992V §A5.) To inerts needing direct attestation: among the many roboshunned matches cited here, the foregoing utterly original&successful atmospheric-refraction theory — tri-neatly solving the INTERMINABLY-contended ancient Earthsize mystery — cannot legitimately be ignored. But it is: fn 111 below. Unbelievably worse: fn 109! One recalls not only JHAD shunning of Diller (fn 25), but the case of L.Boltzmann's kinetic theory of gases, which E.Mach & others spurned because (though theory neatly fit evidence) *no one had ever seen a molecule*. Did this trigger Boltzmann's 1906 suicide (just ere vindication by Wilson-cloudchamber)? We don't know. What we know is: certain pols cited hereabouts wouldn't care. Past perhaps praying for history to repeat. Ever so vainly.

Poseidonious&*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.

I26   Though the atmospheric-refraction solution, explaining erroneous C, has been repeatedly published (fn 108) in the scientific literature — and featured (with generous credit to the author) for years in the 1990s as opening-page demonstration of using physics, in the long-standard physics textbook Halliday, Resnick, & Walker — historians-of-science either [a] cannot follow the math or [b] regard it as mere scientists' intrusion onto grounds best adjudicated by the real experts. Thus, the reaction, decade after decade is: no reaction at all in *JHA* and fellow captive journals, which doesn't help communal awareness[109] of the airbend theory that (to repeat) produces the only match to both attested C values and to 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 §§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*

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[109] On 2011/11/26, a long-persistent *JHA*-debtor (Rawlins 2009S fn 24) vandal, ever-*JHA*-protective, http://en.wikipedia.org/w/index.php?title=Dennis_Rawlins&diff=263054615=&oldid=256011510, determinedly — & achronologically — eliminated mention of *DIO*'s refraction theory even from popsite Wikipedia, http://en.wikipedia.org/wiki/Talk:Eratosthenes, just to help out centrists' ensurance that the public and academe remain totally — totalitarianly — protected from access to this too-successful solution. It was also removed on 2015/4/28 from Wikipedia's Stadion article [its n.8] and currently resides nowhere on Wikipedia. As desired, i.e., *consistent with hermetic communal shunning so thoroughly detailed here throughout.* (**NB**: No *DIO* posting on Wikipedia has ever dislodged a competing academic theory.) Since Wikipedia looks superficially like the prime potential leak in broad shunsnuffing of *DIO*, right-thinking's unsubtle vandal threatened anyone connected to Rawlins: http://en.wikipedia.org/w/index.php?title=User_talk:Dihydrogen_monoxide&diff=next&oldid197202695, http://en.wikipedia.org/w/index.php?title=User_talk:Consequencefree&diff=prev&oldid196285291, threat made-bad by for-years smearing the integrity of *DIO* Board members, world-standard Sun-Moon-planets-ephemeris creator, E.M.Standish, http://en.wikipedia.org/w/index.php?title=Dennis_Rawlins&diff=183177760&oldid=173784061, and world-acclaimed discoverer of Chiron & two Jupiter satellites, C.T.Kowal, http://en.wikipedia.org/w/index.php?title=Charles_T._Kowal&diff=224561000&oldid=186668652, & http://en.wikipedia.org/w/index.php?title=Charles_T._Kowal&diff=405442827&oldid=397176359, & on 2011/7/6. Diller expertise belittled, http://en.wikipedia.org/w/index.php?title=Dennis_Rawlins&diff=463307651&oldid=463306024, incredibly. For a decade, whistleblower Rawlins' Wikipedia biography has predictably been trashed by forces his researches have exposed, though not a single censorial deletion has been justifiable by inaccuracy. Trace the bio's History from a full balanced 2008/10/2 version (including DR-recommended *insertions of refs to his own mistakes as well as a long list of articles attacking his findings*) to today's stub, fixated on his passing involvement with CSICOP, & on newspapers (which Wikipedia Administrator Vsmith **persistently&censorially** insists are more reliable than an academic journal refereed by world-class scientists: fn 4 above). Among DR accomplishments eliminated 2008/3/10 to 2014/9/12-29 (besides those already cited at p.44): asymptotic planetary perturbation-amplitude (*MNRoyAstrSoc* 1970); ending British Neptune-discovery sham (*Scientific American* 2004 Dec p.98); solving BM55555, thus revealing Hipparchos' last Sun-orbit (§F2 above); epochs&latitudes of 4 ancient astronomers (*DIO* 1994); ocular basis for Aristarchos' expansion of the universe's radius to over 100 million Earth-radii (*DIO* 2008); revealing Archimedes' solar diameter was sexagesimal (2012); organizing the prominent citizens committee establishing Baltimore's internationally known 2004 Rachmaninov memorial, www.dioi.org/rar.htm, also composing its text; recent book and play, www.dioi.org/sha.htm, asking if C.Marlowe died 1593/5/30 — or debuted as Shakespeare 13^d later.

Physics, *Scientific American*, *Archive for History of Exact Sciences*, even a well-known physics-textbook, & currently is the cover article of the 2018 Aug *Griffith Observer*.

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

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

Question #3: Would it matter?

Hypothesis-discoverer Rawlins' own 1996 case[110] for re-evaluation (emphases in original) follows. *DIO*'s new PHYSICAL — not standard kneejerk-metrological — theory

> (ascribing both ancient [Earth-C] values' error to [atmospheric] refraction) simultaneously solves . . . . *both* the (very discrepant) Eratosthenes & Posei-donios values . . . ([through] a *single* value for the stade: the same . . . 185m value . . . found even in most dictionaries.) No other simple, coherent theory does so. [Classic Eratosthenian stade-scruncher J.Dutka][111] . . . claims that the reason for the 180,000 [stade] value's lowness is not known. He might've instead noted: [i] a coherent explanation exists for both figures, but [ii] he prefers the theory that explains only one of the figures [— Eratosthenes'].

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

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[110] Quote from Rawlins 1996C fn 47. Those who have spurned the 185-meter stade include F.Hultsch, E.Lehmann-Haupt, A.Diller, C.Sagan (more at ‡2 §N10). The *ad hoc* nature of the durably mythic runty "Eratosthenian" stade is obvious to most specialists, e.g., P.Gosselin, E.Bunbury, D.Dicks, O.Neugebauer, D.Rawlins, J.Berggren, A.Jones (more at *idem*). (Who creditably did not jump indis-criminately at a poor solution, but waited for a valid one to come along. So far so good. But now that *DIO*'s airbend theory is here, no historian-of-science is claiming the math doesn't work. Or that anything works better. Has the-catatonia got the JHAD-tongue?)
[The dwarf-stade myth is efficiently, consistently, bluntly, and utterly evaporated by Engels 1985 p.309. Sexagesimally-defined stade: Rawlins 2012T fn 2, self-contradicting the titular contention of Rawlins 2008Q (& note *ibid* §A4[a]) that early-Ptolemaic survey-based Earth-circumference determination was just legend. This can be seen as showing DR's poor judgement. or desire to learn. Or both.]

[111] Dutka 1993 p.64 cites Rawlins 1982N — whose App.A explicitly links 6/5 to lighthouse and 5/6 to sunsets — without (§I26) citing the paper's atmospheric refraction theory that explains these felicitous fits to the 2 respective ancient C-values at issue, & with no sign whatever of understanding the paper's physics. Strabo's arcs (where Earth-curvature is apt to a meridian circle of circumference 39870000 m): 5000 stades Alexandria-Aswan & Aswan-Meroë, each good to ordmag 1′ for 700 stades/degree: 7°1/8 + 7°1/8 = 14°1/4. (Rawlins 2009S §C notes Philo's solar work at Meroë, presumably for an imperial survey.) Testimony for early-Ptolemaic meridians in 60^{ths}: 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 §A4[a].) Dinsmoor 1950 pp.250-251, cites 5 ordmag-10%-disparate Greek stadiums' stade-long race-courses. (Shcheglov 2016 pp.696f lists even more.) The only post-Ptolemy-I course (Athens, rebuilt +143) 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, in stades: Aristotle 400000, Dikaearchos(?) 300000, Timocharis(?) 216000?, Sostratos-Eratosthenes 256000, and Poseidonios-*GD* 180000. Correct circumference C = 216000. (Meter = C/4/10/10/10/10/10/10/10. Nautical mile = 1852m $\doteq$ C/360/60 = C/21600 = 5/4 of the Roman mile.) Further speculation on the pharaonic stade's history is found at www.dioi.org/cot.htm#kchg. Relating attested meridian-60^{ths} to 185m is another JHAD-uncited completely original *DIO* revelation.

Yet the right solution is: [1] directly for radius (fn 107), not circumference; [2] geographical (§I23), not at all astronomical; [3] physical (§I26), not metrological.

I27 But whence arose the linchpin 185m stade? Before imperial standardization, stades varied ordmag 10% from locale to locale, the smaller among the early ones now naïvely, selectively, anachronistically used by Eratosthenes' mod-groupies to rig right-on correct-ness for his too-big C. In 2014, it was seen for the 1st time that the much-attested (fn 111) early 3rd century BC Greek rule of dividing terrestrial meridians into 60 parts (not 360) — step-one of C's potential **sexagesimalization, conventional Greek fraction-practice** — could've led naturally to the Ptolemaic empire's regularization of the "stade" by defin-ing it, parallel to our definitions of meter and nautical mile (fn 111), as C/60/60/60 = 40000000m/216000 = 185m. This is the best — the **only** — available scientific theory explaining modern-consensus-185m's Greek origin & durable adoption, which survived even influential Eratosthenes' soon-after insistence on a 19%-larger C.

I28 This inevitably-uncertain speculation implies that, c.300 BC, presumably while sur-veying the new Egyptian empire of Ptolemy I (Greek pharaoh −323-284), Greek scientists astronomically determined accurate Earth-circumference C, before dividing it by 60^3 to "define" the 185 meter stade (‡2 fn 49). Who earlier had the required science? Gradual-grade topography? (Camels?!) Was the measured arc along 29°.9 E longitude, Alexandria to Meroë's latitude (*nowhere interrupted by the Nile or sharp mountains*): 1578 km = (in 60^{ths}) 2^{x}3/8 = 14°1/4 = 8550 stades at 600 stades/degree, or nearly 10000 stades (Strabo 2.5.7, 17.3.1: fn 111 here) at later-standard 700 stades/degree? — accurate to ordmag 1′ or 1 nautical mile. Was the hypothetical survey supervised by contemporary scientist Timocharis, demonstrably expert (fn 99) in 1′-accuracy latitude-fix via ringed instruments?

I29 For over 100^y, at least from H.Berger, scholars have wondered if the early overlarge 300000 stades Earth circumference C, cited in Archimedes' *Sandreckoner*, was due to Dikaearchos (c.−300). In 1994, *DIO* showed[112] that if Dikaearchos measured sea-horizon dip from atop conveniently-seaside Mt.Pelion accurately (1°1/10) & computed C from it, then his over-estimate of Pelion's height as 10 stades would (in ignorance of quantifiable atmospheric refraction) have produced C = 300000 stades within ordmag 1%.

