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 $84,000
History of Science Society Theft
Breakfast Lunch JHA Refereeing
$100,000 Prize Eclipse Pair Hunt
Alltime Clumsiest Ptolemy Fakes
Unseen Aristarchos & Archimedes
DIO-Journal for Hysterical Astronomy 22 2018


Table of Contents

Page

†1 Isis Stealing Discovery and BadMathing Greek Accuracy: 2015 and 2016 Egglying 3
†2 The Greatest Faker of Antiquity — Still Foolin’  ’Em: Funniest Ptolemy AlmaJests 10
†3 History of Astronomy’s Serial Data-Tamperers — Endangering Potential Advantages 44
†4 Ptolemites Lay Another Egg: 2014: Suppressing Referee-Urged Contrary Evidence 86

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

This DIO is formed of journals (†1-†3) on new discoveries and academic crimes, sent to cited 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: whaddayagonnamabudit?! Has academy 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-naucitation, data-tampering, grab, shun, fake forever-controversy (‡2 §3-H3, ‡3 §C10, ‡4 §B4), endangering knowledge-advances.

Leaving each paper nearly as submitted entailed overloads, but reflected how many forums ignore corruption & flee demos of poor stats (‡3 p.44, §§F8, fn 100; ‡3 §§C11&C19), abysmal science (‡2 §N7, ‡3 §7T), esp. scientists’ attitude. (Ponder Ragerp’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/iso90.pdf, so degeneracy persists, disgracing even CalTech, Princeton, NYU (†3 fn 9&96, Table 1, §§C5-C11, resp). We value (e.g., p.44) History-of-science’s mining of new ore but regret when sacreddow minds warp its use. DIO Publisher DR’s modest qualifications for these critiques include researches (unexpectedly later-vindicated scores of times: www.dioi.org/lin.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 genteel but (SEE www.dioi.org/ns.htm) shunned&measured 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/lsa.pdf, of an original DIO 2008 discovery (residing for 7’ in ordmag 100 libraries worldwide), & [b] its 2016 laughably math-bungled (†1 §D) attacks on DR’s 1984 Greenwich Centenary conference paper; pseudo-refereeing of Isis’ assault is Hist.scientific, as inside-witless confirms (JHA breakfast—lunch refereeing: ‡3 fn 4’s finale). Angered by submission to Isis of an ultimately mild version (www.dioi.org/qjo.pdf) of unmind 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 punctuations of mis-science (‡2 fn 50) really this scary to those regularly-blundering, www.dioi.org/jhbl.pdf, & pretend-refereeing (†3 fn 66) journals? Are they exaggerating our import by crowing so transparently? The reader may judge.

‡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/cov.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).


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

Shcheglov, particularly on his p.693, imputes several failings to DR’s and others’ bluntness has little to do with why historians-of-science flee us. (Among others: p.44.) Each’s final paper appeared in DIO: 1991&2009.

1 Effectively blinding 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, datatifing hypothesis: that the 1.4-factor error (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.

Curious examples of Shcheglov’s misharges: [a] The simple-stretch idea is alleged (Shcheglov p.693) to bear logical fallacies; none are produced. [b] The stretch-solution is said (idem) to follow R.Newton’s criminal charges versus Ptolemy. Though I agree Ptolemy faked I’ve never said his stretch was anything but a bad mistake (end of §F below). [c] Ignorance of alternate theories is implied (vs DIO 9 [†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 stadelogne guesses.)
Shcheglov (p.705) calls early accurate geography “a quaint illusion” — & his Abstract (ArchaeologicalJournal of IslamicArchaeology 11 [2010]) notes that “the accuracy of pre-stretch 3[1/4]h-3h! [Error about as big as quantity sought: like ±10m] is Quaint at best. Unmentioned in Shcheglov’s attempt to convince historians-of-science that Ptolemy should be somewhat “rehabilitated” (p.687):[1] He “usurped” Hipparchos’ 1025-star catalog (Tycho Brahe, Omnia Opera 3, p.337).[2] “Astronomer” Ptolemy’s four allegedly outdoor solar observations are fifty times closer to his 280′-old indoor Hipparchan tables than to the sky. (Hipparchos’ ratio is less than 3.)[3] His adopted latitude 30°35′ was −14′ off reality, vs just 0′, 0′, 0′, 0′ errors for adopted for his Carthage-Arabela gaffe-gape). As for Pliny 2.72.180, Shcheglov knows Campania & Armenia are c.2° apart. [Longitude gaps between Naples & the Geography’s Armenian cities (Diller Dio 5 Table 17; 1984) Dioskourias, Artaxata, Gaggara are 1°47′, 2°01′, 2°23′, respectively, all indeed about 2°]. So Shcheglov concluded that Pliny’s 3° is too high. Yet Pliny doesn’t say the Campania-Armenia longitude gap is 3° but that the eclipse was seen 3° of local time differently. [Ancients recorded the time of an eclipse’s start: Neugebauer op cit p.844 n.12.] For a solar eclipse, one can’t just equate time-difference and longitude-difference. Local Apparent Time for the eclipse differed in Naples from that at the 3 Armenian cities, by 2°29′, 2°48′, 3°14′; respectively, mean 2°50′. So Pliny’s 3° was not “badly over-estimated”. (Neugebauer, op cit., p.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.  

Shcheglov’s other Pliny record is the same Arbela lunar eclipse Ptolemy mis-reports as 8 P.M. at Cartaghe, 11 P.M. at Arbela. But Pliny has the same event 6 P.M. at Sicily (west Sicily was under Carthage then), 8 P.M. at Arbela, resp, both times correct within minutes. Shcheglov n.8 doesn’t connect the two Arbela-eclipse reports; & neither he nor any other historian-of-science has noted that “authoritative” (Shcheglov, n.7) scientist Ptolemy has accidentally mis-reports Arbela’s 3° Ptolemy mis-reports Arbela’s 3°. But Pliny has the same event 6 P.M. at Sicily (west Sicily was under Carthage then), 8 P.M. is reported as a word (“moonrise”) not a number. Unequal to the Latin, Ptolemy thought 8 P.M. was Cartaghe time. Since his 4/3-stretched map already had Arbela 3° east of Carthage (real gap 2°1/4), he faked Arbela thusly: 8 P.M. + 3° = 11 P.M. The false equation, universal astronomical astronomy in the 2nd century, is ‘Gingerich’s Greatest Astronomer of Antiquity’? Ptolemy’s times mega-disagree with not just reality but his own tables: 2°, 3°! [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/jf03.pdf, he’s uncomprehendingly consulting in his nn.12&15.  

On p.705, Shcheglov’s varied attempts at “rehabilitation” include his pure guess that scientists kept only theory-accordant data [Shcheglov’s n.8 tries alibiing Ptolemy and simultaneously attacking... four otherancient eclipse-pair reports, three’s longitude differences “also give badly overestimated results”: Kleomedes 4° gap is virtually correct since Cadiz is 25°W longitude and Persepolis at 33°32′E are 3°57′ apart. Shcheglov just mis-signed Cadiz and found 3°07′ (comfortingly consistent with Ptolemy’s false 4°3 factor...
DIO-J.HA 22 \( \| \) 1 Ancient Accuracy Vs Hist sci Soc 2018 D.Rawlins

Latinitudes of real observers Timocharis, Aristyllus, Hipparchos, & Ptolemy's Anonymous.

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

Shcheglov admires Geography latitude-accuracy (p.689, emphasis added): “Methods for determining latitude, being rather simple, had [long] been known in Greece . . . . By Ptolemy’s time, latitudes of a number of the most important cities had been determined (e.g., Alexandria, Rhodes, Athens, Rome, Massalia) . . . . Ptolemy calls such cities . . . ‘foundations’ that should be used as reference points for developing the rest of his map.”

Given 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’ amatorius use of asymmetric gnomon). Meanwhile, statistical stellar analyses by Rawlins (IIS 1982; \& DIO 1994, thrice cited in “Secrets”, which Shcheglov read), Y.Maeyama (Centaurus 1984), \& J.Brandt (JAIH 2014) show that all 4 real, non-astrologer Greek scientists cited above at \$G [3] knew their latitude to ordmag 1°. This twice-confirmed Rawlins discovery undoes Shcheglov’s entire inaccurate-geothesis theory. 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 disquiet, 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 luthero-unexplained errors were instead from forced latitude-uniformization-herdings, for astrologers’ convenient access to tables at each key latitude or “klima” (for horoscopes’ Ascendent and other “house” boundaries: “Secrets”, eqs.2-3), corruption inconsistent with the astronomer-scientist Shcheglov sees Ptolemy as.

“Achievement” p.262 lists 17 cities where, in the Geography, latitude matches klima. Dropping notoriously-flawed Bithynia (DIO 2012 §L4) \& way-south Meroë leaves 14 cities.

Selling or owning klimata tables for every latitude-degree was impractically voluminous. [Thus, if Almajest’s 1°/4 klimata-interval was adopted, then each city whose longest-day was closest was 1°/8 to a klima was grouped under it, its latitude made equal to exactly that city’s latitude.] Such groupings of cities under ONE latitude is explicitly attested at Geography 1.4.2 (even while justly criticized at ibid 8.1.1; 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.

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.)

Shcheglov ignores that, besides 7/5, “Achievement” tests longitude-stretching by 4/3.

Poseidonio is connected to 240000 stades by Kleomedes 1.10; 180000, by Strabo 2.2.2. What was the pre-stretch globe Poseidonia? How fruitful is the 4/3-stretch theory?

Dividing 4/3 into \$J’s 14 Geography degree-longitudes vs Alexandria, to unstretch them: those 6 cities within 30° of Alexandria show rms longitude-error c.2°, or about half a degree. The other 8 cities, several of them ordmag 1000 miles from Alexandria, likewise show rms errors 2° (as expected if due to real astronomers, like those of \$G [3]).

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 un stretched 42° from Carthage to Persepolis is correct to ordmag 1°.

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.

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.

Contra Isis, the data are consistent with early currency of astronomically-constructed, accurate pre-Geography maps, which professional astrologer Hipparchos semi-randomly ruined through dodging longitudes by lumping them into discrete klima-cubbholes 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-wide-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 §[11]).

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

The history-of-science enterprise is proud of being nonjudgemental, e.g., of superstition. It feeds on plagiarism by astronomy’s occulters of Ptolemy’s ocular klimata. Ironically, this well-intended discipline has long blinded the field to the obvious: just as his Tetrabiblos was his religion’s handbook for horoscopic interpretation, his Almajest and Geography were also world astrologer-handbooks. (The 1° fully competent translations of Almajest and Geography called each a “handbook”. Ptolemy’s exact title of what most now call the Geography was actually Geographical Directory, as DIO routinely calls it.)

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 91:949-989, 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-occult ancient mathematics, science, and observations that would otherwise be lost.

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).

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-accuracy).

[ii] Eusebius’ Sun-distance, 4080000 stades, is thus 100° (Earth-radii), in the Aristarchos-
Archimedes-Hipparchos-Poseidonios tradition that too-big-for-precision Sun-distance is a
power of 10: their 1000r or 10000r, likely origin of the very idea of order-of-magnitude.
[iii] By the correct (now generally accepted, but still snipped-at) 185 meter stade, 256000
stades is 19% high, near 6/5 of real C; Poseidonios’ & Geographical C’s. 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, 47:12-126-128, p.127. Cited to discover
Rawlins for years 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.