I30 *DIO* produced hitherto-unperceived & thitherto-uncited physical evidence that the Galactic Equator appeared on ancient Greek celestial globes: www.dioi.org/fff.htm#phod.

I31 Kallippos' −329/6/28 Summer Solstice was his famous calendar's epoch. Modern discovery of the event's hitherto unknown hour unexpectedly happened in connexion[113] with 1985 realization that Kallippos' 365^{d}1/4 yearlength ought to have been found by him from the gap between his solstice & Meton's. Meton's calendaric Solstice-hour was Athens' day-epoch, 18^h, for the day **containing** the solstice, not its exact time (Rawlins 2018U §J4), thus −431/6/27 3/4. So, adding 102 Kallippic years, or 37255^{d}1/2, to that date reveals Kallippos' epoch as −329/6/28 1/4, dawn, which is late by 3^h, thus accurate within traditional 1^d/4 precision. Moreover, the New Moon at 4 AM was only 1^h after 3 AM solstice, a once-in-centuries ideal conjunctive epoch for his lunisolar calendar. Kallippos induced his yearlength from division of 37255^{d}1/2 by 102, finding (as it happened) exactly 365^{d}1/4 days. His solar motion was codified into his famous Kallippic 76-year cycle of four 6940^d Metonic 19^y cycles minus 1^d, that is, 365^{d}1/4 per year. Due to Meton's −17^h truncation error & his own Solstice's +3^h error (interval's net error +20^h), he accidentally arrived at history's 1st Julian calendar, nearly 3 centuries before Caesar's Sosigenes.

I32 Note: Superscripts occasionally used here & below: d = days, h = hours, m = timeminutes. Lunar: u = synodic months, v = anomalistic months, w = draconitic months. Solar: g = anomalistic years, y = tropical years, y = sidereal years, K = Kallippic years. (Degree-remainders merely signify 360^{ths}.) Tropical-years here can refer to real ones or the Metonically-defined "tropical" (or Easter) year 235^u/19.

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[112]*DIO 4.2* (1994) §M & fn 22. Dikaearchos' 10-stades-high Mt.Pelion, Pliny 2.65.162.
[113]Yearlength 365^{d}1/4: Rawlins 1985H. Kallippos solstice-hour-epoch: *ibid*, & Rawlins 2018U eq.2. [Wikipedia's Callippus entry falsely implies Meton-Kallippos's 1^d difference relates to precession.]

I33   Sourcing Ptolemy's final lunisolar ratio,[114] $105416^u = 8523^y$, occurred 2 decades ago (all 10[!] digits exactly elicited) by test-exploring Greek awareness of the 800^y sidereal eclipse-cycle nest (1/5 of 800^y cycle attested: Geminos 8.40-41): solution, awareness, & nest not suspected ere Rawlins 1996C eq.31. (Sidereal year accuracy: *ibid* fn 110.) Royal Muffia Cavilliers have produced no math error or alternate solution since. Predictable result (see Rawlins 1996C's title and boxed 2013 statement atop its p.2): permanent silence.

I34   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 ratio's terms, **just the way Ptolemy at *Almajest* 4.2&6.9 explains determining months synodic, anomalistic, & draconitic**. Notably, no matter where, *over a 400^y span* (3rd century BC to 2nd century AD), the pairs' latter eclipses are located in time, all the prior ratio-solving eclipses turn out to be from *the very same century*, the **thirteenth BC** (§I36). 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 shunninity, frustrated by inability (like §I33) to find math error or *alternate eclipses to show non-uniqueness*, has been forced to just datalessly scoff (chief sneerleaders: A.Jones and D.Duke) at the very idea of such remote eclipse records as ridiculous *a priori*. But perhaps neither snickerer has heard about non-cult[116] scholarship by Johannes Koch who had already (10y earlier) estimated Babylonian observations' nascence as about −1350? Surprise realization that Hipparchos' famous 600^y lunisolar tables effectively went back just that far only occurred[117] in 2015.

I35   So we should gauge the proposed eclipse-ratio method by comparing it to what may be verrrry 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-10^6, 3 times out of 3, *eliminating false nearby period-ratios* (§I37). 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&6.9**. No other credible (fn 119) method, attested (or unattested) has math-reproduced ANY of the numbers sought, while *DIO*'s proposal has done so for ALL 24 digits precisely — that is, all six 4-digit components — on-the-nose in each case: see www.dioi.org/thr.htm#cqtp.

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[114] *PlanHyp* 1.1.6 (Heiberg 1907 pp.78-79 or Neugebauer 1975 p.901 eq.3): $105416^u = 8523^y$. Solved: Rawlins 1996C eqs.20-31. Thanks to K.Moesgaard for a perceptive correction.

[115] §I36. The admirable exception to Hist.sci ignoring ancient monthlength accuracy: Pedersen 1974 pp.164&424. But he does not realize how such accuracy was achieved, nor does he go on to challenge the anti-empirical orthodoxy we saw at fn 8.

[116] Moesgaard 1992 p.474. Initial Muffia tactic vs R.Newton & *DIO* was non-citation. But *Isis* Editor Margaret Rossiter's publishing *DIO*-respecting Thurston 2002S defied the 30y shun, inspiring (*what else from pathological unregenerates*?) **DOUBLEshun:** [a] Thurston's swift ever-exile from *JHA* (www.dioi.org/pm3.htm); [b] *DIO*-citations' end in AAS-HAD's *Newsletter* & [c] *Isis*' Cumulative Bibliography; [d] during Thurston 2002S's refereeing (2000), the usual indiscriminate (fn 66) unrefereed anti-bodies prepared for launch: Schaefer 2001 (Pb), Schaefer 2002, Jones 2002E (2nd to Pb), Duke 2005T, Duke 2008W (Pb), Jones 2010B (2nd to Pb), as pols outdid each other (to squash #1 blackballee *no matter how*: §§B-G), all now on *JHA*'s *certified*-Premier (fn 42) board. Re *JHA*'s prior villain, we quote from ‡2 fn 28: "Rewards handed out to those who attacked the R.Newton satan include *JHA* boardship (R.Newton 1991 fn 2) & a MacArthur for miss-man [fn 96 here] Swerdlow. (It's hard to find good help anymore.) . . . . maid-men Evans&Schaefer were elevated at *JHA* not long after their massive bungled 1998&2001-2002 attacks on Rawlins. (The unsubtlety here may actually be deliberate.) Selecting boardmembers [thusly] will damage mean-IQ atop *JHA* for decades to come."

[117] For this recent shock, see www.dioi.org/thr.htm#rbkv. Re Hipparchos' 600^y tables, see Pliny 2.9.53.

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

$−1291/11/23$ vs $−280/1/16$ & $−1273/12/05$ vs $−262/1/26 \rightarrow 6247^u = 6695^v$ (SystemA),
$−1244/11/13$ vs $−140/1/27$ $\rightarrow 5458^u = 5923^w$ (Hipparch),
$−1200/07/11$ vs $125/4/05$ & $−1189/06/12$ vs $136/3/06 \rightarrow 3277^u = 3512^v$ (*PlanHyp*).

(Latter dates: earliest firm System A text is $−262$ [J.Britton 1999 n.6; Rawlins 2002B §E6]; $−140$ [Rawlins 2002H §C9] and $+125$ & $+136$ [Rawlins 2003P §C] eclipses are attested.)

I37   Again: mere integral division is *DIO*'s twice-ATTESTED eclipse-cycle "method" (too fancy a term?) of exactly reproducing all 24 digits. In the 1 1/2 decades since these supersimple *DIO* solutions' 2002-2003 debut: no historian-of-science has publicly engaged a single one's science. Nothing beyond a rigid clique's continued traditional insistence on its vaporous theory that UNATTESTED laborious Babylonian analysis of poor lunar horizon[118] data couldamusta produced such accuracy — if only enough[119] data were averaged! (This bizarre notion came inevitably out of the Neugebauer-Babylonianist cult, ever-clinging baselessly[120] to its sacred tenet that Babylon gave rise to high Greek astronomy.) Naturally, no numbers are provided[121] to show how such a fantastic reconstruction could: [a] repeatedly produce HYPER-accurate results, or [b] find the draconitic month at all,[122] or [c] distinguish

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[118] See Huber 2000 for the variety of systematic errors infecting Babylonian crude horizon data (*use of which for month-gauging is totally unmentioned in any ancient source*) plus the brevity of the Babylonian data's time-base (2.2 centuries). Contrast to *DIO*'s uncomplicated ancient-standard eclipse-cycle-ratio method, clearly attested (§I34) for finding Greek monthlengths from eclipse-pairs separated by 3 1/2 or 6 centuries, or proposedly and fittingly (§I36) 10, 11, even 13 centuries.

[119] See www.dioi.org/thr.htm#cvpc, and Britton 1999, for his theory of System A-year origin, and www.dioi.org/thr.htm#rgbb, for his clique-pique at Rawlins 2002B's simple exact-fit to same: just halving an integral eclipse period-relation! P.Huber dreams that ancients merged non-integral short-period relations to create ordmag 1000^y integral ones (contrary to obvious common-sense, as well as ancient sidereal-vs-synodic records: ‡2 §N15), like Neugebauer 1975 reading into 3rd century AD *containment* testimony (*ibid* p.321) such an imaginary *construction* (*ibid* p.322). But that sort of origin [A] is, unlike *DIO*'s *Almajest* 4.2&6.9-based method, unattested (*ibid* p.555 finds no integral or 1000^y ratios); & [B] wastes long-time-base's accuracy-advantage (from dividing both endpoint-errors by a huge integer), *known to every astronomer who ever gauged celestial periods* (from Mars to pulsars) in the real science world. Ptolemy knew better: *Almajest* 4.6 uses long time-spans, so deduced celestial mean motions "will be valid over as long a period as possible." See Toomer 1984 n.18 at *Almajest* 3.1.

[120] Indicting specifics' sheer breadth: www.dioi.org/j129,pdf, Rawlins 1991W §§E-F, esp. §E3 & fn 73. Hmmm. Do pan-Babylonianists never-ever wonder just why: Babylon had no Aristarchos? No Archimedes? No Apollonios? Not even a Seleukid Euklid? No trigonometry. No transit data. No observed solstices. No vertical instruments. No knowledge of Babylon's latitude (‡2 §N13). No serious astronomy until after Greek conquest (§G5)? Was Seleukid Babylon's gift to science its fortunate preservation of Greek-astronomy glimpses (e.g., §F3 [1]) on durable clay, not fragile papyrus?

[121] *DIO*'s theory, which easily & EXACTLY (fn 119 above) solves System A's monthlength (Rawlins 2002B eq.2) — and is extrapolated at *ibid* p.19, to *DIO*'s General Theory of Ancient Cyclicities — is not cited at all in Britton 2007 p.124 (System A ratio misprinted), though the same *DIO* issue it appeared in is inimically cited at Britton *op cit* n.66. Similarly, defense of Ptolemy's star-catalog authorship by Pedersen 1974 (pp.249&258) omits citation of Delambre 1817's simple contrary proof, though citing elsewhere (*ibid* p.109 n.5), for another purpose, the very Delambre page on which said proof appears: Rawlins 1982C p.362. More deliberate non-citations at fn 10, & in above chapters (esp. §G11) on **data-tampering**, worshipper-"historians" just following hero-Ptolemy's example, after all!

[122] Babylonianist lunar-six proposals for determining the anomalistic month can't work for the draconitic month — therefore a different farfetched explanation needs concocting. Someday. Meantime, *DIO*'s — & Ptolemy's! (fn 119) — uncited (e.g., fn 121) eclipse-cycle method www.dioi.org/thr.htm, solves both. Exactly. A parallel case: to explain Ptolemy's huge solar errors, defenders pushed the theory that they were caused by atmospheric refraction and/or mis-setting of the Alexandria ring (fn 70). Such might have (but didn't, as reluctantly proven by Neugebauerian John Britton 1992 p.44) roughly explained away his equinoctial errors, but could never have explained his solstice-error; whereas the

among almost-as-accurate proximate ratios (www.dioi.org/thr.htm#cpcc), or [d] explain why each solution emerges as a ratio, a glaring feature of ALL pre-Ptolemy adopted lunar motions, which by contrast to orthodoxy is accounted-for perfectly inevitably by the eclipse-period-ratio solution. Jones' blindered private rejection[123] (by subsequently-undercut[124] reasoning) of the *DIO* draconitic solution, flees all of the overkill-numerous, solid, unambiguous evidences consistent with said theory, particularly its PRECISE match to Hipparchos' draconitic $5458^u = 5923^w$ ratio (*Almajest* 4.2) by pairing an early apogee eclipse, −1244/11/13, with his Rhodos-observed −140/1/27 eclipse, *the very same perigee eclipse* which he uniquely had also previously (*Almajest* 6.9) paired with a less early apogee eclipse (−719/3/8) for exactly the same draconitic purpose, with inferior result — inevitably, due to shorter 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   §I36 presents three perfectly fitting 2002-2003 *eclipse-solutions* to Greek adopted monthlengths. Why shunners' 15^y-impotence in finding DR-errors? Or alternate solutions? Answer: **there IS no other umbral lunar eclipse-pair** whose integral-months ratio precisely, proportionately, directly yields (by *Almajest* 4.2&6.9 method) any of §I36's three attested, never-before-solved Greek integer-ratio motions, with: both eclipses visible in Greek-Babylonian region, latter eclipse within 50^y of discovery-date (c.−262, −140, +136, resp), earlier eclipse not ere *Almajest* 4.6's −720/3/19 (*oldest eclipse-data historians-of-science accept that Greeks possessed*: Toomer 1984 p.166 n.59). To pioneers who undo the above negative assertion by finding, before 2020/1/1, real umbral lunar eclipse-pairs directly solving the ratios in question (under above specs, incl. hist.sci's own 721 BC bound), *DIO* will gratefully grant: $10000 for 3277^u/3512^v; $20000 for 6247^u/6695^v; $30000 for 5458^u/5923^w; *$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 fn 2) without a professional-survival priority — nay, necessity — of treating archons with an attitude of nondissent, even supplication, to allay (non-imaginary) fear of being unpublished or outright blackballed, as if such etiquette-issues outrank (§H5) 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 determine whether historians-of-science can ever grasp the empiricism & brilliance of Hellenistic science. [f] Essential, genuine neutrality and curiosity (hardly compatible with a cult's insistence on aggressively protecting sacred viewpoints for decades on end), enhanced by willingness to hypothesize-explore — ever subject to evidence-congruity (e.g., fnn 16&83 vs fnn 20&25&40). [g] Philosophy-of-science&common-sense Occamite

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theory of fabrication explains both and to the *Almajest*'s 1^h precision. Therefore, in each case (lunar or solar): *which approach would Occam prefer*?

[123] Half-dozen evidences for Hipparchos as author of $5458^u = 5923^w$: Rawlins 2002H §C.

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

weighing of competing theories by such criteria as simplicity (§H above), probability's relation to confirmation (‡2 §N15), minimal hypotheses (§D & fn 122), fruitfulness (§§C3[b], E, & F), predictivity (§§F & G). Instead of by herd-grantmanship.

J2   What simple, Occamly-coherent theory explains the serpentine communal behavior detailed above? — targetted non-citation, desperately indiscriminate "alternate" solutions to demean solid achievement, dishonest smearing, data fudgery and even destruction? Answer: shunning[125] (aimed at anyone upsetting archons' tenets or pretensions) — the single spare hypothesis that ties together all of the foregoing superficially mysterious, deeply inexcusable offenses against ethical scholarship. No use denying it: jihad-shunning of Diller, Newton, Rawlins, and *DIO* is common knowledge throughout the JHAD circle. (Can't blame on Rawlins' acknowledged shortcomings, for sedate gentlemen Diller, Newton, & Thurston [www.dioi.org/pm3.htm] were shunned from 1934, 1968, & 2003, resp, most of them years before Rawlins barged into The Ptolemy Controversy in 1976. Even highly expert Britton felt he'd been somewhat exiled, as he imparted to Duke, for honestly owning that Ptolemy's solar errors were not innocent: fn 122.) But, given the above consistently weird incidents, one needs no inside dope to smell heresy-banishment, along with the temptation that attacking the most archon-resented heretics (*no matter how shoddily*: fn 66) will bring advancement to any careerist willing to stoop that low.[126] What scholar ever lost immediate status by adding to an establishment cult's sacred crank literature? Concluding:
[1] Outside the clique here examined, can historians-of-science recall any cases like the above-cataloged rear-guard mass-vandalism of data and thus of potential historical progress constructable thereon? (But, then, have there previously been unanswerability-panics of the magnitude that R.Newton & *DIO* represented?)
[2] For the ancient astronomy field, has Curtis Wilson's cleansing hope (fn 59) been snuffed?
[3] Greek astronomy will ever rank uniquely as man's 1st foray in precise predictive science. Its lofty place in human history need not be desecrated by archons' mundane limitations.[127]

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[125] Jihad-shunning (longstanding: above, fn 109, & Rawlins 1991W fnn 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 academe. Like marriage, shunning is easy to commit, hard to end (‡2 fn 34): usually originating in archonal rage at intellectual rebellion, in fields so degenerate that Disrespecting archons and exposing pretensions are worse crimes than the counter-crimes of smearing, shunning, deceiving, stealing, doctoring, censoring, *en route* to effecting decades of knowledge-subtraction. The exiling entity commits to the banned's worthlessness without anticipation of concomitant risk: what if the shunnee then produces valuable knowledge? The shunner can never admit banishing valid scholarship. So either [a] he loses faces by unshunning owning to a mistake or [b] omertà-fakes (e.g., fn 66) the exile's vindicated work as being just as worthless as Infallibly decreed at the outset & bars non-denigrating citation. How many image-protective archons ever chose option [a]?

[126]Whenever weighing cultists' attacks on *DIO*'s frustratingly reliable inductional and computational achievements, it may seem difficult to distinguish between [i] those meant to impress archons with toadily-awesome 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 trashing data (§§C-G above)? Further on the theme of openness&honesty in the history-of-science world: [a] In 1992, complaint from sometime *Isis* boardperson R.Kargon caused temporary cancellation of Johns Hopkins Univ's Library subscription: *DIO 2.1* p.2. [b] At the cozy Muffia 1994/5/6-8 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.

[127] History-of-astronomy's present #1 archon Evans joined the unHoly Trinity ruling the *JHA* less by quality of research (though that's not negligible: fnn 9&34, and www.dioi.org/cot.htm#gjne) than by ingratiatory loyalty to *JHA* power-operator-editor & AAS-HAD co-founder O.Gingerich. Due to these less-than-entirely-academic factors, upon Evans' 2013 elevation to *JHA* Editorship (Rawlins 2009E fn 7 had predicted years in advance that Evans was heirhead-apparent), no audible historian-of-science thought it mattered that (among other ethical lapses: fn 10) he continues decades of ducking explanation of his laughable 1987-launched & 1998-suppressed lunar observation of 1981/7/16 (fn 11) and so lets

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Thanks to all who've launched an explorer into a 9th decade of a life of unfenced curiosity.
Blessed by feliciciously often-confirmed inductions of the distant future of the distant past.

stand indefinitely-uncorrected his own miscalculated-backfired evidence, not retracting the slander of Greek science it was adduced for, in ever-orthodox support of the 2 prime inter-related field-dominant clique-myths regarding Greek astronomy that we've been discussing hereabouts: [a] Ptolemy's honesty (fn 9); [b] Greek data-ineptitude's unattested-but-alleged consistency with allegedly-normal science (fn 8; also Evans 1998 p.209) that allegedly-retained only theory-fitting data: i.e., forcing all Greek astronomy to fit a *faker-astrologer*; then, pre-post-erously, with Ptolemaic evidential circularity, using that very model to prove his normalcy, a truth-inversion warp that's ruled the field for most of **a century**.

‡4   Ptolemist Enormity

Ptolemy-Defense Cult Lays Yet ANOTHER Egg
On Own Already-Unwipeably-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 archonal apology-defenses of Claudius Ptolemy, sacrosanct mascot-astrologer of the American Astronomical Society and its HAD: author of astrology's bible, the *Tetrabiblos*; science-fabricator; & *Almajest*-perpetrator. The spectacular result has been one of history's grandest compilations of establishment pseudoscience literature, all accomplished in the service of attempting to save the reputation of a "scientist" icon who was not a scientist at all, but (§A below) just a mathematician who faked science. Badly. Among Ptolemy's numerous clumsy AlmaJests [the silliest on scintillating display here in ‡2] were, e.g., [a] 2 different dates (37 days apart!) for the same Venus maximum-elongation, [b] plagiarizing Hipparchos' star catalog, [c] impossible-for-regular-observer ignorance of his own city's latitude, [d] 4 alleged solar "observations" which were (as no historian-of-science denies; or admits) many times nearer Hipparchos' old indoor tables than to the outdoor sky's actual Sun.

The *JAHH* paper in question, J.Brandt, P.Zimmer, & P.Jones (below known as Brandt *et al* 2014B), attempted mathematical analyses of the *Almajest*'s stellar declinations, observed by four successive ancient Greek astronomers over nearly half a millennium. The journal and authors contend that the dozen *Almajest*-contemporary declinations could be Ptolemy's observations, never warning the reader that zero evidence is provided to establish that claim, while simple, definitive, long-published, referee-urged proof to the contrary is below shown (§C5) to have been deliberately omitted. The paper's "bivariate least-squares" statistical analyses were not bivariate and thus didn't exactly find any least-sums S_o of residual-squares, as is also demonstrated below (§C23). While observers' epochs E are nearly right (but not new), attempts to find their geographical latitude-errors x are revealed as grossly misguided, at a primitive level (§§C9&C12), though referee DR provided, ahead of publication, accurate x (& standard deviations) for all four of the ancient astronomers being analysed, solutions which could've been (but weren't) crudely verified by elementary arithmetic, as will be shown here (fn 34 or §C23). Our discussion's bluntness derives from the fact that, though Brandt *et al* 2014B is politely written, its knowing evidential omissions cooperate in trying to grant eternal life to an establishment myth — Ptolemy as Great Outdoor Astronomer — that rolls on, decade after decade, persisting only because the American Astronomical Society doesn't care that its Historical Astronomy Division is deeply invested in a pathetically obvious historical lie, viciously (fn 4) defended by those JHAD archons who long ago mistakenly decreed Ptolemy "The Greatest Astronomer of Antiquity"[1] and thus have faces so at risk of megga-eggitudinal disgrace that they must forever encourage pseudo-science-for-The-Cause of forever-pseudocontroversy, cult-obediently incapable of admitting that any skeptic has ever made an indubitable contribution to knowledge. Below, at §B, the most recent misfire (Brandt *et al* 2014B) is put into the context of decades of like uniformly baseless mobaganda (though those interested only in 2014's mismath may skip straight to §C), which has by now so brain-dirtied the mass of non-specialist historians that writing in opposition may be little more than preaching to the perverted.