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 Portunodo (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) p led too “rusty”: 2017/9/5.) Having heard from neither Editor nor
Board, DR wrote the latter 2017/4/1, www.dioi.org/isb.pdf, hoping (emph in original)
to encourage communication while correcting [Isis 107.4’s] unfortunate December
misinformation, unwary Isis publication of which might have been avoided, had Cohen possessed the humility to recognize he didn’t understand Shcheglov’s (2016 December Isis) paper except that it enticingly attacked one who was upsetting Cohen by asking Isis to publish too-accurate criticisms of his fellow poles. For Shcheglov, Cohen should’ve sought refereeing from not just the usual suspects but from DIO (re, after all, a huge attack on DIO&DR) during a period when Cohen was actually exchanging emails with DIO, but preferred secrecy. Now, instead of owning to errors, he’s covering up for not just Ptolemy but for Cohen, taking you all into hiding with him. The Isis board’s non-reply so far risks being interpreted as . . . doing nothing
— about mere plagiarism, and miscomprehending the meaning of accurate and

There’s no way to interpret the board’s non-reply so far risks being interpreted as . . . doing nothing
— about mere plagiarism, and miscomprehending the meaning of accurate and
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/jio/doc; now less diplomatically transformed into paper §3 below, here] to a referee until he got a negative report safely in hand [a report again not evaluating any scholarship]. Does he imagine such transparent tactics are not noticed by serious academe? Truthseeking institutions communicate. And will not hide their demonstrated miscalculations. And don’t doubly (2015/3 & 2016/12 n.14), knowingly appropriate credit for a (needlessly) rival journal’s discovery. If Isis does not acknowledge receipt of this letter . . . it will be reasonable for previously unenlightened observers to conclude that your society is unprincipled, and you will not hear directly from DIO again.

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

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

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

The Greatest Faker of Antiquity: Still Foolin’ ’Em

[On 2014/8/26&12/22, a somewhat restrained&spare version, www.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 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/www3l.pdf, that JAHH’s initial request for cuts was made in vain hopes (encouraged by years of Gingerich-circle slander of DR) that DIO would adamantly refuse revision or cuts, thus killing the paper without JAHH being indictable for censorship. DIO instead agreeably refused to fall into that trap; thus, fleeing was JAHH’s only escape-option to effect pre-ordained rejection. The 2015/9/30 version follows, very slightly enhanced.]

ABSTRACT

Over a hundred simple independent evidences demonstrate that the history-of-ancient-astronomy subfield rulerships’ 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 (§31 below) of our valued heritage from classical antiquity’s mathematical astronomy. Though Princtitium’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 authorized astrology’s bible, the Tetra-biblos. His employers no doubt appreciated his consistent demonstrations that reality was in perfect accord (§M2) with divine celestial theories. However, for centuries, historically savvy astronomers have known that this famous 2nd-century AD astrologer-geocentrist-mathematician accomplished said illusion by indoor-faking or plagiarizing all his allegedly-outdoor “observations” of celestial phenomena, to force precise accord with indoor mathematical models (some of which had already existed for centuries) and tables computed therefrom. Ptolemy’s depredations even included stealing and mis-processing Hipparchos’ immortal 128 BC 1025-star catalog (R.Newton 1977 pp.239-242), a deed which for over a millennium polluted astronomers’ attempts to gauge precession, until Tycho in 1598 detected (Rawlins 1993D fn 141) and threw out Ptolemy’s fakes & was thus able for the 1st time in history to predict star-positions — to ordmag 1° accuracy! — 100° in advance (ibid Table 23: 100 select stars for 1701.03). But a few invincibly innocent & deeply committed (§B2; fn 11) archonial 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 imagining1 (& see Neugebauer 1975 pp.69, 119, 205; J.Evans, 1987-to-

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 damed 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 persistently attempting to prevent. After all, on this controversy this 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 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) epochal inanity.

‡

* * *

The Greatest Faker of Antiquity

2018 Dennis Rawlins
The Greatest Faker of Antiquity

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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 have been what has consistently and exclusively used for decades to do what a one-rolling cult wanted done: make a case — any case ([§2 & 13 fn 66] — that Ptolemy was an honest, outdoor-observing astronomer. The referee imagines that the bad-old-days still apply, when Ptolemites could keep doubters from being taken seriously just by portraying them as those who had committed the crime of being too few, and [by] of merely disseminating June H.J.A.— what a classic way of actually dream-contents ([fn 5]) that the view that Ptolemy faked or plagiarized is still today a way-out end-of-the-spectrum theory, a charge which unironically classifies Ptolemy-doubters Dennis Duke ([fn 23), Sam Goldstein, Gerd Gräfholff, 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 Moe-ggaard, who has gotten to realize that not only the Ptolemy’s fractional-endings orthodoxy, §2, Moegaard 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 J.H.A., 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 1846/797 letter to Neptune-affair co-conspirator J.Challis: see Rawlins 1992W [B2 & Rawlins 1999N [H2.]) And for more unintended quasi-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 wanna-be-numeric historians-of-science who have repeatedly ([B2] labelled physicists R.Newton and Rawlins as dishonest crazy incompetents and who have ([fn 5]) for years debunked doubting Rawlins ([compare to fn 6, below]), though having the courage to serially pour his work in the light 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 shunning-necessities & practices all too obvious from fn 34 & 3 fn 109."

5 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).

6 The “premier” ([Schafer 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 DIO-J.H.A. 22 2 June 2018 D.Rawlins 12 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 clefie to contend he was not ([Evans 1993 p.145; Rawlins 1999 §§7-F8.]) That the controversy has been rife with “unprofessional” acts, such as shunning, has been nationally published (Schafer 2002 loc cit), followed soon after by a direct appeal (fn 35) to the head of the American Astronomical Society, urging supervision of its shamelessly 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]) — the discreditable Ptolemy-myth is apparently multilecten, can only widen 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 ([idem] for believing what he himself privately knows ([idem] to be true. Healthy restoration
will merely (!) require the opening of discourse and minds.

B2. Ptolemy regularly fabricated data from which he perversely claimed to derive 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 as empirical data in support of this same theory.” *Ibid* p.258 also originally rejected the charge that Ptolemy stole from Hipparchos the thousand star-positions in the Ancient Star Catalog — even while suppressing citation of Delambre’s 1817 discovery of proof (§K1) that Ptolemy avoided, is, ethically speaking, original. And, then, it’s always stimulating to be diagnosed as insane by one who has for 50+ led a crusade to convince the world 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 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 Ptolemaic controversy to a scientific basis — above politics, smearing, slandering, religious devotion — the American Astronomical Society for a half-century hasn’t lifted a finger. But (fn 35) has generously given it to the skeptics. Soon after Gingrich had brought P.A.S.P. up to snuff on who is and isn’t reliable, *Skeptical Inquirer* Editor K. Frazier asked him to debate Rawlins on Ptolemy in *Skhoq*; Gingrich (1978/2/2) explained his refusal by calling Rawlins “exceedingly paranoid” for “suggesting that a cabal has been suppressing the consideration of [R. Newton’s] work on Ptolemy. When finding that Frazier had sent a copy of this helpful character-profile to Rawlins, Gingrich got miffed at Frazier! (1978/6/2 letter) — so exalted is his permanent mental state, assured of ontological immunity from the ethical standards of ordinary mortals, sinless by very definition. Like his image of his inherently unpredictable 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 sandleere — accuse someone of insincerity and suppress heresy even when wrong. (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 poles’ reverent silence on his secret actions, some even going so far as to suplicate on his behalf), Gingrich, for personal permission, for personal permission, for personal permission, on 1999/7/3, wrote to Gingerich in a 2000 referee report to *Ibis* on an article (Thurston 2002/7) appreciative of Rawlins’ inductive successes, typically promoted freemessage-in-theory while inserting an element aimed at ensuring that heretics’ Irresponsible abuse of freedom would be properly recognized as what could only be isolated from a disorder mind: “To say that the paper should not be published will only fuel the paranoid claims of the small [fn 1] group fighting Ptolemy, who believe that a cabal of [Neugebauer] ‘nuils’ [sic] are perhaps behind the point from being aired in the public. (Gingerich to P.A.S.P. p.362).” With these words, he 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.doi.org/pmi1.htm). And who was this fellow-paranoid, who imagined a closed Neugebauer “cabal”? No other than O. Gingerich! In the midst of a decade of documented — ah — “inconsistent,” character-assassination fantasy, Gingerich adds one more fantasy, soberly describing himself as “a practicing Christian” (1997/2/2 to Frazier). Obviously, being religious doesn’t mend vetticis. We recall gullible or cynical saint-mythologist Cardinal John Henry Newman’s politically dept holy war on straightforwardly ethical independent idealist Chas. Kingsley; at Newman’s 1890 death, after the standard public eulogy, Cardinal Henry Edward Manning privately mourned his colleague thusly: “Poor Newman. Poor Newman. He was a great man.” (H. E. Manning to J. H. Newman) Eminent Victorians, 1815, Manning chapter, end of part 9.)

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

Rawlins 1982C p.362 proves that Pedersen positively knew of Delambre’s crucial discovery (§K1), but chose not to impart it to his readers. [See §B3 fn 121.]

in fact had stolen it. Pedersen 1974 p.258 added (emph added): “Our general impression of [Ptolemy’s] moral and intellectual integrity would be damaged beyond repair if we had to believe that he simply derived his catalogue from a previous work by Hipparchus without the slightest acknowledgement of the fact.” Yet when it later became obvious that the theft had indeed occurred, Pedersen flexibly decided that stealing stars didn’t really prove Ptolemy was dishonest, after all (Pedersen 1993 p.559). Agreeably reverting to the 1974 Pedersen, Evans 1998 p.256 says: “At stake is Ptolemy’s reputation as an astronomer; at issue are his honesty and reliability as an observer.” At stake? Well, not necessarily — for either 1974 Pedersen or JHA Editor Evans. Each could always be counted upon to evince no negative evidence 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 deceive. Nothing can now compensate him for the hateful, largely-whispered bile heaped upon him to prevent his case from getting a fair hearing while he lived: sampling at fn 35, plus MacArthur Genius and amateur* scientist Noel Swerdlow’s branding this brilliant physicist a Velikovskian “con-man” (see *DIO* 1.1 §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’ chauvinist
bunker — to admit error in the slightest degree.

Notable features of modern Ptolemites are: [a] Consistent preference (fn 1&50; §N15) for the inherently unlikely, [b] Failure to notice that serial-proposals of a disjointed collection of odd-hoc theories, each tailored specifically and entirely for dodging the latest individual Ptolemy-implicating bullet (see also §D6 and fn 18) defies probability — as well as Occam’s Razor, which seeks the single coherent theory without confessing. (As with the late Frederick A. Cook Society, some dementia’s only cure is death.)

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/vels/wb2.pdf, DIO.11.2 (2005) [and p.30, p.33 items 5&6]. We await the century when Swerdlow or Gingerich can display in print the same profound humility as the “pompous” (fn 1) skeptical side.

For examples of downright embarrassingly improbable apologia to defend archons’ pro-Ptolemy commitments, see here at, e.g., §§K&L2, fn 37. (As well as Rawlins 1985G n.12; Rawlins 1991W §E3 & fn 99. Cumulative oscillation-oscillation historical review at Rawlins 1992V §C31.) The ultimate far-fetchness 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, for which Evans prefers a 40’ ending]; then, after sighting the stellar querry with ring 2, we read where ring 2 meets ring 3 AND THEN [“mentally”] ADD BACK. ON TO 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 error absent waves [§H1] from 1°2 to 34°41’), but would (Evans, loc cit) further commit Ptolemy to the extra bother of having to fudge over a hundred (Rawlins 1994L Table 1) stars’ resulting 25’ and 55’ endings (into 20’s and 00’s, respectively) in the manner shown at §4. As one encounters no less than 64 pages of such desperate resorts, we recall (§B2 & B3; 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 ‘Mathematical’ Ptolemy.” (In the 4 decades since then, Ptolemy’s name has become the epitome of sophistry, yet even the ancient world’s most kindhearted 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); topNewton-exiler (below, fn 35) Swerdlow 1989, on whose invariable math-innocence (repeated 1979, 1981, 1989, 2010 while reaching for Ptolemy-exulations), see above, at fn 8 or Rawlins 2018V §G2B3. Two more cases of shamefully false statements of the sort: 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., Tooher) that all outdoor astronomers’ extant ancient yearlengths were solstice-based? — Meton (Alm 3.1), Euktemon (idem), Kallippos (Tooher 1984 pp.12&214, fn 72; Rawlins 1991W §§K4, Euktemon (§N18 below)), Hipparchos (§N1 below). (Recoverable Greek equipment. 26° [§N7] of solstices survive, plus 18° [§M3] of equinoxes.)