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

Text for the Day:

In the 1946 Alfred Hitchcock film *Notorious*, German spy Claude Rains suddenly learns he's oops-unknowingly been connubially sleeping with a U.S. spy, and realizes that his fellow German spies would snuff him yesterday, if they discovered his security-breach. So, he seeks advice from his wise mom, who consoles him by pointing out that it would never even enter their heads that their own choice as the ring's most-exalted chief could possibly ever commit the ultimate espionage blunder. As she comfortingly puts it:

You are protected by the enormity of your stupidity.

The point might be kept in mind by observers of the decades-ongoing spectacle of the history-of-ancient-astronomy field, whose most eminent journal and most prominent society — in tandem with a MacArthur Fellow and a Harvard prof serving as untruthbound propagandists — spread behind backs (fn 18) the slander that no-one but **a CRAZY**[2] person could suspect dishonesty of the history-of-ancient-astronomy field's ultimate hero, ancient astrologer Claudius Ptolemy, who 4-times-out-of-4 reported observations of the Sun that were undeniably but captive-journal-unprintably 50 times closer[3] to Hipparchos' 280^y-old indoor tables than to the real Sun. The prime forums perpetrating this fantastic but profitable joke on academe and the public, **for consecutive decades**, are the *Journal for the History of Astronomy*[4] and the American Astronomical Society's knowingly[4] unsupervised Historical Astronomy Division (HAD) — which we shall refer to as the "JHAD" combine. Seemingly incredible fact of the last 4 decades of the Ptolemy Controversy: **not a single published defense of Ptolemy has ever been valid** and most have not been particularly smart[5] or honest,[6] as we are about to see again&again below. But as with oft-crafty Rains, the perpetrators of this fantasy-literature are protected by the very incredibility of the idea that such ultra-eminent forums and scholars could seem so stupid. (Also invisibly back-stabbing, slanderous, & deceitful: fn 18.) The gulf between the pompous mask and the dumb arguments that are insisted-upon (by people some of whom are normally as smart as Rains) is so beyond the comprehension — the very universe — of emotionally normal onlookers & pressfolk, that the latter have not, cannot, will not believe the solid reality of what has been the dispute's history, even though oft meticulously documented in *DIO*.

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[2] Prime smear against dissent is Insanity (as with media on pols' heresy) by megafunded establishment-polishers & darlings Gingerich (fn 16) & MacGenius Swerdlow (fnn 4&18, ‡2 fn 35), scientifically-challenged (‡2 fn 8) MacG even mirrorlessly calling JHU-physicist & JHAD-satan Robert Newton a Velikovskian "crank and a con-man": www.dioi.org/j113.pdf, *DIO 1.1* ‡3 §§D2-D3.

[3] Nobody disputes the 50-to-1 indictment. But no Reputable Forum (including even popmags, newsrags, & toob) dares broadcast such heresy-supportive truth, either. The rigid decade-after-decade policy: *hide it from the public*. (Given the power-secretarial state of the nation's Free ₛₙᵢ𝒸ₖₑᵣ Press, that's not even a challenge. Consider: *would archons behave as described here if they thought there was even a 1% chance the press would expose it?*) E.g., in 1983, the *Journal for the History of Astronomy* so insisted on (at-the-last-minute, without-warning) deleting the 50-to-1 evidential crusher from a projected DR article, that the paper was suppressed by *JHA*, being finally published by DR 16^y later: original unexpurgated text at www.dioi.org/j913.pdf, Rawlins 1999 §E. Understand the attitude: you the public just can't be trusted with certain central facts, because you might "misinterpret" them and start believing something Unapproved. (Similarly at www.dioi.org/vols/wi0.pdf, *DIO 18* §§T13-T16.)

[4] DR has asked AAS to monitor HAD's "unprofessional" (Schaefer 2002 p.40) behavior: 2002/10/2, 2015/12/29, & (www.dioi.org/jcx6q.pdf, email) 2017/6/26 (no reply) transmitting photographic proof of dishonest archonal smearing: www.dioi.org/pm1.htm, vs www.dioi.org/pm2.htm, tactics long known to *DIO* recipients, a class which includes the AAS, whose chief in 2017 joined the deaf&dumbers.

[5] Some authors may be able, but this breed of apology never quite is. Evidentially countering such feeble and comically self-contradictory (Rawlins 1992V §§C31-C32) effusions is not a serious challenge ("like shooting fishstories in a barrel of monkeys": www.dioi.org/j13a.pdf, *DIO 1.3* ‡10). And that is exactly why Ptolemists eschew (‡2 fn 52) risking rational debate with DR, written (www.dioi.org/deb.htm) or spoken (‡3 fn 5), preferring character-assassination-stealth's bravery: fn 4.

[6] Ptolemists' integrity-level (e.g., §B6 below) generally shows up less in the (perhaps-unintentionally) deficient original paper than in subsequent failure to acknowledge its thesis-gutting flaws.

A   The Shy Archon Triggering the Present Paper: Politics vs Science

A1   In 2011, DR belatedly[7] 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 referee it, though WO didn't mention that its progress was already so far along towards publication that serious changes appear in retrospect not to have been feasible at the late date of *JAHH*'s request. (Not the 1st time [e.g., Rawlins 2008S fn 42] Ptolemists 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 interests) not to take all pains to provide the most accurate 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 *JHA*ish inability (increasingly typical of the whole ever-less-scientifically-skilled[8] 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 article 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[9] 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.262]".) So DR responsively submitted a paper, "Ptolemy's Fraudulence" (‡2 above), to the *JAHH*, whose chief, W.Orchiston (formerly established in Oztrollya, like *JAHH*, but lately transplanted to Thailand) turned it over not to a specialist in the relevant science

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[7] When asked to send Brandt his 3-decade-old star-declinations ms (later slimmed, revised, augmented with new discoveries of absolute latitudes, and published as Rawlins 1994L), DR took the time to profitably review his 1982-1994 conclusions, sending his further-revised 2011 thoughts in a letter, www.dioi.org/bjr3g.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 1994L had already concluded that +159 was the Clean Dozen's epoch): for epoch +159 (unlike for +137) *the split between Clean Dozen & SickSix stars was overlaplessly clean*: §C17. (But Brandt *et al* 2014B didn't cite any of this.)

[8] 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 50y 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.

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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[10] 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[11] including accusing DR of an "apparent need to disparage those with different views". This from a cult which has for a half-century repeatedly (and reliably-always behind-the-back) smeared, as insane, anyone differing from its own reality-detached view of Ptolemy — a genuine, mentally-disabling insanity which The Leader is now at the last almost[12] alone-in-the-bunker with, outside of (publicly) loyal J.Evans, J.Brandt, & possibly B.Schaefer. (Ptolemy's many 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 be, 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[13] editorial revisions, would suffice to dodge publishing archon-loathed heresy.

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 biassed chumminess with its mentor (who has loathed and libelled DR for decades). So [A] DR unexpectedly refused to enter into any argument over content, granting full veto power to *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

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[10] Gingerich's private ref-reports on DR's work can't resist personal remarks having no place in such. (Too remote from principled stands even to recognize one, OG actually claims [*DIO 2.1* ‡3 §§C8&C12] DR *wants* to be shunned.) DR's atheism heaps extra aggravation upon this self-described "practicing Christian" of the Mennonite cult. Students of the psychology of hate might profitably investigate this 40y obsession: bizarre details (& Cardinal Manning's perceptiveness) at ‡2 fn 5.

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

[12] One could add Swerdlow to the listlet of diehards, but not from admiration of Gingerich: fn 22.

[13] In retrospect, it was predictable that *Journal of Astronomical History & Heritage* wouldn't publish a paper showing its recent 2014 BZJ article was false in claiming Ptolemy's star-observership. Editor Orchiston's demand for revision looked like the start of an endless, wasteful game of never finding DR's requested self-censorship sufficiently adequate. (The theory that the paper was doomed from the outset is verified by test in the next-last paragraph of www.dioi.org/oww3l.pdf, unsurprisingly.) Since a durable cult lie (Hoskin to Thurston 1986/9/5) is that DR is "impossible to deal with" (contra DR cooperation with, e.g., *Polar Record* [Univ Cambridge] & *Griffith Observer*), it was presumably believed that this approach could kill the paper while never appearing to censor anything. Note that if the editor objected to parts or words (*he, not DR, knows his tastes in this regard*: ‡3 fn 100), he need only have taken up DR's 2015/9/30 suggestion, at www.dioi.org/jav9u.pdf, to strike such (isn't this what competent fields' editors do & are for? — the job would've taken ordmag an hour) & sent the revision back for DR's OK; but, then, what if DR had replied "Done"? Fixers keep their plans flexible.

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 [‡1] *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 to evaluate, as thoroughly demonstrated below. Also: some among the authors may have found themselves rushed or only partially consulted, and out-of available space for their paper, as a deadline approached — and out of time, having waited too long to call in expert advice. (But this doesn't excuse the central omission: Ptolemy's $-14'$ error in latitude L, emphasized in www.dioi.org/jar3g.pdf, DR's 2011 letter to Brandt, for its fatal contrast with the trivial L-error shown by the data.)
[b] Even while 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 neutral referee.
Finally, DR emailed a friendly 2016/2/29 letter, www.dioi.org/oww2t.pdf, to WO and learned of his block — an arbitrary act which at last nakedly unveiled a proud new addition to JHAD cowering-archondum (§A3). This confirmed that the usual heresy-containment info-control (standard for the last half-century: ‡2 fn 35) was being governed by the priorities of, in this instance, two colluding politicians. A final 2016/3/21 *DIO* letter (successfully sent to WO by alternate email address), www.dioi.org/oww3l.pdf, again tried collegially to allay hypothetical shyness (about editing a DR paper) by pointing out that DR could hardly complain of any WO deletions if he'd asked for them! As DR had, in his 2015/9/30 letter. The new letter ended with an easy test (fn 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 fleedom to WO. (See www.dioi.org/isa.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

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

Ptolemist archon: "you mean she isn't?"

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 extreeeeemely peculiar cause of protecting the reputation of a fellow pseudo-scientist (and grant-cow), the ancient Greek astrologer Claudius Ptolemy? — known for centuries to informed scientists as the most notorious liar in astronomical history. *JHA*'s longtime #2 official, Harvard's Astronomy 101 teacher & deft Disraeliesque trowel-wielder Owen Gingerich, has repeatedly billed[16] this ancient faker and superstition-peddler[17] as "The Greatest Astronomer of Antiquity", claiming that all who question this eminently-sensible-to-him proposition *are the insane*[18] *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 archonbishops, 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 $1^d 1/2$! — or a degree and a half, which is over **FIVE TIMES** the angular distance from the Sun's center to its limb (edge) — this, while his report agreed with Hipparchos' indoor tables to within $1'$ or a fraction of an

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[14] AAS-darling Gingerich 1980 p.264. Quoted at www.dioi.org/j43f.pdf, *DIO 4.3* ‡15 fn 43.

[15] Schaefer 2002 p.40.

[16] Gingerich 1976, Gingerich 2002. Disraeli (L.Strachey *Queen Victoria* 8.3, 1921 [Harbrace pbk p.244]): "when you come to royalty you should lay it on with a trowel." Gingerich summed-up succinctly: *DIO 2.3* ‡6 §F. To enjoy Rob't Peary's like supplications, see www.dioi.org/cot.htm#dtrw.

[17] Ptolemy authored astrology's bible, the *Tetrabiblos*, the tripe in which has got to be (but usually isn't) read if one wants a measure of how seriously to take Ptolemy as a scientist. See Rawlins 1977 pp.70-71&79 for analysis of the book's escape-hatchery, plus a fundamental Ptolemy gender-miscalculation (*ibid* p.69) which has escaped detection & thus survived for millennia, persisting even in today's horoscopes, e.g., those on sale in our grocery-store-checkout-counter literature-departments.

[18] Psychoanalyst Gingerich's intended-to-be-anonymous 2000 referee report to *Isis* (outted 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 *JAHH* referee report's complaint that DR **doesn't respect those who disagree with him**. *Can it get any weirder*? Well, actually, yes. As we see from www.dioi.org/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.)

hour. Similar giveaway factors for his three other solar "observations", all of which agree just as closely with indoor calculation. (Interim question: given this stark&unquestioned circumstance about Ptolemy, think carefully about **what kind of scholar would dedicate himself to defending him, even to the extent of calling all skeptical scientists insane**? The answer has been, for nearly 1/2 a century: virtually anyone who said anything. And this field expects to be taken seriously by scientific scholars? *Seriously*?)

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

The 1st was misconceived at a juniorhighschool level. The 2nd was a clumsy fantasy:[19]

[1] 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*[20] then. So refereeing by Phi Beta Kappa (fn 20) and by Reverend Gingerich, as usual (one might almost say: as-always, given the reliable brand of sheeple who man or oldboy Hist.sci's most prominent forums)[21] has approved an argument implying that if we toss a ball upward and catch it 4 seconds later, a 9th-grader (or younger) can't tell that it peaked at 2 seconds?

[2] Swerdlow's fantasy for explaining why Ptolemy's four solar "observations" were (§J2) scores of times nearer Hipparchos' indoor solar tables than to the outdoor Sun: all ancients selected[22] outdoor data to agree with indoor theory. (N.B.: This would naturally justify destruction of the unused data.) Comments: [a] Even if it were true, the proferred alibi wouldn't explain physically-impossible repeated 1°-off-the-mark Ptolemy "observations" that could never have been made outdoors in the 1st place! Especially again&again&again. (The human eye can see to about two ordmags better: roughly 1′, and the solar semi-diameter is 16′, so his equinox-solstice errors average about 4 times the distance from the Sun's center to its edge: §B2.) [b] Further, we know that 2nd century BC Greek *scientist* Hipparchos reported real observations which disagreed with his theories and with each other (‡3 fn 8); thus, faking or selecting data was not genuine ancient astronomers' normal procedure. [c] So many accurate Greek astronomical achievements (e.g., lunar distance

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

[20] The incredible reasoning of Swerdlow (MacArthur&PhiBetaKappa!) is examined at R.Newton 1991 fn 20 and Rawlins 2018U §§B2-B3. He and J.Evans continue (in ignorance of both the observing technique and the historical record: details at ‡3 fn 96) to insist that solstices could not be measured accurately compared to equinoxes, despite several inconvenient facts:
[A] Outdoors Hipparchos' solstices are about 4 times more accurate than his equinoxes: ‡2 §N7.
[B] More expert at the relevant science than certain modern wannabees, all ancient scientists used solstices not equinoxes for gauging yearlength. (Enumeration of these at *idem*; sources: *ibid* fn 11.)
[C] Not even recent miraculous recovery of the 1900y-old papyrus *P.Fouad* 267A, with solstice's time correct to ordmag 1h (actually to a fraction of 1h, by chance) has yet enlightened any cultist.
See Rawlins 2018U for full details of ancient solstice-determination, and *DIO*'s new formula (*ibid* §H) accounting for ordmag 1h errors in such, inevitably but trivially due to asymmetry from Earth-orbit eccentricity, errors which Swerdlow&Evans couldn't even quantify, ere so prominently (*JHA* & Oxford Univ Press) displaying their own [A]&[B] double-ignorance, Swerdlow of course adding a (pricelessly ironic) sneer of imagined superiority: R.Newton 1991 fn 20.

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

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

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known within 2%, all 3 monthlengths accurate to 1-part-in-a-million or better, observatories' latitudes correct to ordmag 1′), could never have been arrived-at over centuries of investigation, had ancient scientists just unprogressively copied their predecessors.
The cited clique's mass-slander of all ancient scientists' empiricism and ethics is widely believed among academics, who've no notion that they have been protected by skewed journals from learning that it is nothing but a wrench of history directly caused by the continuing pretense that indoor-cheater Ptolemy was the ultimate ancient astronomer.
[d] The purely dreamt-up claim that it was standard practice for ancient Greek astronomers to select outdoor data to fit indoor theory, merely models all ancients after Ptolemy, in order to then turn around and defend Ptolemy as being just like all ancients, *the very same literally-preposterous logic LEARNED FROM PTOLEMY who faked "observations" agreeing with his theories, in order to then "prove" his theories from these same data.* Shame-shame-shame on DR for accusing JHADsters of ineducability. . . .

B4   Delambre 1817 had noted and Rawlins 1982C had investigated the glaringly unique failure of Ptolemy's 1025-star catalog (*Almajest* 7.5-8.1) to contain any stars lower than 6° above his horizon, indicating Hipparchos as the catalog's observer, since his southern Rhodos Island observatory (geographical latitude 35°53′) stood c.5° north of Ptolemy's Alexandria ($L = 31°12′$). So Schaefer 2001 contended at enormous length, in (yet-another!) *JHA*-Pb-anti-RRN paper, that the catalog could've been observed from Alexandria nonetheless because aerosols (atmospheric crud) blocked[23] low stars. Among Pickering 2002A's unanswerable responses: if this were the problem, the southern limit of the hundreds of stars in Hipparchos' *Commentary* would also be raised, so it should be 5° higher than Ptolemy's, but: *it's the same* : **END OF ANY REAL CONTROVERSY**. Why didn't Schaefer know that? Simple: no *JHA* pseudo[24] referee noted that, throughout his 42pp paper disputing Hipparchos' *Almajest*-catalog authorship, Schaefer had never consulted Hipparchos *Comm*'s 100s of star-positions. His later unembarrassed but embarrassing retort claimed that no one could know anyway which stars Hipparchos *Comm* was referring to. Which revealed he had no idea how Manitius 1912-3 & Graßhoff 1990 had used the various phenomena, www.dioi.org/fff.htm#ngjm, to pin down virtually every star unambiguously. And Duke 2002C p.33 cleverly noticed that among 13 deep south stars Schaefer 2001 had argued were Ptolemy's, 5 of these stars shared undeniably huge positional errors with Hipparchos *Comm*, as Graßhoff 1990's brilliant examination had already shown years earlier. None of these definitive findings has caused Schaefer or *JHA* to retract anything.

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

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[23] Ever-fertile Evans 1987 p.166 even argued that 6° of rocks or trees might've blocked Ptolemy's southern view. The easily testable flaw in this alibi is explored in mathematical detail at ‡2 §L.

[24] Don't miss www.dioi.org/pm3.htm, longtime (1970-2013) "premier" *Journal for the History of Astronomy* Founder-Editor Michael Hoskin's efficiency: refereeing&verdict between breakfast&lunch! Must be read to be believed. Lucky nobody will ever find it quoted in our vaunted watchdog "Science Press" whose ever-advancing investigative impotency has carried its transformation into lapdog ever nearer the ultimate intimacy it aspires to: lapdancing a needy establishment.

to $\Delta\lambda = -29' \sin\lambda \tan\beta$, which gets substantial in the north. In the star catalog no such $1°/2$-amplitude waves exist, so neither did an outdoor star-collecting Ptolemy. Of course, ever-openminded Ptolemists robo-countered this new shocker with their usual standard-weapon: scientific inability so truly embarrassing as to raise the question of whether impenetrable Ptolemism 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* ‡3 §C11) longitudes' conversion to great-circle measure entails multiplication by $\cos\beta$, thus gt-circ $\Delta\lambda$ can't exceed $29'$. Swerdlow's response: he hides (*DIO 8* ‡5 §J4). Meanwhile, Evans' attack on the (§B5) absent-error-waves argument confused sine waves with cosine waves (‡2 §H1) blowing off a *63° degree phase-difference* with: "the phase is not exactly right" (Rawlins 1991W fn 152). All this to obscure that he can't find a cosine wave in the Catalog's zodiac stars, with amplitude even 1/3 as large as the $1°/2$ he sought.

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

[1] In 1981 Evans used a cross-staff to measure the longitudinal distance of a star from the mid-eclipse Moon. "I find on examining my notes from that evening" the longitude's error was c.$-40'$ (Evans 1987 p.275), proving Ptolemy-skeptics were wrong to deny that ord-mag 1° errors were not unusual for ancients. However, Rawlins 1991W fn 288 and Rawlins 2009E later showed that if Evans' reduction hadn't bungled his lunar parallax correction's sign, the error would've been merely ordmag $1'$. Evans' response: he's hidden "my notes from that evening" ever since, hiding also from questions on the incident put by Rawlins and Thurston. (See ‡3 fn 11. 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 (nearly-verbatim: Evans 1998 p.259), Evans conveniently forgets to discuss that 1981 eclipse at all (switching instead to adducing a previously unmentioned 1977 eclipse he hadn't outdoor-measured with) — and continues ineducably contending[25] for ancient errors of ordmag 1°.

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

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

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

C   Latest Into the Lists

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

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

C3   Of *Alm* 7.3's 54 star-declinations δ reported by 4 ancient observers, BZJ's 2014 project examined 53: Timocharis 11 stars, Aristyllos 6, Hipparchos & Ptolemy 18 each. These data had already been studied by Pannekoek 1955 (1st to appreciate the accuracy), R.Newton 1977; also Rawlins' 1982 bivariate least-squares study, which *Centaurus* refused to publish, though this scrupulous 46pp paper was sent to K.Moesgaard on 1982/7/14, requesting *Centaurus* publication. Moesgaard's overseer, Editor O.Pedersen, spitefully published instead a mathematically unsophisticated[26] monovariate paper, Maeyama 1984 (received at *Centaurus* a year later,[27] in 1983 June), whose standard deviations for the four

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[26] 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 σ_E.