Funny . . . Forum’s general Miles Gloriosus as philosopher: Stand Aside. I Take Laaarge Logical Steps. Of Ptolemy’s 1970s promoters, who yet today dreamly (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.

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 — that explains multiple features of this available evidence. Ptolemy’s fraudulence is that single simple theory which does so.

In case a response to this paper appears later, look carefully to see if it deals with all nine (§§C-L&K-L) of our proofs of Ptolemy’s dishonesty hereabouts (a display which would all but 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 time.

The greatest Fakism is not just making a fraud look innocent, but making anyone guilty of the fraud.
The Greatest Faker of Antiquity

2018 D. Rawlins

(See formula at ibid \(\circ\) 1). This, though naked-eye solar measures can be made to ordmage 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 (ib 22) he has broken the law of empirical science by pretending to base theory on outdoor celestial “observations” actually computed, indoors on the sky, from or to fit the very same theory (Thurston 1994P §D & Rawlins 2002V §B3-B5). Such fakes then used to “prove” said theory, a literally preposterous sham-process as Delambre saw: §B2.

To appreciate the grossness of the illegality here, consider its sheer enormity (as emphatically current, and planetary contexts throughout E. Newton 1977 for his 3 equinoxes, Ptolemy is alleging 1°-hand visual sightings of the Sun’s center on the celestial Equator at times agreeing to ordmage 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 [137/720] Alexandria Apparent Noon), isn’t a constant in time: it varies by \(-2.3\,\text{cy.} \) (A rate consistently bi-miscomputed at CalTech’s Swerdlov 2010 p.152, item 3.) So: when is Ptolemy’s Sun correct? Obviously that epoch must be 137.547° + (100°/cy/65.6°/23°/cy) = 145.5, which (as seen at Rawlins 1991W §M6; similarly below at 139 AD) is Hipparchos’ era, and is indeed the regnal epoch (Ptolemy VII Physikon Year 1) of — and the time of creation (Rawlins 2018U §O) and launch of — his Prime solar orbit (dubbed “PH at idem”). How can Ptolemy’s defenders expect to credibly deny that he plagiarized from Hipparchos, when Ptolemy’s Sun and thus his entire longitudinally contingent celestial system (not just the Sun but the Moon, planets, and stars) is correct only for the time of Hipparchos?

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

D5 This unsuble echo connects to the irony that Ptolemy didn’t fake the solar data via tables but by even cruider means. (He fabricated similarly elsewhere as well: Venus [§C1; Rawlins 1991W fn 166], the stars [fn 37], and the Arabela 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.Ixvij-Ixix) 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&R.K.) For example (by method of ibid \(\circ\), reconstructing independent Ptolemy’s 132 AD Autumn Equinox) to recover Ptolemy’s 132 AD Autumn Equinox (Alm 3.7), just add 278 Hipparchan years (365°.5/24 = 1/300 each, 6° longer than

has been claimed, e.g., in The Acquittal (§M). And in Hipparchos’ case, a ratio something above unity is to be expected since the tables were, after all, based upon his slightly flawed observations, of which he ([contra same delusional Acquittal] reports several discordant with theory & each other: §J 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°½ or 150 arcmin! (See, e.g., Neugebauer 1975 pp.895-896.)
Moreover, such an error (see math of Alm′) would carry exactly into the “Clean Dozen” unfudged and unused Ptolemy-contemporary stellar declinations δ reported at Alm′ 7.3 (the only honest Almajest star data from Ptolemy’s era). These, however, show (Rawlins 1994L §F9) that the observer’s error20 in assumed geographical latitude L was +4°±2′, about 9 standard deviations distant from Ptolemy’s −14°. (Similar disconnect for the 1025 celestial latitudes β of the Alm′ 7.5-8.1 star catalog.) So the δ were plagiarized from a contemporary21 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°59′ (§E1): a hard conflict which alone shows that he had nothing to do with the Clean Dozen (§§E2-D3) maps. Ptolemy’s known 3°-2′δ anomaly into.

E4 At GD 4.5.76 Ptolemy lists the Alexandria harbor Pharos lighthouse’s L as 31°05′, bithely 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°59′ (consistent with GD 4.5.9’s S′-rounded 31° value) why didn’t an Alexandrian22 & allegedly-outdoor observer notice he’d thus inadvertently stretched by ordmag TEN-fold the 7-stade-long embankment connecting Alexandria to its Pharos, an embankment explicitly named Heptastadion (επταστάδιον: Strabo 17.1.6)?

E5 So by carelessly23 copying disparate Alexandria L data from Vitruvius (§E1) and Hipparchos (§E4), “astronomical observer” Ptolemy adopted — simultaneously — two L over decades, have never noticed either? — which could have saved themselves a half-century of drip-torture serial-emarrassment, 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 beset by being attacked with with pseudo-scientific postures of the Merceder (Assumed latitude L)?

20 Excluding no stars when bivariate-least-squares the Clean Dozen produces the L-error cited, leading ultimately (carefully trace Rawlins 1994L §§F3-F9) to the observer’s L = 3°11′±2′ (ibid Table 3) — consistent with Alexandria’s L = 31°12′. Same process produces the Clean Dozen’s epoch, 159±8′, in fine accord (ibid fn 45) with the 10th century Suda’s dating of Ptolemy to epoch Marcus Aurelius 1 (160/7/14). Previously unnoted vastness of Alm′ 7.3’s leaflag anachronism: the Clean Dozen δ were observed c.160 AD (as just shown by G2-D2) and then merged with the Sick Six δ which were precessed-faked so inadequately for stated epoch 137 AD that their coherent 5 stars’ mean (is fn 37) instead correct for over 100 before 160 AD! There are two unknowns when analysing ancient star-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-outr ´e 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.x,331) for the latitudes’ “accuracy” (which the authors compute instead of x) are: Timocharis 0.72, Aristylos 0.18, Hipparchos 0.24, Ptolemy (?) 0.3. values which are oversmall by an ordmag. Likely-wasted-at-present wakeup to the history-of-science community: outside of DIO (§3 Table 2, or Rawlins 1994L §F9 & Table 3) no paper on the Alm′ 7.3 declinations has ever correctly understood how to find both E and x & associated standard deviations). The 2014 paper also errs in dropping near-solstitial stars for being weak indicators of epoch — forgetting that they are superior indicators of x, and thus matter in gauging the accuracy of the 4 men’s adopted latitudes for their observatories.

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

22 Yes, Ptolemy clumsily double-lists L values for sites other than Alexandria (e.g., Heliopolis-ov-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, Aristylos, 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.

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’ works (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 it! Besides stealing them both! Pharos’s flame was slightly over thirty degrees south of due west, so the two sites’ L could not possibly be the same. (Real L difference: 12·sin30° = 6.) Further evidence that Ptolemy “doesn’t seem to have allowed his eyebrows 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°9. Yet The Greatest Astronomer of Antiquity gives the identical mean motion in both works, to six sexagesimal places, and this speed is anyway precisely computed not from the alleged observational base (angular-arc-traversed-time/interval) cited at Alm′ 9.10 but from the period-relation 52200/1680224′ found at Alm′ 9.3. [Full calculation of 5° discrepancy: Rawlins 1987 p.236-237.]

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

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

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], 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
foreign eclipse times could not have supported the longitudinally-stretched geography (§G4) he borrowed (with credit)\textsuperscript{25} from Marinos of Tyre. Instead, Ptolemy’s vast opus provides (GD 1.4.2) but one\textsuperscript{26} example: two longitudinally much-separated reports — 500° old! — of the famous Arbela — 330°92°0 lunar eclipse’s start, saying it was seen there at 23° and in Carthage at 20°, thus proving that the 2 places are 3° or 45° apart in longitude.

\textbf{G3} However, Pliny earlier reported the same data very differently: 20° (8 PM) for Arbela (modern embattled oil-city Irbil) and 18° (6 PM) for Sicily, whose west end — major city Lilybaeum — was part of the Carthaginian empire, and of longitude similar to Carthage. Modern calculations\textsuperscript{27} show that non-astronomer Pliny was quite accurate, while the Great Astronomer of Antiquity was amazingly wrong, over 2° off for Carthage, 3° off for Arbela. The former error nearly equals the entire actual 2\textsuperscript{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° Carthage and 3° Arbela. How explain such an entertainingly disastrous fabrication? Start by consulting Pliny 2.72.180 on the −330°92°0 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 “2\textsuperscript{2/4} hour” after sunset, or about 20°, which is the very time Ptolemy gives for the Carthage report. Why? Well, look carefully at the Pliny passage cited: by a fluke of grammar, “secunda hora” appears nearer in the sentence to “Sicilia” than to ‘Arbelam’.

\textbf{G4} But how did Ptolemy arrive at 23° for Arbela? Since Gessollin 1790, it has been obvious that multiplication by an expansion factor (Diller 1984 §C5) had been applied by Marinus or Ptolemy or their source to a prior map’s accurate longitudes, creating the 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\textsuperscript{1/4} east of Carthage, then expansion by 4/3 would produce 3°, the very gap — the very wrong gap — Ptolemy reports. I.e., typically for him (and his defenders) the conclusion was established ere the evidence was engaged.

\textbf{G5} He simply added this 3° to 20°, thus arriving at his fantastic 23° time for Arbela. \textit{NB:} This solution adds powerful new evidence favoring the theory (still-foolishly-doubted; §§G4, Rawlins 2008Q §J & Rawlins 2008S fn 13&45) that the GD fatefully corrupted the accurate prior case by map 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.

\section{STAR CATALOG TESTS AND ANOTHER DOUBLE: PTOLEMY AS LOSER-MAGNET}

\textbf{H1} Had Ptolemy observed the Ancient Star Catalog via armillary astrolabe (described at Alm 5.1) with its elliptic ring off by his notorious −1°.1 mean longitude error, the real and instrumental ecclesials would be tilted by 1°.2 vis-à-vis each other (since the instrument rotates about the equatorial not ecliptical pole: as we can see from, e.g., the educational paper model Evans has helpfully disseminated), so (Rawlins 1982C p.361 & Fig.2) we’d find error waves of amplitude 1°22° in the Catalog’s latitudes β (cosine waves: \textit{ibid} eq.4) and northern longitudes λ (sine waves: \textit{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 Ptolemitian 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 \textit{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.

\textbf{H2} If this be true, ancient c.1990, even to the most religious, that many Ptolemy stars were Hipparchos. However, no archon was ever going to admit in print the plain truth: the establishment had been blinded by proof that its challengers had been right all along — that Graßhoff’s test had now unexpectedly surprise-vindicated the long-loathed Tycho-Newton Rawlins notation 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 taken seriously.) The post-Graßhoff era has been especially fertile for indiscriminate fallacy apologies by Ptolemy’s (selectively) malleable modern choir, as the politically ambitious realized that the \textit{JHA} would ever so gratefully\textsuperscript{28} publish anything that muddied the clear evident situation, in order to save archons from facing apt appreciation for decades of falsely denigrating now-vindicited scholars: simply pretend vindication either never happened or isn’t 100.0000000%. 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 shuts 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 at what Graßhoff 1990 hasn’t proven that all\textsuperscript{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 1\textsuperscript{st}-hand observation of all 1025 stars. And argument [2] is on the logical and ethical level of defending a bank-robber by pleading to avoid having to publish anything is 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 \textit{JHA} for decades to come.

\textbf{D2} 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.)
I  

CATEGORIES OF FRACTIONS: JEKYLL’S SLYDE

HYDE COVERUP

— KNOWING DESTRUCTION OF DATA

I

The Ancient Star Catalog (Alm 7.5-8.1) has an obvious excess of 0° endings and 30° endings in the latitudes \( \beta \), due to ancient Egyptian and Greek proclivity for expressing non-integers by using inverse integers: “unit fractions”. (Cause of both excesses detailed at Rawlins 1994L §B4.) But the most common ending for the longitudes \( \lambda \) is 40°.