[27] Maeyama 1984 p.308 acknowledges that he had seen DR's paper in 1983. It appears possible that, until noticing this, BZJ were in some doubt as to whether the DR 1982 ms (unmentioned in Brandt *et al* 2011) was really done then (perhaps supposing that Rawlins 1982G was just based on guesswork not statistics), as 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, Rawlins 1982G split Aristyllos off from Timocharis and gave both astronomers' dates, explicitly on the basis of star declination studies [calculated & tabulated in the unpublished 1982 ms], adding the novel finding that all five precise ancient Greek star collections showed that their observers knew their geographical latitude L to ordmag $1'$. See §C14 below.) Or why-

Greek observers' epochs E were eyeballed not computed.

C4   Brandt *et al* 2014B attempted modest improvement & useful checks on previous work by bringing in modern satellite-determined data, and providing independent (if shaky) statistical indication of the separation of Timocharis & Aristyllos (the split 1st statistically proposed in Rawlins 1982G). DR was asked to referee the paper: *DIO*'s report, www.dioi.org/jau8q.pdf, is on the *DIO* website (as are our letters[28] in this connexion), and that report (looking for any possible basis to be positive about) recommended publication of the new material.

C5   But the *DIO* referee report, www.dioi.org/jau8q.pdf, also emphasized that the paper should not suppress a few extremely germane items, fully known to *JAHH* (through the referee-report), which point in a direction other than its inexplicable Ptolemist conclusion. (Though R.Newton is cited as a skeptic on Ptolemy, none of his or DR's damning evidence appears anywhere in the paper, and the reader will not even learn that DR doubts Ptolemy, much less what his reasons are. Surely an odd way to treat a conscientiously helpful referee.)

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

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

$$\delta = L \pm Z \tag{1}$$

(minus-sign for southern transit, plus-sign for northern upper transit, where Z complements altitude h: $h + Z = 90°$), so L's error carries directly, additively, fully into the δ data, the systematic error of whose mean is therefore the error of L. This would seem to be obvious, but the 1st researcher ever to perform the test upon star data, to show contra-conventionally the admirably small error in ancient star-observers' L-error, was DR, for the History of science Society: Rawlins 1982G. From the δ data contemporary with Ptolemy, all analysts since (including Brandt *et al* 2014B) have concluded that there is but tiny error (ordmag $1'$) in the observer's assumed geographical latitude L. So it should not be hidden from the reader (as it is, throughout Brandt *et al* 2014B) that *when Ptolemy reduces **transit** data* (via eq.1), he uses an Alexandria $L = 30°58'$ (*Almajest* 5.12-13), **which rules him out as the declinations' observer** since this L is in error by $-14'$ (Alexandria being at $L = 31°12'$).

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

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else say that DR was (*ibid*) merely "interested in checking" the latitude-errors? — as if the discovery of these had been around for years. Why, throughout, is Maeyama 1984 usually cited ahead of DR's earlier 1982 works, when both are mentioned? It seems especially strange to find DR's unambiguous priority, in computing separate dates for Timocharis & Aristyllos reported thusly at Brandt *et al* p.334, www.dioi.org/bzj.pdf, "Until the early 1980s [their dates] . . . were taken to be the same. Currently, the dates are considered to be different (Maeyama, 1984; Rawlins, 1982a, 1982b, 1994)." This becomes even harder to explain when we find that the earlier, refereed (otherwise nearly identical) version of the paper, www.dioi.org/bzj0.pdf, has the verbatim-same wording except for the citations, which were simply chronological back then: "Rawlins (1982, c.1983, 1994); Maeyama, 1984)."

[28] DR: www.dioi.org/bjr3g, www.dioi.org/owu8g, www.dioi.org/owu8q, www.dioi.org/owucm.pdf, www.dioi.org/owv9u.pdf, www.dioi.org/owwt2, & www.dioi.org/oww3l.pdf, the last promising not to contact WO further if no reply.

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

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had he faked them by adding 2 2/3 centuries worth of his false 1°/cy precession, namely, tacking 2°40′ onto all Hipparchos' stellar longitudes. (Closely agreeable date, assuming the star-declinations were faked similarly.) No mention of this Coincidence in Brandt *et al* 2014B! — though urged by referee DR. It has been explicitly well-known for over a century that virtually the same date matches the date for which Ptolemy's 1025-star Catalog's fakes would seem correct, were they real, which few scholars believe anymore. E.g., Peters & Knobel 1915 p.15 noted that +58 is the date when Ptolemy's misprecessed 1025-star *Almajest* 7.5-8.1 catalog is correct. There is no sign that BZJ knew of this match prior to DR's referee report, nor does such vital information appear in Brandt *et al* 2014B subsequent to it, either. This positively belongs at the paper's p.333, where we instead hear that besides his star-declinations (emph added): "Ptolemy offers additional *evidence* for his [(false) precession] value elsewhere in the *Almagest* (e.g., [Toomer 1984 p.]338)" — innocent of the A.Jones-witnessed fact that upon viewing Graßhoff 1990's evidence decades ago, Toomer agreed that the Catalog stars came from Hipparchos — which renders irrelevant the faked "Ptolemy evidence" cited to Toomer 1984 *loc cit*. The *DIO* ref-report asked: "So are we also to ascribe the Catalog to the same secret observer, who thus must have created a catalog of over 1000 stars though no one ever mentioned his or its existence?" [D] For the 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 *Almajest* 7.3) — if one adopts the independently-arrived-at epoch E (+159) and geographical latitude-error x (+4′) already found through bivariate analysis (Rawlins 1994L) upon the unsuspect data, without any selection among or tampering with these 12 data, long-separately-recognized and separately treated by Ptolemy. Significance of this for Brandt *et al* 2014B's peculiar new split is emphasized below at §C21.

C6   *JAHH* readers *have a right to know §C5's four central considerations*, but Editor Wayne Orchiston (WO) has not felt the need to inform[30] them. So much for the integrity of the paper's Ptolemist conclusion. And of the *Journal of Astronomical History & Heritage*.

C7   Brandt *et al* 2014B p.331 claims that its analysis is a bivariate repeat of DR's 1982 analysis but (as warned in www.dioi.org/jau8q.pdf, the 2014/8/26 *DIO* referee report on the paper) it is really[31] just a try (like Maeyama 1984) at solving a bivariate problem monovariately. Except for Ptolemy (where different samplings[32] 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 σ_x are not given anywhere: not even when BZJ attempts recounting and repeating Rawlins' analysis (which explicitly supplies and tabulates both x & σ_x). Instead, BZJ present miscalculated (§C9) values for a single entity, "accuracy", which they confusedly seem to regard as sufficiently equivalent.

C8   The *DIO* referee report warned[33] that the "accuracy' values given at Brandt *et al* 2014B p.331 are "astonishingly low".

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

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

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

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

C9   The erroneous[34] figures for "accuracy" rs in Brandt *et al* 2011 and Brandt *et al* 2014B were an ordmag too small, presumably because they were mistakenly found[35] (as hinted at in Brandt *et al* 2011) by [a] searching monovariately for the E that minimizes the sum S of the squares of the residuals, [b] subtracting the subsequent mean residual from each datum, [c] with the adjusted data, re-computing the problem nullivariately for an independently estimated best E, [d] computing "accuracy" rs by meaning the minuscule leftover residuals. (Our reconstructions of data via this procedure are in fn 35.) Perhaps we could dub this the "least-non-squares test". The impossibility of BZJ's numbers are easily seen: when Brandt *et al* 2014B p.331 puts the "accuracy" of Aristyllos & Hipparchos at $0°.004$ & $0°.003$, this translates to 14&11 **ARCSECONDS**, resp — obviously a fantasy (and BZJ were warned of this on p.4 of www.dioi.org/jau8q.pdf, DR's invited referee report), considering that the data's rms is admittedly $0°.1$ on the same page: Brandt *et al* 2014B's Table 2. (Equally incredible: *idem* lists rms values for methods of Maeyama, Rawlins, & BZJ — that agree with each other to a 1000th of a degree!)

C10   Interlude: From where did BZJ get §C9 [d]'s wacky idea that one should simply **mean** the residuals? Answer: from misconstruing p.283 of their preferred prior analysis, Maeyama 1984, where it is stated that (following determination of epoch E), "The epoch [where S is minimal] will be our first approximation. The resulting **mean deviation** at this epoch will then correspond to the mean systematic error[36] . . . ." 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.1, 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^y 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* 2011 (BZJ's *BAAS* 2011 abstract) or Brandt *et al* 2014B p.331. (Note: The paper Zimmer *et al* 2013 admirably takes no part in evaluating anyone's x — or anything at all about Ptolemy.)

[35] BZJ's initial abstract, www.dioi.org/bzj11.htm, Brandt *et al* 2011, gave figures for "accuracies" (where we flip BZJ's unconventional C−O signs): Timocharis $E = -295$, 11 stars $rs = -0°.022$, Aristyllos $E = -258$, 6 stars $rs = +0°.004$, Hipparchos $E = -128$, 18 stars $rs = -0°.010$, Ptolemy $E = -115$, 18 stars $rs = -0°.005$. Later, Brandt *et al* 2014A p.6 & Brandt *et al* 2014B p.331 have (now signlessly), for the same samples & dates, rather different rs: Timocharis $0°.003$, Aristyllos $0°.003$, Hipparchos $0°.004$, Ptolemy $0°.009$. Our speculative reconstructions (via §C9's [a]-[d]) alter the experiments but (in a delicate problem) get agreements with some among BZJ's above false rs values. So maybe this or something like it was BZJ's procedure?
[Accurate bivariate least-squares result follows each observer's reconstructed BZJ data, in brackets; plus actual minimal residual-square sum S_m, to show that most BZJ solutions do not approximate it.]
Timocharis 12 stars: $E = -295$, $rs = -0°.022$, $S = 2745'^2$.
[$E = -277 \pm 18^y$, $x = -0°.076 \pm 0°.077$, $S_m = 2441'^2$.]
Aristyllos 6 stars: $E = -258$, $rs = +0°.003$, $S = 147'^2$.
[$E = -258 \pm 10^y$, $x = +0°.016 \pm 0°.045$, $S_m = 147'^2$.]
Hipparchos 17 stars (Alioth $\delta = 67°3/5$): $E = -128$, $rs = +0°.004$, $S = 446'^2$.
[$E = -133 \pm 8^y$, $x = -0°.001 \pm 0°.021$, $S_m = 392'^2$.]
Ptolemy 18 stars $E = +111$ (Brandt *et al* 2014B Fig.5 no-prop-mot), $rs = -0°.005$, $S = 2539'^2$.
[$E = +115 \pm 13^y$, $x = +0°.004 \pm 0°.052$, $S_m = 2521'^2$.]

[36] In his 1983 Aarhus talk Maeyama did not yet know that "mean systematic error" relates to error in the observer's assumed latitude. He later disremembered that he learned this from DR's ms: fn 27 above. The results displayed at Maeyama 1984 p.292 Table 1 are not from bivariate but monovariate 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-monovariate test and had the good fortune that the unknowns' correlations were not too serious. And at least (unlike BZJ) he realized that the leftover residuals after the 1st monovariate test were to be fed into the 2nd such, to find the value of x which *minimized the sum of the residuals' squares*. However, for x's standard deviation σ_x, Maeyama 1984 Table 1 column d wrongly lists σ_o, the mean error of a single observation. The resultant errors range as high a factor of nearly 7 (the Hipparchan 44-star sample).

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

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

C12   Moreover, any of those who've since 1982 tried vainly to improve upon DR would have remarked (had they done a valid 2-unknown simultaneous least-squares) that the rs for the Greek observers is zero in all four cases. None has. A useful extra check: in the cases where correlations happen to be tiny (e.g., Timocharis & DR's Hipparchos analysis where $n = 19$ stars), one can come quite close (since x has a unity coefficient in Brandt *et al* 2014B's eq.1) to finding x's error σ_x through just dividing σ_o by $\sqrt{n}$.

C13   In a true bivariate solution, e & x are least-squaresed simultaneously.[37] All the figures given in the 1982 manuscript and in Rawlins 1982G were so accomplished. (By hand, incidentally. The later computerized solutions, 1994, 2011, & present Table 1 here [identical to ‡3 Table 2 above, except for Timocharis] barely differed at all.)

C14   Before 1982, no one had ever used these data to find the accuracy of the four Greek astronomers' observatory-placements. Misled by the crudity of the data of most of Ptolemy's *Geographical Directory* (*GD*) & the rigid infectious mantra of certain history-of-science archons, that ancient Greeks were non-empirical (Rawlins 2008R §A), many had long ago gotten the idea (persisting to the present in the History of science Society's rulership: ‡1) that ancient geography was typified by position errors of ordmag $1°$. That was why Rawlins 1982G — whose main analysis showed $1'$ precision in the solar transit work of 3rd century BC Alexandrian astronomers — emphasized this revelation (in a brief footnote on stars: fn 27 above) to a History of science Society audience: DR's 1982 discovery that bivariate least-squares had determined for the 1st time, from the *Almajest* 7.3 declinations, just how well Greek astronomers could know their geographical latitudes L — which of course led on to the question of why the *GD*'s coordinates were so awful (‡1 §I; ‡3 §I11; Rawlins 2008S). DR's papers have called L's error x, and epoch-error e (epoch E solution minus tester's assumed E_o); if the mutual solution is done truly bivariately, it will find not only e (thus E) and e's standard deviation σ_e, but x and its standard deviation σ_x, as well as the single-datum standard deviation, σ_o. All these solutions are displayed here in Table 1.

C15   If it seems odd that, previous to 1982, no one had found the accuracy of the four ancient observatories' location (see Rawlins 1985G §3 conclusion), let's expose something even more revealing: in 36^y since 1982, **no one else has computed it correctly, either**. The two post-1982 papers both waste precious journal-space extensively on graphs & histograms, all to do the analyses inferiorly, e.g., monovariately finding that E where S is minimal, S_o. (And the archons of history-of-ancient astronomy imagine they have the capacity or right to judge, shun, condemn, & censor the scrupulous, competent researches of scientists in such matters? Note the parallel to the *Journal of Astronomical History &*

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

Table 1: Ancient Observers' Epochs E, Adopted and Actual Geographical Latitudes L

Obsrvr	$E \pm \sigma_E$	Adop L	Its Error x	Actual $L \pm \sigma_L$	σ_o	σ_r
Timoch	-302 ± 08^y	$31°12'$	$+1'.5\pm1'.9$	$31°10'.5\pm1'.9$	$\pm6'.1$	$\pm5'.9$
Aristyll	-258 ± 10^y	$31°15'$	$+1'.0\pm2'.7$	$31°14'.0\pm2'.7$	$\pm6'.1$	$\pm4'.2$
Hipp	-131 ± 05^y	$36°08'$	$+0'.2\pm1'.2$	$36°07'.8\pm1'.2$	$\pm5'.2$	$\pm5'.0$
Anon	$+159\pm09^y$	$31°15'$	$+4'.4\pm2'.0$	$31°10'.6\pm2'.0$	$\pm6'.0$	$\pm5'.6$

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

C16   In 1994, 12^y later than 1982, DR discerned a new method for finding each observer's *assumed* geographical latitude: from nulls[38] 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 (σ_E & σ_x), as well as single-datum standard deviation, raw (σ_o) and with the effect of rounding[39] removed (σ_r). All these desiderata are produced here in Table 1, slightly[40] improved (see fn 38) vs the values of ‡3 Table 2 above or Rawlins 1994L Table 3. Strangely, Brandt *et al* 2014A, the refereed version of Brandt *et al* 2014B, www.dioi.org/bzj0.pdf, claimed that Rawlins 1994L had latitudes "close to our values" — this, even though BZJ *have to this day never solved for any of these latitudes*. So www.dioi.org/jau8q.pdf, the *DIO* 2014/8/26 referee report, at pp.3-4, suggested that this point be clarified *and that Rawlins 1994L's x values* **and absolute L values** *for all four observers* be printed, *since BZJ had brought up the point*, and had supplied various[41] of DR's other numbers (the majority correctly) — preferably along with a sentence on the novel though simple means which *DIO* had invented while pioneering this entire line of inquiry. But, probably because *DIO*'s x values especially & hugely disagreed with *JAHH*'s "accuracy" values, the published article did none of these things.

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

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

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

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

[41] E.g., at Brandt *et al* 2014B p.331, for all 3 observers, our 1982 ms' epochs E & σ_o are relayed, conspicuously omitting our x & σ_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

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which by either version of the paper were previously unknown to Ptolemy or anyone else. The p.332 grouping is E (Early 6 stars) and L (Late 9 stars), which does not follow the traditional split, namely: the "SickSix" stellar declinations (which Ptolemy "deduced" his false precession from) versus the "Clean Dozen" real declinations (which his precessional math ignored): "our groupings have no simple connection to Ptolemy's selected six stars" (Brandt *et al* 2014B p.334). Why? Well, R.Newton 1977 pp.220-225 rightly argues that Ptolemy typically fabricated the SickSix from 1°/cy precession in order to equally-typically then "prove" said precession from the fabrications: ‡2 §B2. The new grouping would undercut this view by fracturing the SickSix. Problems with the new split (as noted in DR's 2011/3/15 letter to Brandt): the traditional split is simple, is Ptolemy's own. Further, DR's 1994-adopted $+159$ epoch (Rawlins 1994L fn 45: all 12 Clean Dozen stars with *no deletions*) was understood in 2011 for the 1ˢᵗ time to *establish no residuals-overlap* (fn 7) in the Clean-Dozen-vs-SickSix split, a finding nowhere cited in Brandt *et al* 2014B, which keeps mis-rendering DR's also-uncited $+159$ date, though it was repeatedly, www.dioi.org/bjr3g.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 Alioth 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 $E = +159\pm9^y$, $x = +4'\pm2'$ ($L = 31°11'\pm2'$: Alexandria) and the extremest residuals are symmetrically within $10'$ of zero: Betelgeux $+10'$ & Pollux $-10'$. For contrast, one may examine the results of applying, to the Sick stars, the very same test just done on the

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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 that their zero error occurred later than 200 AD (a criterion eliminating Altair [p.334 vs p.335&Fig.8], though it was nonetheless retained as a member of group L) which only adds on a date-based bias among those small-declination-speed stars — though the date is the very entity one is seeking. (In dropping Betelgeux, DR was guilty of a similar mis-step in 1982 [repeated in a different way by BZJ in 2014] which was cleared up in 2 stages, in 1994 and — thanks to Brandt's restimulation of interest — in 2011. But this was explained in DR's 2011 letter to Brandt, to no effect.) The ref report also noted a mistaken claim (p.334) that one of the SickSix stars is among the Unhelpful, though none are (by either of the paper's two versions of grouping) — and, in 2014's *DIO* referee report, www.dioi.org/jau8q.pdf, we guessed (though not even told the membership of groups L&E) that the paper's statement that three of six E stars were Sickies should read four of six. This turned out to be the case (Alcyone, Capella, Spica, Alcaid) — again, for either version of grouping. Yet no correction was made before publication. Is this odd slip vestigial of an early trial-version of the selection process, during the sample-forming's shopping-around period (similar to the slip at Duke 2005T p.173, noted at Rawlins 2009S §K6; and don't miss fn 22) — before settlement upon the final versions of L&E? This further hint of arbitrariness is one of the factors vitiating the paper's lengthy, impressive-appearing search for groupings of stars different from the skeptics' simple acceptance *of Ptolemy's own groupings*. In the final paper, no correction occurred for the above-cited ref-noted fact that Aldebaran was eliminated from BZJ's groups L&E, 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") — when these are *the very stars that least-flexibly measure latitude-error*. Real bivariate investigation would know that and would know that getting E exactly 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/fff.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 1982 ms' eq.2), thereby equating Observed-minus-Calculated with what is actually just Calculated. If taken seriously, this makes Observed equal to twice Calculated!

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

C18   For finding epoch E, Brandt *et al* 2014B adheres to depending on each star's "crossing time" (the year when its residual is zero) & "slope" (rate of change of declination/year). Though of some interest and utility as rough checks (on better procedures), these approaches are sub-prime (especially when compared to standard approaches — which are perhaps avoided by BZJ since they give results in accord with R.Newton?), repeatedly necessitating debatable decisions on deletions and weighting. It's almost as if it was decided to hunt up results every which way but the best: full bivariate least-squares. E.g., small-slope stars' low weight (for E-determination) is automatically accounted-for by least-squares, so there is no need to delete such stars — additionally: doing so will 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), Brandt *et al* 2014B tries including weights by slopes' *absolute magnitudes* (p.331 & Fig.6), the kind of Legendrian primitivity that Gauss devised least-squares to obviate. (Again: this requires deletion of stars which Gaussian analysis doesn't.) But when the paper moves into cluster-analysis, even this precaution vanishes.

C20   The paper concludes with a long, illustrated section (slightly altering §C17's L-vs-E regroupings that replaced Ptolemy's simple split) which tests for 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 Pollux, whose slope is smaller? — probably because their mutually wan slopes (*nearly*

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

[45] If we re-check the residuals via monovariate test for L at Ptolemy's alleged observation-year, +137, instead of +159, we find the Clean Dozen more poorly fitting, residuals ranging from −8′1/2 (Zubenelgenubi) 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 σ_o (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 137/7/20 Alexandria App.Noon.) Note: a monovariate e solution for the same stars leads to +150, *a serious difference* (see §C13 on simultaneity). And, since such automatically assumes $x = 0$, we have $L = 31°15′$ (see §C16 above, & Rawlins 1994L §F8), 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 overlarge 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 Brandt *et al* 2014B persistently refused to recognize DR's discovery of +159, misprinting it (over warnings) again&again as +131. See §C21 below.