II

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 \( \beta \) unchanged, while updating all Catalog longitudes \( \lambda \): 1°/century-precessing them by adding 2°2/3 (like Evans 1998 p.272) to create arc-tangent-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 \( L = 55\,\text{°} \), impedes these difficult Sgr stars’ availability. (By recording Fomalhaut, Tycho actually went down to within 2°.6 of the horizon: Rawlins 1993D Table 17.)

III

Most critiques of Ptolemy’s chicanery point primarily to the excess of 40° endings (vs 0° endings) in the Catalog longitudes \( \lambda \), but (thanks to the evidence’s imbalance) the spectacularly greater number of 10° endings than 30° endings. (Before Ptolemy added 2°40’ to Hipparchos’ \( \beta \), these were 30° and 50° endings, respectively.) Looking naively at the tabular distribution, the fact that 30° are the least frequent endings is bizarre, since 30° is nearly the most frequent latitude ending, as it should be. Indeed, for the latitudes \( \beta \), 30°s are roughly twice as common as 10°, but this is reversed for the longitudes \( \lambda \), by far the strongest confirmation of R.Newton’s hypothesis for explaining the longitudes’ odd fractional-endings distribution.

IV

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 Ptolemaic 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.

V

So R.Newton (§J2) explained why longitudes \( \lambda \) are near-bereft of 40°s and 45°s’ (only 32 5 in all: five 15°s, four 20°s, 10 31°s; 30 35°s, but this is reversed for the longitudes \( \lambda \) 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 Ptolemaic 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.

VI

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 \( L = 35\,\text{°}\,39\), impeded these difficult Sgr stars’ availability. [B] Shevchenko 1990 p.194 discovered for a specified half of the zodiac, stars’ \( \lambda \) exhibited no particular excess of 40°s. Later, DIO 10 (2000) fn 177 tested Gemp-sgr (roughly Shevchenko’s range) and found that — excepting Sco, whose prime stars’ \( \beta \) reach atypically far south for the zodiac — these stars’ original Hipparchos’ 00°-excesses were not in ecliptical longitudes but in polar longitudes. The novel and insuperable impediment thus created for Ptolemy’s defenders is found in the footnote cited.

VII

The root of the persistence of the embarrassing-long (given the evidence’s imbalance) “debate” over the Ancient Star Catalog, is that sneaky (§J4) stealing ordm 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.

VIII

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 stars (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 \( L = 55\,\text{°} \), impedes these difficult Sgr stars’ availability. (By recording Fomalhaut, Tycho actually went down to within 2°.6 of the horizon: Rawlins 1993D Table 17.)

IX

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 succession of evading skeptics by putting the blame on even even accent Ptolemy’s use of the word “we” when describing purported 1°-hand observations — a tack which wishes to refute the accusation that Ptolemy faked the Catalog by instead proposing that he plagiaristically stole credit for another’s work, theft without, as Pedersen puts it (above, §B2), “the slightest acknowledgement” of his actual source.

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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 \( L \).

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 the Syntaxis has 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 early 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.”)

J2 The educational observation we may end with, regarding the timeless state of the modern history of ancient astronomy community, is this: its fiscal rulers can read all that you have just read, argued, both finding text a digital copy of a date in the ancient texts and finding text absolutely nothing of note. [A] Ptolemy has done absolutely nothing of note according to [B] DR should continue to be non-cited for [1] his witchcraft (fn 35) in co-hyponotizing scholars (fn 1) into realizing Ptolemy cheated, and [2] exposing the vile tactics of archons who’ll never admit they were wrong to slander 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 early 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.”)

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 continue his ignorance of the shunnee’s output (as he must also continue his ignorance of all others’ output). This is the only way he can avoid ever the decipr de can stop. As we get to the point where evidences CENTRAL TO THE FIELD (e.g., $\frac{11}{17}$-N17 and N18; fn 1, 7, 47 item [1]) cannot be openly discussed without fear of archons — or editorial submission to such (fn 1) — we’ve descended into knowledge-destructive sociopathy.

35 Among JHA circle-hard-talk compliments toward R.Newton and his solid mathematical analyses have come in support of Ptolemy’s "incompetent" (A.Aaboe) and ignorant (Toomer 1984 p.viii), and (all Swerdlov) "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 1.11 § [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 members engaging in such ridicule. Instead, we’ve had 4 decades of well-reasonable non-comprehension and equally inexcusable inaction. And the proposal inadvertently trades a 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."

36 The 1st time culists 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 Hipparchus data) of any star declinations δ found in Alg 7.3. Various modern scientists have noticed that Ptolemy “proves” his false δ/cy precession from his 18-star sample using the "SickSix" (Peters & Knobel 1915 p.15; Peters & Knobel 1915 p.15; R.Newton 1977 p.273, §§ 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 ($\frac{11}{17}$) by acquiring Double-Peek fail-safe control over this paper’s revelations. (Ref-report to JAHH: “If DR revises the manuscript, I would be happy to look it over.”) Just as Gingerich did with R.Newton forty-seven years ago [now 50!]'— details at Rawlins 1994S §B3 [a]. After all: must protect even ‘ulit-now-underfied Theland and the antipode Land-of-Oztrollya [§4 §A2] from the full truth about the integrity & ability of Ptolemy and his too-never-too-far-relations organ.

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41 Similarly, a recent paper (Brandt et al 2014B; see also fn 20) tries to deny the certainty of Ptolemy’s fabrication (from Hipparchus data) of any star declinations δ found in Alg 7.3. Various modern scientists have noticed that Ptolemy “proves” his false δ/cy precession from his 18-star sample using the "SickSix" (Peters & Knobel 1915 p.15; Peters & Knobel 1915 p.15; R.Newton 1977 p.273, §§ 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 ($\frac{11}{17}$) by acquiring Double-Peek fail-safe control over this paper’s revelations. (Ref-report to JAHH: “If DR revises the manuscript, I would be happy to look it over.”) Just as Gingerich did with R.Newton forty-seven years ago [now 50!]'— details at Rawlins 1994S §B3 [a]. After all: must protect even ‘ulit-now-underfied Theland and the antipode Land-of-Oztrollya [§4 §A2] from the full truth about the integrity & ability of Ptolemy and his too-never-too-far-relations organ.

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 transted 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 scientifically in Rawlins 1982C), all these uncataloged stars were invisible to Hipparchos, who observed 5° north of Alexandria, so that his anticentric-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 anticentric-circle testing, speculating that because no previous mass-star-cataloger known to us had resided so far south as Ptolemy, there were no constellations to which he could attach stars in the 5°-wide strip of sky he could see but Hipparchos could not (and which no hypothetical early southern constellation had filled), so we must excuse Ptolemy — excuse him, that is, for not doing what The Greatest Astronomer of Antiquity himself actually says he did, namely, record all visible stars (Alg 7.4; Toomer 1984 p.339): “we observed as many stars as we could sight down to the sixth magnitude.” Evans’ dodge typifies modern Ptolemiens’ death-agonyms: allbing one’s extra-visibility-tearers to ‘nd other too often requiring reposition (as from position function 30, 37, & 42). I.e., even if one accepts Evans’ argument, it simply exchange a charge of plagiarism against Ptolemy for a charge of lying. But Evans’ theory fails anyway since...
The Greatest Faker of Antiquity

2018 D.Rawlins 29

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 thieft. 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 make his putative observations' declination-range deceptively look ≈ as if the observer were at Hipparchos' latitude L instead of where Ptolemy's

Various of the non-cataloged Alexandria-visible stars were conveniently attachable to nearby constellations. And we know that Ptolemy was (or copied) a star-attacker: Alm 7.5-8.1 lists dozens of "informative" 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 Argor stars aren't in the Catalog (though unCataloged e 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' Rhodes, since all were (see idem) of such dim μ as to be beyond Hipparchos' in-practice mean magnitude limit μ0 for capture. Even more peculiarly absent from the Alm catalog are α and β Gr. Both of pre-extinction magnitude m ≈ 2 — i.e., of Big Dipper prominence! — and quite visible (§K2) to Ptolemy at μ about 3 and 4, resp (though at all hours below Hipparchos' horizon), they could have just been set aside as a new99 constellation. After all, [i] There already was a two-star constellation, CMi (PK847-848); [ii] Ptolemy was inventor of the new99 astromer Antinouos, which he formed c.130 AD from six9 stars "around" Aql (Toomer 1984 p.357).

K2 Stars α, β, and δ111 Gr are missing from the Alm star catalog, though all were visible from Ptolemy's Alexandria (μ = 3.3, 3.9, and 5.2, resp) and were attachable to nside Pto, as suggested by Ptolemy's including (into PsA) γ Gr (PK1022), a star only 5° from ι PsA (PK1021) versus 10° from α and δ Gr, and 7° higher than δ Gr in declination, which connects to why γ Gr was visible to Hipparchos while α and β Gr were below his horizon, and his δ Gr's μ = 7.1. So, why was γ Gr Cataloged while the other 3 Gr stars weren't: [1] the three's distance from Ptolemy's PsA? or [2] Hipparchos invisibility? Answers: [Gaps exceeding 10° between constellations' prime stars aren't rare. Cep: γ/β (PK76&77) 11°. Aqr: β/κ (PK632&636) 12°. Peg: γ/κo (PK316&318) 17°. Hya: β/κ (PK916&917) 22°.] So the no-available-constellations argument is slaughtered by [2] the obvious explanation for γ being the Catalog's sole Gr star, namely: of α, β, γ, and δ, only γ was visible to Hipparchos.

the SickFive, whose weighted mean error vanishes c.59 AD — since he faked it to co-prove false E/ly perception from Hipparchos' atypically very-inaccurate Arcturus δ which was accidentally correct for a time later than his actual endpoint by (you guessed?): c.50 . To emphasize this glaring giveaway by summation: if the SickSix sample were actually observed in 59 AD (& its SickFive do indeed have near-null declination-error for that date), it is remarkable that the lone Ptolemy star (Arcturus), whose δ = 40° north and μ > 5.0, should just-happen-to also-be the lone Hipparchos star whose δ-error also goes null c.50° later than Hipparchos. (To be exact, 52° after Hipparchos; 56° after LMO.) Hipparchos' μ0 was a bit dimmer than 5: [§L3. For his deep south sky, virtually every identifiable star brighter than μ = 5 appears in the Catalog: note: this is about the μ (Rawlins 1993D §4) at which Tycho's normally high star-cataloging accuracy begins to fade. (Tycho had 14% less sky visible to him than Hipparchos, so he had to go a bit deeper in μ to approach his goal of netting 1000 stars.)

Evans 1984 had argued for Ptolemy's originality with the equant, but only 3 later is adding his UNoriginality to squirm out of a religious paradox. See Swerdlow similarly at Rawlins 1992F in 43.

Not just the date argues for Ptolemy's Serapiac temple being the source of Antinous' interminent celestial immortality: there is a copy of that very temple in the Canopic Vale of Hadrian's Villa, outside Rome. Go to www.doi.org/cot.htm#rhv for further analysis, plus Rawlins photos of Hadrian's Canopic temple replica (with pools/dive stone coracle), as well as of a classical-era Antinous statue (Early Hadrian).}

42 Likewise, Schaefer 2001 proposes that atmospheric aerosols instead of rocks blocked Ptolemy just enough to fool us into mis-concluding that most of the catalog was observed at about Hipparchos' Rhodes Island L = 36°N. This requires assuming (contra Pickering 2002A §F) that Schaefer's modern Alexandria daytime [mean] atmospheric stars 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 antique circles of the stars collected by Hipparchos and by Ptolemy) his argument inevitably requires that Alexandria's aerosols were much greater than Rhodes' — 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, ibid) choosing an extinction value of 0.23 mags/atm makes his argument (contra Pickering 2002A §F) further demonstrates that bright stars were visible on the horizon since achronychal effects cannot even be defined at

40 Has it previously been noticed that not one of Antinous' six stars appears in the Hipparchos Commentary's Aql, or, indeed, anywhere else in that work? The stars were PK295-300; or from Bayer: η, δ, ε, 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 1992F in 44) and presumably to thank the emperor for his visit and for favoring (perhaps initiating) imperial sponsorship of the Serapeus astrological-medical superstition the Canopic temple specialized in.
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 \( \mu \) and [b] apparent altitude \( h \) above the horizon. If, in Ptolemy’s list [b], all the stars above \( h = 6^\circ \) are in the Catalog while all below are not, then the rocks aren’t in the 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 \( L \); altitude \( z \); above sealevel), Hipparchos’ list [a] exhibits a startlingly clear\(^3\) split at a post-extinction magnitude \( \mu_0 \) slightly less bright than 5 (obviously his effective limit for capture): the stars dimmer than \( \mu_0 \) are not in the Catalog, while those brighter than \( \mu_0 \) are.\(^4\) Comparing these sensible results, to those gotten from applying the same Magnitude Split Test (DIO 9.1 1999 p.2) to The Greatest Astronomer of Antiquity’s Alexandria, will (fn 43) give any scientist a hearty upchuck.

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

M1 How did too much of the academic establishment get sucked into promoting astro-
nomical 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?\(^5\) — but astrologer-mathematician Ptolemy was not a scientist. Not empirical.

any other altitude than \( h = 9^\circ \); 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 1° magnitude stars (pre-extinction \( m = 1 \)) were visible on the horizon in antiquity. (In the exchanging-frauds tradition noted at §K1, some loyalists disbelieve this, thereby [ibid fn 93] assuming Ptolemy’s “horizon-stars-dishonesty [in order to argue] his Catalog-stars-honesty.”)

But, if we assume Evans’ preferred (0.20 mags/atm) modern-model opacity, then \( h \) magnitude stars’ visibility on horizon entails (ibid §L8) ancients’ eyesight perceiving 12° magnitude stars (\( \mu = 12 \); and \( \mu = 12 \) magnitude stars similarly entail ancients’ seeing \( \mu = 11 \) magnitude stars). So: why didn’t Archimedes beat Clyde Tombaugh to the discovery of Pluto?\(^3\)

Define split-Vagueness \( V \) in §L3’s list [a] as: dimnest Cataloged star’s \( \mu \) minus brightest nonCataloged star’s \( \mu \). Testing Hipparchos’ \( V \) (\( \gamma \) Ara vs \( \kappa \) 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, §17) [Evans 1987, #2], 0.20 (Evans 1987, #1), 0.23 (Schaefer 1981a) = 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, \( V = 5/4 \) (§L Sgr vs \( \alpha \) Gru). And for mags/atm = 0.23, 0.3, \( V = 4/5, 3/4, \) resp (\( \gamma \) Ara vs \( \alpha \) Phe in both cases).

Among those tested by Rawlins 1982C Table II, the only major star (\( m = 3 \)) that seems unambiguously to be missing from the Catalog is \( \mu \) Vel (\( m = 2.7, \mu = 3 1/4 \) 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 \( \mu \) Vel, high by \( 3/4 \) in R.A. (3° great-circle), but in declination fully accurate to ancient precision.

The half-century Ptolemy Controversy should have been over in half an hour — had all participants amiably cooperated at the outset and sat down together to compare data and enlighten each other. Had defenders realized early on, before positions hardened in ignorance of, e.g., the significance of Ptolemy’s geographical latitude \( L \) (§5) and real ancient scientists’ high-accuracy achievements (§M3), participants would (Panglossiany 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 with as 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): if what 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 de-
fended by reason. [d] Did damage to Ptolemy’s sacred-grant-cow value trigger the stunning \( (\$) \) of R.Newton’s valuable insights? With the Almajest as \( [i] \) the central surviving an-
cient work on mathematical astronomy but \( [r] \) suffused with fraud, a grant-raising problem was presumably feared (perhaps needlessly; \( [j] \), leading to attribution Newton’s but (for 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 Ptolemist. [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 false in itself and evil 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 (§E1) 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

the 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.  

DIO-J.HA 22 Æ2 The Greatest Faker of Antiquity 2018 D.Rawlins

31
Ptolemy as the quintessential or ultimate ancient scientist — knowingly rejecting the inconvenient fact that his genuinely empirical predecessor Hipparchus, though also motivated by astrology (at least in the period —157 to —145), published theory-discordant data, solar (fn 15), lunar (Alm 4.11), and stellar (fn 47). The attempt to alibi Ptolemy by wrenching academe’s view of ancient astronomy to fit him has caused as much damage to modern scholarship (§§M1&N) as Ptolemy visited upon ancient and (above, §A) medieval.

M3 “Acquittal” adds that ancient astronomers “were mathematicians who concerned themselves with proof, rigor, logic, and consistency rather” than with observational accuracy. Gingerich 1976 p.477 approvingly quotes Neugebauer 1975 p.108, “It makes no sense to reject the ancient concept to condemn the ancients for the accuracy that they could not have had if 154 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 solar diameter within c.2% (59 Earth-radii: Alm 5.13)? Or find their observatories’ geographical latitudes to ordmag 1′? (See [I4 Table 1] or Rawlins

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 cack 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 imperial eclipse 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 the experts’ judgements regarding the precision achievable by ancients, not-very-implicitly asking: So, Missrs. 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 parallel when reducing each observation — a theory that decades later also proved just as neatly fruitful for Regulus’ hitherto-inexpiably 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 unrealized, 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 debehased himself FOR NOTHING: these random goofs can’t alibi Ptolemy’s systematic fit-to-theory errors.] All while non-convicting Hipparchos’ usual accuracy or Rawlins 1991W fn 288! No-better-demonstration-could-be-wished [1] of the JHA Assoc Ed [now Editor!]’s citations integral (DIO 8 1998 p.2) & [2] of cultist disconnect between evidence & classification — when to other collapses the latter standstills entirely, an inverse similarity 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 1974’ passage when to JHA Editor/S. 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′? Whence arose a Greek stade-length consistent with knowing the Earth’s size to 1% (fn 49 below)? Was it just as a series of miracles that all 4 surviving ancient collections of star declinations are (Rawlins 1994L §§F5-F9 & Table 3) consistent with each of the 4 observers knowing his latitude to ordmag 1′? From where (Martian visitors?) did the ancients obtain all 3 lunar months to (Rawlins 2018U §D) one part in ordmag a million or better? For the synodic& draconitic months: nearer ordmag 10 million! (Most of these accuracies were unknown before DIO.) It would be more realistic for the ancients to be able to have gotten such numbers, if researchers had merely tailored data to previous values. Back in 1979, did Ptolemites ever take 5 seconds to contemplate such a self-evident & devastating point, ere committing to an obviously uncertain position so deeply that [fn 34] there could never again be a chance of turning back: with the courage of a lynching-mob, gang-smearing as an incompetent crank (fn 34 & fn 2) prominent Johns Hopkins physicist R.Newton, whose analyses of Ptolemy employed math which historians-of-science couldn’t even understand much less perform. Before such fateful investment, did they even know (in 12) of the inescapable (§2F) 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 (§§N) less than nothing. Hopefully, these considerations will warn today’s budding archons how a mess establishment — initially from preconception and careless science, then in heedlessly hot outrage at heretical challenge (Swerdlow at §B2, longtime JHA Editor M.Hoskin in §D) — virtually 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 — without 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 — virtuously 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 remarks and fanatics of the history-of-science discipline, their plans can hardly be improved upon. (Which actually would be quite unjust, in that such men as Neugebauer, Aabo, Briton, and others are brilliant despite their blindspots.) Think this too strong? Well, if you have the independence to actually read what follows in this section, you’ll be taking evidence before making up your mind. (We trust this doesn’t break a cult principle.) The specific delusions that have long been orthodoxy in this unreal field:
Assuming that Ptolemy got his astronomical elements from his "observations" — the central JHAD-inversion of this controversy. Contra: [1] A particularly penetrating yet simple demonstration of the truth is by Thorston 1994P ($3D$), who noticed 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’ $\text{Alm}$ mean synodic motion is accurate to ordmag $1/\text{century}$, yet the “observations” on which Ptolemy’s proof of it is purportedly based are off by ordmag $1$ (Rawlins 1987 p.237). [4] The $\text{Alm}$ proof of Mercury’s mean synodic motion was already shown above ($\S2$) to be pretense, by one who had started with the answer and (if the Canonic Inscription’s elements were also based on alleged observations then he) TWICE — differently — fabricated the “observations” to prove it. [5] The centrally phony aspect of the whole $\text{Alm}$ is seldom understood (PHuber the happy exception — Rawlins 1991W fn 224), namely, its near-exclusive use of $n$ observations to solve geometrically for $n$ orbital elements. From the often excellent results sampled here throughout, we realize that ancients’ actual historical evolution towards accurate elements obviously involved repeated adjustments from numerous observations. As a mathematician not a scientist, Ptolemy never noticed the value of overdertermination — where the number of equations of condition exceed the number of unknowns sought.

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

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

Accepting that Greek astronomers were not primarily empirical ($\S3$; DIO 1.1.11 fn 24; Rawlins 2008R §A1 & fn 20). Among objections beyond the a priori: far too many extremely accurate ancient measures survive ($\S\S$M3&N1, fn 8 [3]).

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 Neugebaurians 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.)

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

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

When a ball is tossed upward at 0 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 jih-JHA-publicized every-scientist-Ptolemy-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 4th error 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 disbeliefing Greeks’ ability to measure solstices accurately (Swerdlow, Evans, Duke) noticed that the newly available papyrus P.Fouad 267A ($\S$N9) has tried to enlighten them by directly surprise-testifying to an ancient solstice which was accurate to ordmag $3^4/3$? 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 $5^0$ off, when in truth its error was only $1^0$: 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 $7^3$ (consistent calculational conclusion of Britton, Newton, and Rawlins: summarized at idib §B4), while in spite of $6^h$ rounding, the errors in recoverable ancient solstices (one by Kallippos; one by Aristarchos; two by Hipparchos: idib Table 3 & eqs.1&2&27&4) are $+3^0$, $0^0$, $+2^0$, & $-1^0$, resp, indicating that Hipparchos’ rms solstitial systematic error ($1^0$) 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 arcm in 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.

It will be convenient to here list in one place prominent instances, of the pile-on pile-on pile-on...
Historians-of-science unexceptionally ignore the perfectly Occamite 3-for-349 hit—within about one percent in all three cases—of the perfectly Occamite 3-for-349 hit—within about one percent in all three cases. 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 values was widely adopted: Eratosthenes’ (really Sosistratos’: 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.