[46] See at www.dioi.org/je01.pdf, *DIO 14* ‡1 §J2; & www.dioi.org/je03.pdf, *ibid* ‡3 fn 13, the parallel case of now-needless metrological theories that keep getting proposed to weakly explain the already strongly explained ancient Earth-size of Sostratos-Eratosthenes.

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

C21   A peculiarity related to the question of accurately locating the "Ptolemy" stars' epoch E: Brandt *et al* 2014B *repeatedly* ignores (fn 45 above) the request, by the *DIO* referee report, www.dioi.org/jau8q.pdf, to correct its *repeated* misrendering of Rawlins 1994L's date (for Anonymous' Clean Dozen stars) as +131 instead of Rawlins 1994L Table 3's actual published value, +159. This is a 28[y] difference, which matters, as we saw at §C17 & fnn 42&45. (The misprinted epoch, +131, was merely Rawlins 1994L's date for *Hipparchos* — not Ptolemy's Anonymous — additionally shorn of its minus sign!) Importantly, +159 is an epoch which, for the 1st time, renders stark and unambiguous (§C17) the Clean-Dozen-vs-SickSix split (by contrast to *any* of the many other Anonymous epochs E that were flirted-with in the article or the referee report), and in doing so implicitly jettisons as needless (fn 46) Brandt *et al* 2014B's elaborately-derived unorthodox groupings. (See fn 42 above, for the advantages of adopting what is after all Ptolemy's own split.)

C22   A shock that might give historians-of-science pause before continuing weird promotions of the myth of observer Ptolemy: *the declinations issue was solved with full competence over 30[y] ago*, by R.Newton & D.Rawlins. The 2 papers historical journals (*Centaurus* & *JAHH*) have published on the issue since have just messed it up some, while discovering nothing new that's valid. Indeed, as seen from ‡3 Summary's conclusion (p.47), R.Newton would judge these efforts primarily "subtractions from the sum of human knowledge". Which anyone could've foreseen by noticing that both papers inexplicably got deeply involved in graphical solutions by trial&error, the latter paper soberly treating the former as the prime prior research!

C23   As a final quietus to *JAHH*'s monucementally stubborn 2014 adventure, we now show how easily a scrupulous journal could have checked on whether its or our solutions for x were correct. All *JAHH* needed to do was: vary the x&e of their solutions to see if their residual-squares-sum S was minimal, i.e., equal to our minimum, S_o. (Which is the square of the appropriate σ_o in Table 1, multiplied by the number of degrees of freedom.) Or: for any of the four ancient astronomer's star-residuals, [1] subtract *DIO*'s tabulated[47] x for that astronomer, and then [2] just re-run Brandt *et al* 2014B's monovariate test for him. BZJ will then encounter a sorta-pleasant surprise: all four astronomers' values of S, the sum of residuals-squared, will be found to have declined (comparisons in fn 35), showing that Brandt *et al* 2014B's S generally (except for Aristyllos) didn't get very near optimal (extremal) solutions S_m. (Due to low correlations, the differences are not huge; but they show that true bivariate procedures were not applied by BZJ.) Even so, the suggested better solution being found by crude means (fn 36), results in S usually near but not quite at the lowest S possible. This goal can, however, be accomplished through a true bivariate least-squares (as in Rawlins 1994L), which efficiently finds the point in x-e space where S is a minimum. If BZJ have any doubts that *DIO* has found THE actual minimum S, they need only conduct the very same test, using our e&x values, to find an S lower than their own. Using the slightly improved values (vs the referee report) of Table 1 above, the S ***cannot be decreased further*** (more than micro-trivial noise) by varying either x or e. Our solutions for x are just 0-4 arcmin. The size may be small, but the issue isn't: the

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[47]DR's latitude-errors x for Timocharis, Aristyllos, & Hipparchos have for decades been available, for all to check, at Rawlins 1994L pp.44-46 & Table 3 — virtually the same as in his 1982 ms, to which Brandt *et al* 2014B p.331 acknowledges access. The BZJ paper's sampling differs from Rawlins 1994L's 19 stars (vs BZJ's 17) for Hipparchos & 12 stars (vs BZJ's 18) for Ptolemy, but the x that's appropriate for BZJ's sampling was provided at pp.3-4 of www.dioi.org/jau8q.pdf, the *DIO* 2014 ref report. (Due to minuscule differences in adopted star-places, the x value that will produce minimal S_m may not be super-precisely identical to *DIO*'s; but further trials will easily find it, very, very nearby.)

central point here is (as 1st revealed in Rawlins 1982G) that ancient scientists found L to ordmag $1'$ accuracy. For that reason, as well as Brandt *et al* 2014B's p.331 advertising $1'$ accuracy, the most precise solutions for x are appropriate. This becomes important (fn 45) for the Clean Dozen, where $x = 4'$, closely reflecting the error in the observer's adoption (independently demonstrated in Rawlins 1994L §F8) of $L = 31°1/4$ for Alexandria, which is $3'$ (close to $4'\pm2'$) 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#bnld.

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

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

D2   The truth behind the unprincipled[48] — sometimes (e.g., fn 18) even vicious — stubbornness[49] of those determined to protect Ptolemy from public exposure by any means (www.dioi.org/mot.htm#xcfp) is that they are not protecting him but themselves and/or their gooroos — resorting to any sloppy argument, any curtailment of free discourse necessary to prevent the larger scholarly community as well as the public from learning that the field's most powerful archons (controlling the funding and thus the career-security, rewards, & awards of those who volunteer to espouse and do battle for sacred myths) made two huge and related blunders (see ‡2 §M2: "***to fit him***"), when they long ago prematurely announced Ptolemy an honest observer and misperceived Greek astronomy as non-empirical.
(See, e.g., ScAm 1979, discussed above at fn 22; and more thoroughly at ‡2 §M3 & fn 52.)

D3   Being politicians, Ptolemist archons are the sort of people whose idea of intellectual engagement tends (for obvious reasons) not towards weighing scientific arguments but to [1] slandering (‡2 fn 5) their opposites as fools, knaves, and nuts (before discussing either — if ever doing so at all), while [2] pointing[50] innocent onlookers to the bemedalled, Reputable people who've taken their side: after all (as we ask at above p.87, in the Text-For-the-Day intro to this article), how could such cynosurae seem so Enormously Stupid — as they must be or act, if skeptics are right?

D4   Well, here's exactly how: just [a] keep smearing heretics behind their backs (details & photos at §B1 above) while continuing to [b] publish pseudo-defenses of Ptolemy's honesty — no matter how ridiculous (‡3 fn 66). Meanwhile, disallow — as too Disrespectful[51]

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[48] But, to be fair, let's admit that it's not just the Ptolemy-defender side that uses Dirty Tricks in combat. In those apologists's eyes, *DIO* uses Dirty Tricks just as cruelly and frequently — that is, whenever we resort to outrageously outré 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#gbsc): a man who can't be bribed can't be trusted.

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

[50] 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 Ptolemists (like BZJ) still exist: "neither side [is] able to produce decisive evidence to convince the other side." (But one thing we can be sure of: inserting such archon-comforting & gratuitous irrelevancy is sure to get a paper published at *JHA*.) One trusts that jollypol Schaefer is smiling as he watches the JHAD show. And one recalls Thurston's quote from Bishop Berkeley, "I observed how 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 a tenet he cannot defend in discussion?" (For these & other *DIO* germs, see www.dioi.org/mot.htm.)

[51] Does the fatal crime, Ya-Disrespected-Me, sound familiar? Seen any mob or blaxplo films lately?

— 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 & suppression of his experimental record (§B6 item [1]) will ever appear in the irredeemably dishonest *JHA*, though undoing this now-conscious deceit is a required&essential part of any counter to Ptolemism, since it is the Pb-paper-prominent "empirical" centerpiece of the JHAD's fantastic 1987-to-1998-to-eternity tenet that huge, Ptolemy-sized observational errors were normal in antiquity. Further, no mention is allowed of definitive evidence (fn 3) of JHAD crimes against academic decency (such censorship constituting just one more crime to be henceforth protected by censorship!): evidence-hiding (as just noted), data-fudgery (‡3 §§C-G: a half-dozen examples), slanderous lies (§B1; ‡3 fn 5), thereby implicitly revealing what has been for 40y the awful hidden truth, namely, that the *entire* Believer side of the Ptolemy pseudo-debate has actually long since become ***no side at all*** (as with creationists), their output having no coherent case whatever, thus resorting to tactics as cited, plus increasingly farout & embarrassing coulda→musta alibi-scenarios. (Deepest dregs at ‡2 fn 11.)

D5   Such childishly obvious illusionism, as delineated at §D4 above, utterly confounds the non-specialist part of academe, as well as the *increasingly non-investigative* (and ever-seminumerate) "science" press, and is the key to the endless pretense of Ptolemist ***historians***-of-science — knowingly careless of concomitant hurt to academe's accurate perception of ancient ***history*** — that archons have not-either been proven as *Notorious*ly foolish as Raines: if just a few of puppeteer Gingerich's claque can forever (§A2, & Rawlins 1992V §C24) keep publishing contrived even-if-laughably-transparent-to-scientists defenses, sapping an ever-befuddlable 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 Ptolemists' lack of science's attitude is the mundane matter of skills. Virtually every member of their clique, whatever his eminence, has no high scientific expertise[52] relevant to the Ptolemy controversy. Non-specialists — unable to understand the debate's technical details (or too busy to take the time) — are oft impressed with networking archons' too-oft-network-granted posts, awards, university connexions, etc. And are thus so easily diverted from the seemingly obvious point that just because a Ptolemist is an astronomer doesn't mean that he knows much about *positional astronomy, orbit theory, & statistics*, which are the specialties one needs for research into ancient astronomy. (Astrophysics, planetary astronomy, & spectral analysis are worthless for it.) These are the very specialties of such astronomers as R.Newton and *DIO*'s Myles Standish and DR. Lack of such expertise shows up in the truly historic fumblings of, e.g., Swerdlow & Evans, as shown above in §B and the especially comedic do-not-miss topsyturvyfest at ‡2 §N — and now in the serial-learning-experience stats (§C8) of *JAHH*'s 2014 paper.

D7   Concluding: we anticipate that (unless made shy by our 2014 referee report, www.dioi.org/jau8q.pdf) Ptolemist archons are already typically (§D3 above) pointing to Brandt's many awards&posts, as if relevant, proud that yet another Reputable Figure has, after reviewing the evidence, decided to help the needy establishment by coming out for Ptolemy, hoping no-one will notice the §A1-obviousness of the fact that eventual discordant-evidence-sterilized Brandt *et al* 2014B's conclusion was all-along set in cement.

                                Slippers.

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[52] In mathematical history-of-astronomy, dimbulbs + careerists + thespians + pols ***now constitute a majority***. If able, honest scientists ever rejoin&review the present era, it'll be remembered, with eyes aroll, as the field's Dork Ages, when reason was punitively proscribed in favor of Invincible Innocence. Archons will stoop to ANY tactic, to postpone that day indefinitely. Understandable. For them.

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