§§ A5&A7) by taking it to the extreme thought-experiment case where horizontal light rays’ curvature is 1/6 Earth’s, thus the lighthouse-flame method’s result is expanded by factor 6/5, while the double-sunlight method’s result is contracted by factor 5/6. However, to verify this, one must be able to follow the mathematical physics. There is as yet no evidence that any historians-of-science have ever done so, despite the 25/3 of a century (summary and citations going back to 1982 provided at Rawlins 1996C fn 47) since DIO published the hypothesis that atmospheric refraction is a smooth function that navigators’ familiar formula for the horizon’s “dip” has been based upon identical 6/5-mathematics for over a century?§ These 2 DIO matches exceptionally are achieved without judging the stade at all, but just by adopting the standard 185 meter value. [Thus serving as the final, controversy-ending proof that 185m was indeed the true length of high antiquity’s stade.] Our achievement here may be compared to the controversy’s endlessly wheel-spinning meteorological literature: sniffing & shifting through ancient lore in search of hints of oddball stades, which of course existed all over the place before the Potelomess 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), measuring each stade against actual data within about ±5 meter (namely, Eratosthenes’ 3997000 m, a Strabo-attested 10000 stades interval (at 700 st/degree) accuracy enhanced by factor 6000/185m (about 1/4, the stade = 185m is the cube of 40, 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; §§ fn 111) to define the stade so that C = 60 m or 216000 stades. I.e., sexagesimalization (triple division by 60) of Earth-C ∼ 40 million meters produces a geometrically correct “sexagesimal” stade of 185 meters, fine for 600 stades/degree, but not for Sostratos-eratosthenesian 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 athletics ran in various cultures, there must have been a general-gradation scheme: gr.250-marathon, 200-sprint, 185m-leisure.) Table 1 presents for 5 ancient stades the disparate lengths of the traditional 1-stade footrace, where the sole 185m one is also the only one (Athens, reconstructed + 143: Gibbon Decline . . . Chap.2 [Mod.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 ordingam 1000 km. Given that the Alexandria-Meroé arc’s curvature is apt to a meridian circle of C ≈ 39870000 m, a Strabo-attested 10000 stades interval (at 700 st/degree) seems within the possibilities (within 40m, 1 part in 1.1, of that the cities’ separation = 1851.4), the inhabitable Nile Valley’s narrow sinuosity and the non-trivial longitudinal difference between Alexandria & Meroé: how would the survey team measure the 1578 km latitudinal N-S difference between the two cities, in order to divide by 2 3/8 60°, to find C? If the proposed survey was thorough & scrupulous enough, we may speculate that this would’ve been possible, though quite laborious. (But: more laborious & expensive than Alexandria’s non-speculative achievement of erecting & maintaining the Lighthouse?) Surveying had been a vigorous science in Egypt for over 2000 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 1993G 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 arc, could be measured without being interrupted by the Nile or rough-ground mountain at any point by the allay south to Meroë’s latitude (already measured by Philo: § 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 Meroé’s Pho-lo-determined latitude (16° 57′), 14°14′/stade, the stade would have been indicated to be 1578000m/(14·14/60) ≈ 185m. [Note added 2018/4/24. DR’s researches have ultimately realized a hitherto-unperceived 3rd century BC Greek science dichotomy: adoption of Babylon’s division of the circle into 360° (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 [B.] 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 §4.) An obvious & devastating point, never previously emphasized for its implications: not a single trig table survives in any form from Seleukid-era Babylon. Babylon 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°, is also the only L actually recorded in Greek records, not a word on L and there in any uniform 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 §33), in which equating 9660 synodic months with 781 sidereal years (the interval between two attested local-midnight lunar eclipse records, Babylon — 719/3/8-9 & Alexandria [Heron] 62/3/13-14) neatly recovers all 10 digits of Ptolemy’s previously mysterious final lunisolar equation (Rawlins 1996C eqs.21-31): 8523 tropical years = 10546 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 irreducible evidence for ancient use of sidereal—tropical transformation of period-relations (Rawlins 1996C eqs.26-27), a process later extensively employed in DIO 11.2, which allows (here, in fn §4 [i]) reconstruction of Venus’ accurate pre-blunder synodic motion.

N15 Among the most egregious of all inversions of ancient astronomical procedure: our uniformly on-the-nose huzzle-cycles-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 [§3B] 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 PHuber, likely inspired by Neugebauer [also primarily a mathematician, like Ptolemy] that is so incoherently contrary to sensible scientific practice as to gain special popularity among gaping cult-minds, thirsty for any refutation of unserving outsiders’ proposals, thus not just suggesting but insisting-upon JHADists’ pure speculation that very long-period jussi must have originated for all 6 much shorter relations, & no other hypotheses need apply. (The truth was 180°—opposite from this rigid position, of course: ancients’ more-convenient short relations descended from less-handly 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, t₁ to t₂, so that the error in deduced P = (t₂ − t₁)/N caused by the errors in t₁ & t₂ is trivialized by the enormity of N. (How else could the ancients determine [Alm 4.2] the synodic month correctly to within well under 1 time-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 of a remainder of fewer, of the cycle’s imperfection), & (see item [D]) [ii] tropical, not sidereal. [D] Genuine, huge, measured integral planetary periods in Ptolemy’s Planetary Hypotheses (Neugebauer 1975 p.906 Table 15), mostly on the order of 1000°, verifying to all but splice-dreaming JHADists that records were long recorded in cycles. 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 now 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° (5′ short of 15 synodic revolutions) and 47° (4′ beyond 22 synodic revs), to produce 79° (1° short of 37 synodic revs) with the advantage of reduced (but still non-zero) remainder. (Neugebauer or source mistakenly renders the three-day 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° 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 ordmage) without such needless fiddling, & upon which (Rawlins 2003J) all Alm 9.3’s short periods were ultimately based. [F] The Alm 9.3 Jupiter 71° tropical cycle’s superficially—inexplainable big remainder (while the well-known 83° tropical cycle was available with a 50-times-smaller remainder) proves its historical descent (Rawlins 2003J eq 40 — eq 44) from an unremarked 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 §34-137) 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° (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 §N16 lacked such divisibility.) Consult esp. the half-dozen near evidences and fits (Rawlins 2002H §§C3-39) backing the theory that the draconitic month was determined by Hipparchos, by using the very same back-end — 1401/27 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 [whichinsta-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-ordmage—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 echoes 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: in 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 1st BC.

N17 It is revealing that the JHAD clique has not only failed for decades to understand the central (§3M) significance of the Almajesty’s three lunar periods’ high accuracy — but to then let shunning dictate refusal to cite the 1st solution (§N16) anyone has yet achieved, for how these periods were obtained? That’s non-citation with an impressively unanimous lockstep. . . . (Is there a prize we don’t know about, for this special brand of perfection?) To summarize, cultists keep secret from JHA readers and the larger public BOTH: [1] the historically revolutionary empirical significance (§M3) of the inescapable fact that all three ancient lunar-speeds are micro-accurate, and [2] the eclipse-cycle source (§N16) of such accuracy. (Let Animal House’s Dean Wormer trump THAT Double Secrecy.) And don’t miss the saddest&sadist-masochist part: all disbeliefing archaeologists are religious Babylonians, but their coherent disdain for target-heretics trumps even worship of Babylon, as their rabbinic 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 solar-solar knowledge by any civilization: observation and study 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° = 781° case does survive (§N14 & §3 [J33]). [b] Is it JHAD-banned to use intelligence to induce Greek astronomers’ possession of the §N16 front-end eclipse-records? — just as DIO induced (§N19) Hipparchos’ −157 solstice & adoption of Kalippic motion, both of which (after same JHADists’ scoffs) turned out (§N19) to be anciently attested. In this field, what is intelligence for, if not to 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° Great Year, Aristarchos originated the “Babylonian” month, 29191/0°’50’, decades before Babylon (§3 SG4; or Rawlins 2002Ea q.4-8). [2] DR’s 2001/6/27 British Museum lecture showed (ibid: eqs.9-11; or §3 SG5) Aristarchos applied to this the Metonic cycle (235 months ≈ 19°), resulting in his Metonic “gregorian” year \( \text{Y}_{\text{MA}} = 365\frac{1}{4} - 15/4868 \) (fatefully off by 6°, virtually same as Metonic cycle itself). [3] Years before item [2] was discovered, the matching recovery of exactly this yearlength was discerned in Vatican-held ms data listed under Aristarchos’ name on vat. gr. 191 fol. 170 (data at Neugebauer op cit p.601). \( \tau \approx 0' \xi \approx \beta \) or \( 365 \approx 20' 60' \); if expressed as the continued-fraction \( 365\frac{1}{4} = 1/(20/2 + 26/0) \) (Rawlins, op cit eqs.12-13), this is \( \text{Y}_{\text{MA}} = 365\frac{1}{4} - 15/4868 \), verifying above item [2]. All without altering any Vatican document number and much aided by Neugebauer 1975 p.602’s perspective interpretation of \( \xi \) as sixtieths. [4] The sidereal-year companion data, Aristarchos’ \( \tau \approx 0' \xi \approx \beta' \) or \( 365 \approx 10' 4' \) from vat. gr. 381 fol. 163 (Neugebauer 1975 p.601), we write as continued-fraction \( 365\frac{1}{4} = 1/(1 - 10 - 1) \) (§3 §G2; Rawlins, op cit fn 14-15), yielding sidereal year \( \text{Y}_{\text{MA}} = 365\frac{1}{4} + 1/125 \) (good within a few time-seconds), again altering no Vatican-ms digit; and, again, a hit: the interval from Meton’s −431 solstice to Aristarchos’ −279 solstice is just 152°. [5] The difference between \( \text{Y}_{\text{MA}} \) & \( \text{Y}_{\text{MA}} \) IS PRECESSION, suggesting geomobiolists Aristarchos’ apt pre-Hipparchian 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 unstrippped data encourage. N19 Rawlins 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 Kalippos’ \( 365\frac{1}{4} \) year solar motion. Both his −157 solstice (a different day: Rawlins 2018U §[L-M]) & use of Kalippic motion are explicitly provided on the 1900° old papyrus. (Kowal & Van Bruammen 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, \( 365\frac{1}{4} - 1/309? \) The field was informed of these discoveries in 2015. Inert reaction: [3] fn 66. N20 Confusing the Alanoids and handbooks of extant derivative science — cuneiform texts (§N13) and Almajesty (§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-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 archshipmanship improvidently50 for decades banished anyone who object 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?

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 unengineering 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 sure success.
References

Almajest. Compiled Ptolemy c.160 AD. Eds: Manitius 1912-3; Toomer 1984.\textsuperscript{51}
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D. Rawlins 1991H. DIO 1.1 16.
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D. Rawlins 1992W. DIO 2.3 19.

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

\textsuperscript{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 [\S7 and In 8] trio cited in the piece he published [Swerdlow, Gingerich, & V. Thoren], reflecting the kind of muttered slander (more at In 35) created and spread behind backs by parties of 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 unliterally 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 1\textsuperscript{st} place resorted to slander & shunning & running instead of inviting debate, If (\S1) it genuinely believed evidence & competence backed its position? Why did peace never break out? DIO 16 p.2 in 1 (2009), emphasis in original: “Rational, pacific discourse shows who’s right & numerate, so: why would archons tolerate peace?”
Astronomer in Wonderland: Historians-of-science

The technically&ethically-rockbottom brand of “research” skewered in the following pages recalls Dio’s satirical summary during NatGeoSoc’s 1989/12/11 meeting launch of NGS’ amateur [Rawlins 2017B] data-juggling defense of its dying Peary North Pole hoax:

Orchestrate more fiddler factors than the New York Philharmonic.


Universities’ science departments deserve to know the kind of mis-math (fn 13), herd-think (fnn &10), data-tampering (§§B-G), & idea-grabs (fn 10, §§8) too often passing for scholarship in prominent but joke-referred (§2 fn 3; Rawlins 1991W fn 6) & cover-up-prone (fn 10, 11, & 97) journals in history-of-science, a field rife with smearings (fn 8), shunnings (fn &116; Rawlins 1991W fn 171&173), threats (fn 109), & rejection of normal science (bizarre details: idem & §§26; fn 100) if favoring heterodoxy, with research-advances’ acceptance contingent upon whose clique the disseverance belongs to. (Repellent examples: Rawlins 2017E §§3.) 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, §1A, §2 fn 49), to expand&refine knowledge. The result (p.45 & §§B5&G-C) regarding advances in ancient astronomy, is inevitably more destructive than constructive.

However incomplete, the following chronology is a start towards top academic institutions’ enlightenment re contemporary history-of-science’s failurites. (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 reciprocation.) 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 stand version, www.dioi.org/gjr.doc, of the following please-clean-your-house paper (with amiable cover letter), was inflexibly (fn 100) spurred 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 astronomy ALTERING DATA (esp. §§C-D&F-G), uncorrected-unretrieved math-batches of science (§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 (§1), and systematic non-citation of the scientific-history journal DIO, though for over 25 years it’s been the easiest most mathematically and technically competent journal in the science-history field, even the 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, eremitus 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 Tomey’s four Sun “observations” were FIFTY TIMES closer to Hipparchos’ 280-old indoor tables than to the outdoor sky, none daring (§2 §§8) Neugebauer-Gingerich-Schloen’s decree that an astrologer & clumsy faker whose frauds damaged&retarded predictive astronomy for 1000 years (§2 §A) was “The Greatest Astronomer of Antiquity” (fn 1 here). Have shunning, 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-negative? Simultaneously with Isis’ resistance to the below, it was learned (see §1 here for links to all papers) that: [1] a 7th-old DIO discovery (Rawlins 2008Q fn 6) had been unattributedly published, www.dioi.org/cgv.pdf, as Isis’ 2015 LEAD article (repair request repulsed), and [2] Isis’ pseudo-referened final 2016 LEAD article had extensively attacked Rawlins 1985G (Greenwich Meridian Centennial paper) on ancient longitude accuracy (referred on its mathematical merits by a panel of prominent scientists), calling such accuracy a “delusion” — due to the critic’s own amazing delusions (fn 97): [a] Treating a solar eclipse as lunar, neither author nor Isis even yet realizing it sorta matters. (See hist.sci icon Neugebauer’s able 1975 analysis at §1 e; & 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/issb.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 (Winne 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-referencing 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&conhestous 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) on the very opposite of the manyways-obvious (§110) truth of an issue as central as Greek astronomical empiricism, meanwhile becoming ever-more-incapable of self-righting the field’s ship.

After decades of observing science-shy historians-of-science and watching limited mentalities (fnn &96) like careearies 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-goodness by history-of-science archons, have led to a defensive pattern (gameplan?) of curling-up into a self-entwined world of classicistic, turf-confined, think quoted, oh-so-Scholasticism. See News 1994§ §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 leaked research, and [b] if historical investigation in the mathematical sciences would be more opined&technically capable if it were hence to proceed within, or sometimes in supervisory association with, the relevant science dept’s of those universities that value it.
History-of-science — Data-Tampering, Idea-Theft, Seminumeracy, Smearing, Shuns, Club-Preferringe Wellspring of a Projective Myth: Greek Science as Fumbling, Fabricating, and Unempirical Muffia Cult’s 84 War On Greek Astronomers’ Cornucopia of High-Accuracy Achievements Current Historical Advances Endangered

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

Carefully crafted and refereed advances in the history of ancient astronomy and ancient mathematics: [a] have long been exiled by centrist-journal editors who shamelessly flee (fn 100 below) whenever they cannot justify their actions, as observed 34 unprogressive years ago by Robert Newton (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 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 ([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. §§, with selected links), meanwhile asking how ancient astronomers copying predecessors could advance to their surprisingly numerous but heretoformally remarkably unappreciated high-accuracy Greek measures. (Below, compare §110 [& §11 §D] to craniolithic cult-insistence on Greek inaccuracy: fn 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-sunset; the method explaining all 3 Greek-adopted monthlengths becoming undeniably accurate to 1′ or better (!); how all 3 hitherto-unresolved lunar speeds were based on classical-era use of 13th century BC Babylonian eclipse data ([§34]; pseudo-Aristarchos’ daily retrograde Moon & Archimedes’ degree-use (both obvious, yet unnoticed for 2000!). §§11&12 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 quadraphy-verified (fn 88) reconstruction of his temporally-vast 4868 Great Year, 1′-accurate monthlength, and pre-Hipparchos discovery of precession. One of R. Newton’s favorite expressions for counter-revolutionary mis-scholarship:

A subtraction from the sum of human knowledge.

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

AI 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 frontline, certain careerist serial knowledge-subtractors, — compactly called the Muffia or the JHAD (fn in 1) hereabouts — have themselves made a discovery, to wit: that their network of politically centrist captive journals will help wage JHAD against heterodoxy by publishing effectively unrefered articles that:

1 Wikipedia’s article on the virtually unrefered Journal for the History of Astronomy (JHA) actually claims the journal is “peer reviewed”! (See fn 109 below, also re Wikipedia’s 2008-2014 war upon Rawlins’ Wik-biography.) Meanwhile, the best-refereed journal in the field, DIO, is repeatedly, aggressively clashed by Wikipedia as Unreliable (not deserving an article, with bio-references to Rawlins as “publisher” persistently suppressed), though neither Wikipedia’s CSCICOP-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 dared to referee): see fn 50&86 below and at 1’s POSTSCRIPT; also www.dioi.org/jh.htm, www.dioi.org/III.htm#op. Historians-of-science seeking ready buddy-publication (to convince their universities and funders that they’re academically contributory) evidently prefer such laxity vs being refereed scrupulously, since far more vitae get padded by indiscriminateness than by care. Given the spectacular muff-frequency of the history-of-astronomy cult and its captive JHA, plus its distaste for wasting time on alien authors or on the bother of serious refereeing (fn 97&100), DIO from its inception has dubbed this incestuous, 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 mufyffy is JHA’s riggorous refereeing of sufficiently

hugely contra-reality super-adherence to 280′-old indoor tables, is uncriminal since Greek astronomers were theorists not empiricists, who suppressed — i.e., destroyed — data inconsistent with prevailing models (fn &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. §§, with selected links), meanwhile asking how ancient astronomers copying predecessors could advance to their surprisingly numerous but heretoformally remarkably unappreciated high-accuracy Greek measures. (Below, compare §110 [& §11 §D] to craniolithic cult-insistence on Greek inaccuracy: fn 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-sunset; the method explaining all 3 Greek-adopted monthlengths becoming undeniably accurate to 1′ or better (!); how all 3 hitherto-unresolved lunar speeds were based on classical-era use of 13th century BC Babylonian eclipse data ([§34]; pseudo-Aristarchos’ daily retrograde Moon & Archimedes’ degree-use (both obvious, yet unnoticed for 2000!): §§11&12 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 quadraphy-verified (fn 88) reconstruction of his temporally-vast 4868 Great Year, 1′-accurate monthlength, and pre-Hipparchos discovery of precession. One of R. Newton’s favorite expressions for counter-revolutionary mis-scholarship:

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1 New early spherical trigonometry date, 2nd century BC (fn 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: [§32.] JHAD is cartel of JHA (Journal for the History of Astronomy, O. Gingerich principal editor for 40y) & 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 ([§10]; Rawlins 2017E) vs Hist.sci reverence for alibi-inaccuracy, to ameliorate Ptolemy’s gross fabrications, e.g., inverting relation of theory&evidence (Ragep crudely: fn 9, & focusing on Greek “theoretical structure, erected in spite of the enormous difficulties that beset the attempts to obtain reliable empirical data” prominently quoted by Gingerich 1976 p.477; see fn 8, 62, & 97 here). Neugebauer 1975 p.951 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: §§52&250 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 supernormal, palming off — as valid scholarship — notions unworthy of a rational enterprise. For a JHAD-wayhouse of ultra-outré occultisms, see here at: §§C1, D2&D3, E2, G7-G9, G11, H4, I22; fn 12, 33, 44&45, 55, 68&69, 89.

3 DIO’s principled approaches to knowledge are brought together below, at §11 [g]. See, too, fn 10.
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 stop refereeing at all, realizing it was the only way to start refereeing) believes papers by JHA favorites aren’t refereed, but rather preferred: straight-to-press unread (flagrantly, laughably clumsy Centaurus example written by JHA boardmember: fn 50 below), a practice additionally eased by JHA insistence on printer-ready (Rawlins 1991W fn 6) and wordprocessor (www.dioi.org/pmn3.htm) submission. (Such conveniences are only exacerbating a trend already underway [as DR warned JHA, 38 ago: DIO 1.2 fn 68&B4] whereby “editors” become little more than printers. Note DIO’s typical insensitivity, www.dioi.org/jsh.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/pmn3.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/jsh.htm, as fact 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 unprincipledness guarantees: #1 response to this DIO will be fresh irrelevant smearing. Same timing at www.dioi.org/jsh.htm#fipd. (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-skeptic, www.dioi.org/pmn2.htm, in private communications. E.g., his 2000 referee-report to Isis innumerately broadbrush-labelled the now-substantial (if largely silent for professional reasons) Ptolemy-doubting party as just a tiny “paranoid” bunch (#2 fn 5); merely for objecting to communal shunnings he and everyone who would know us, fn 5 here; and www.dioi.org/pmn1.htm, www.dioi.org/jsh3.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 [§7; also see fn 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/jsh3.pdf, 1994. His referee reports on skeptics often pretend (between shunnings) 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, impudently ignore mathematically and logically solid but cult-displeasing findings for decades. This transparent behavior continues even despite 2002 national attention to “unprofessional” acts characterizing the Ptolemy Controversy (ancient astronomy’s hottest). Are debate-averse conservatives justified in unprofessional contempt towards unprofessionally uppity heresy? That question is explored below, along with the field’s dominance 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:

[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 . . . or outright altering (e.g., §D5 item [C]; §F10 fn 25&85 [& see fn 30&11&85]) the data upon which an offending discovery is based, decreeing that whatever version of said data was previously accepted (right up until “forbearance” fiercely found the day) readily, if (ironically) 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 §§, examples of such advances where those, governed by agendas, shunning, & cliques — ever-attended by denigration of outlanders — instead (of resorting to data-alteration) just

§§

5 Shunning R.Newton: Gingerich 1990 p.364. Shunning DR: fn 109&125 here. Hoskin 1983 blackball: DIO 1.2 §[B2-B3] (1991). Shunning’s reality acknowledged by Schaefer 2002 p.40 while carelessly retaining nationally in Lie&TeleScope yet-untracted 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 unprincipledness guarantees: #1 response to this DIO will be fresh irrelevant smearing. Same timing at www.dioi.org/jsh.htm#fipd. (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-skeptic, www.dioi.org/pmn2.htm, in private communications. E.g., his 2000 referee-report to Isis innumerately broadbrush-labelled the now-substantial (if largely silent for professional reasons) Ptolemy-doubting party as just a tiny “paranoid” bunch (#2 fn 5); merely for objecting to communal shunnings he and everyone who would know us, fn 5 here; and www.dioi.org/pmn1.htm, www.dioi.org/jsh3.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 [§7; also see fn 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/jsh3.pdf, 1994. His referee reports on skeptics often pretend (between shunnings) 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, impudently ignore mathematically and logically solid but cult-displeasing findings for decades. This transparent behavior continues even despite 2002 national attention to “unprofessional” acts characterizing the Ptolemy Controversy (ancient astronomy’s hottest). Are debate-averse conservatives justified in unprofessional contempt towards unprofessionally uppity heresy? That question is explored below, along with the field’s dominance 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:

[B] Hiding Modern Empirical Data: Boomerang Irony & Lawlessness

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

§

In 1987, in order to justify the modern Ptolemaic 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-sluder was insisting: in 34. Which is why the parties soon proven right [14 §B4] and Catalog thefts were exiled, while who were impecuniously elevated were the most-impenetrable lifted into the field’s politically-olta Editorship. No surprises.) Evans’ paper tried alibing Ptolemy’s ridiculously huge errors to his and his cult’s satisfaction, by adding three instances of grossly erroneously 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 Σgr — the record of which has since disappeared without explanation — and two ancient observations of the star Spica vs the eclipsed Moon (Almajest 3.1) by Hipparchos in −145 and −134. The errors were all ordmag 1°: respectively, −40′, −33′, +33′. 

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

We quote from this 2009 DIO paper, which so precisely (and ironically) solves (§) downright 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 Σgr) 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) showing that the huge errors in some ancient observations were so ordinary that such were a poor basis for learning anything about ancient science [i.e., condemning Ptolemy’s gross errors]. As further examples, Evans specifically mentions (idem § p.235) Hipparchos’ two hugely disparate Spica data [also eclipse-based] . . . . which disagree by over 1°. He then draws for us a [Ptolemaist] lesson (emph added): “No better demonstration could be wished of the uncertainty attached to the method” of fixing stars’ longitudes by eclipses. However, when instructor Evans repeats the very same sermon (on Hipparchos’ eclipse-star errors) 11 later in [many]many-vaever-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 1997-1998 interim) Evans had read [the 1991 revelation] . . . . that DR had discovered
that Evans’ and Hipparchos’ errors. . . . WERE NOT OF MEASUREMENT BUT OF BASIC SPHERICALE-ASTRONOMY MATHEMATICS. . . . [Yet] when ineducable educator Evans’ 1st-hand evidence somneauts, he just pretends he was right anyway, unable to admit DIO scored & ‘premier’ JHA bellyflopped . . . . Contra Evens, neither his own nor Hipparchos’ problems were observational. Both simply misconputed . . . valid observational data by using invalid math: the wrong sign for their parallax corrections . . . . the [1981 Seattle] longitudinal lunar parallax $p_3$ was virtually $1\degree/3$. . . . the sign mixup would naturally cause an error of . . . . $-40$ (legal) & the laughably impossible $20000$ [observation?] [here in fn 12 & §10.4] “observed” I$1$’s value of $1\degree$ is indeed (Evans 1987 loc cit): “too small by about 40”. (Typically, Evans has no comment since, despite [Rawlins . . . face-to-faces] & Hugh Thurston [by letter] directly bringing the matter to his attention.)

After correcting for this Muff, we [see] the admirable smallness of the 1981 observational error of Evans (a dedicated student of ancient instruments & possessor of a steady hand, since the cross-staff requires it): merely 1 or 2 arcmin, just the sort of accuracy DR has consistently ascribed to the best ancient . . . observations.


In this year, I$1$’s 1997-2009 stimuli? (Idem shows that removing proposed parallax-sign confusions, as well as accounting for the serious systematic errors of the solar orbit Hipparchus used for his estimation of mid-eclipse-time, $B$ errors in empirical Moon-vs-star gaps of, respectively, just $-2\degree$, $+7\degree$, $+1\degree, +2\degree$.

$B$ 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): $0\degree$ for 3 lunar-limb-vs-Sun measures, Almajest 5.3 & 2’ for 17 solar equinoxes on Rhodes (mean’s traceable error 7, mostly non-observational: Rawlins 2018U §B4), Almajest 3.1; $S$ 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 empirically 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 has 127 below; and colleague’s cooperative averted gaze (fn 11 above) is consistent with the dreary theory that they prefer their frubs be granted silence in return. Mutual consequence-free lawlessness.

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

C1 Expressing them in stades north of the Equator, Strabo preserved a dozen-plus Hipparchus’ geographical latitudes $L$ corresponding to what ancients called “klimata.”

C2 narrow referee Toomer, Rawlins 1985G passim, [Rawlins 1985H fn 1] Pro-Greek-competency: DIO 1.1 (1991) § fn 24. More Evansiana: mean errors of lunar limb-vs-Sun separation (Almajest 5.3 & 5), mean error $0\degree$: Thurston 1998A $\pm1$. For Rhodos equinoxes, Hipparchos’ scatter (mean single-datum error) was $2\degree$ 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 $S$ scatter (here in Table 2). Regulus-restoration: Rawlins 2009E §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 $0\mp4$, the above-$S$’s exact before-correction-vs-after data are found here at $B7$ (or at Rawlins 2009 fn 22). Evans’ refusal to recognize that DIO’s analyses have ordmag-shrunk all 4 of his and Hipparchos’ longitude errors (each from $F$ to $0\degree$ or $1\degree$; chance odds ridiculous) implies that he suspects scientifically-irrelevant dark magic behind treasonous dirty-tricks [$\pm$ fn 48], & unprecedentedly insufferable quadruple-l`ese-majesté.

C11 Familiarity with klimata is vital to understanding the disgraceful ordmag F incoherence of Hipparchos’ geographical latitudes. For the purely astrological Hipparchian cause, see, e.g., here at §1 [R], or Rawlins 1985G pp.260f. Both sources analyse evidence consistent with the self-evident theory that organized ancient scientists had corresponded for lunar eclipse local-time comparisons (accuracy limited mostly by ruling-fineness of sundials & astrolabes used for timing eclipse-occurrences), to find longitudes to a mean accuracy of ordmag a degree ($\pm11$ 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 front-chrono-clonologically designated as repeatedly-For 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 11, which dishonest ‘is placed to print or even evaluate since it shows that ‘is’ editors & referees didn’t notice that Shcheglov achieves his denigration of ancient competency
constant-L strip-regions extending east-west around the Earth, sharing the same longest-day $M$ — which is mathematically determined by $L$. Since klimata were used for Hellenistic horoscopes’ house-divisions, ancient astrologers tabulated them for every quarter-hour or so of $M$. (Klimata tables are found, e.g., in Almajest2.6. And phenomena are tabulated according to klimata at Almajest 2.8 for rising-times, at Almajest 2.13 for parallax.) In 1934 the eminent Egyptologist Aubrey Diller made two connected, unexpected, epistolal discoveries: [1] all Hipparchos’ klimata were computed via spherical trigonometry16 (which in 1934 scholars doubted was available as early as the 2nd century BC), [2] using an obliquity17 untested but the most accurately anciently adopted: 23° 23′.

C2 **Mystically 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 inherently 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 crediblity 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 Fenacuto to no-decision until he “engaged his adversary in a theological discussion. Here the pagans was easily defeated” and, thus confounded, was finally slain by the sword. When, despite being repeatedly informed of contrary evidence, our era’s equally eminent whistleblower-resisting SCIENCE journals (j2 fn 11) dissentently trust for decades Mennonite Jesus-hugger Gingerich’s insistence that his fellow-occultist&court-AlmaJester was The Greatest Astronomer, of an antiquity on whose ingenious empiricism (§) Gingerich remains incovincibly clueless, can we regard contemporary academe’s forums as any less deliberately mythmaking-for-the-cause than those of accurate klimata’s history’s prior Dark Ages?]

14 L = arctan (cos(7.55α)/tan α) (L in degrees; $M$ in hours); Almajest 2.3, Neugebauer 1975 p.38; further sample klimata tables, ibid, pp.706-736.


16 Diller 1934.

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

18 Contra Hipparchos’ access to sp 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. 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-Rawlinson klimata table (Rawlins 1994M p.56). All ducked, with an unwillingness to acknowledge Diller’s vindication (or even a minim of merit) that was unashamedly, unblemishly unanimous, even Britton: DIO 16 p.2 (2009). (See also Dicks 1960 pp.102-104, where Dicks escaped the Neugebauer cult.) Neugebauer 1975 p.334’s half-failed 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 ever care about the dirt done a refined, dedicated scholar? During decades of insisting on fitness 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 (at age 76) what 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, nonits (ere col.4’s 5 rounding) there underlined. Rawlins 2009S, www.dioi.org/jg03.pdf, Tables 1&2.

23 Diller 1984 Table 2. The 14 klimata: Equator, Cinnamon Country, Meroe, Syene, Lower Egypt, Phoenicia, Rhodos, Hellespont, Massalia, Pontos, Boryshenens, Tanais, South Britain, North Britain. 25 Jones 2002C. 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-protagé & eulogist Swdrow 2010 p.151) wasn’t a gnomon shadow-ratio at all, but the Alexandria klima’s shortest:longest-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 (same Shandy 2.5.38) caused 1000 of north-Africa latitudinal map-distortion. (Rawlins op cit §4; similarly durable longitude-disaster for Araba eclipse: Neugebauer 1975 p.938.)

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### Table 1: Diller SpH Trig Proof. Hipparchan Longest-Days in Hours ⇒ Latitudes in Stades

<table>
<thead>
<tr>
<th>Klm</th>
<th>Longest Day M</th>
<th>$L$ Calcd SpH Trig fn 14 eqn</th>
<th>Round Nearest Degr/12</th>
<th>Conv Stads $\S3$</th>
<th>Round Nearest 100 St</th>
<th>Strabo Princ Instt Neug</th>
<th>NYU JHA Jones</th>
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<tbody>
<tr>
<td>Egtr</td>
<td>12°/3°</td>
<td>0°</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>1500</td>
<td>8800</td>
</tr>
<tr>
<td>Cin</td>
<td>12°3/4°</td>
<td>12°36′23″</td>
<td>12°7/12</td>
<td>8808</td>
<td>8800</td>
<td>10200</td>
<td>8800</td>
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<tr>
<td>Mer</td>
<td>13°</td>
<td>16°35′04″</td>
<td>16°7/12</td>
<td>11600</td>
<td>11600</td>
<td>12800</td>
<td>11600</td>
</tr>
<tr>
<td>Syc</td>
<td>13°1/2°</td>
<td>23°59′43″</td>
<td>24°</td>
<td>16800</td>
<td>16800</td>
<td>17600</td>
<td>16800</td>
</tr>
<tr>
<td>EgL</td>
<td>14°</td>
<td>30°33′49″</td>
<td>30°14/12</td>
<td>21400</td>
<td>21400</td>
<td>21800</td>
<td>21800</td>
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<tr>
<td>Pho</td>
<td>14°1/4°</td>
<td>33°31′04″</td>
<td>33°1/2</td>
<td>23450</td>
<td>23400</td>
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<td>36°14</td>
<td>25375</td>
<td>25400</td>
<td>25500</td>
<td>25300</td>
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<td>15°</td>
<td>41°07′34″</td>
<td>41°1/2</td>
<td>28817</td>
<td>28800</td>
<td>28800</td>
<td>28800</td>
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<td>Mas</td>
<td>15°1/4°</td>
<td>43°16′44″</td>
<td>43°1/4</td>
<td>30725</td>
<td>30700</td>
<td>30700</td>
<td>30300</td>
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<tr>
<td>Pon</td>
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<td>45°15′40″</td>
<td>45°1/4</td>
<td>31765</td>
<td>31700</td>
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<td>37975</td>
<td>38000</td>
<td>38000</td>
<td>37900</td>
</tr>
<tr>
<td>Sbr</td>
<td>18°</td>
<td>58°12′31″</td>
<td>58°1/4</td>
<td>40775</td>
<td>40800</td>
<td>40800</td>
<td>40700</td>
</tr>
<tr>
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<td>61°04′56″</td>
<td>61°1/2</td>
<td>42758</td>
<td>42800</td>
<td>42800</td>
<td>42700</td>
</tr>
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</table>
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 Syracusan’ latitude by 200 stades), which he argues must be corrected-for — thereby justifying-excusing his replacement of already-perfectly-fitted unmanipulated data, with sub-50%-fitted (Table 1) revised data. Seeing Jones extrapolating from a very exceptional klimata to the whole set, atheist DR is reminded of creationists who generalize from rare, anomalous geological strata to rejecting natural selection entirely. Obvious point against the significance of Jones’s disparate data-injections: he well knows28 Hipparchos changed adopted parameters over time.29

Further, Jones (ignoring $3^\circ/2^\circ$’s confirmations: JH; Rawlins 2009S fnm 23 & 54) accepts Ptolemy’s 1.12 testimony that Hipparchos was Eratosthenes’ $\epsilon = 23.5^\circ20'\prime$, a value which Jones imaginatively attributes to a speculative Hipparchian computation from a confused-up non-Hipparchian Alexandrian $L = 31^\circ$, without realizing that obliquity $\epsilon$ would already be known since ancient sources found it concurrently with $L$ — and via solstices, not (as Jones curiously assumes)78 equinoxes.

C7 Jones also-traditionally alters28 the Almajest’s text for Hipparchos’ Marseilles latitude, from $L = 43^\circ0'4''$ (consistent with $43^\circ 1/12$ of the Ptolemy Geographic Directory [GD]) to $L = 43^\circ0'11''$, in order to reconstruct (using Eratosthenes’ $\epsilon$) 2nd century BC Hipparchos’ Jones-suggested indoor calculation-invention (for untested states, and counter-clockingly) of Pythaeus’ well-known longago (c. -300) solstitial noon gnomon ratio, $41.4/2\$ (whose precision argues it was an outdoor29 datum) and, to force the speculation’s success, Jones begs tolerance of an odd-but-convenient Hipparchian miscalculation,30 yet another ah-doke wakening of ancients’ data. [While rejecting Table 1’s normal rounding!]

C8 Jones’ promotion31 of such jigsaw puzzling seeks at least a half-share of Diller’s discovery. Worse: by fantastically alleging that Diller used invalid data, Jones (Neugebauer-rially: §C2) lodges his half-fitted theory — unvetted (§C10) and untabulatedly-whimsical — as SUPERIOR to Diller’s ultimately-perfectly-fitting one. Though willing to refuse Diller’s 1934 paper with 2 nonfits of 11 listed klimata, until DIO’s 1994&2009 upgrades: §C3 [a] & fn 34 end, shunsoldier Jones dutifully, consistently refuses to acknowledge the bare existence of Diller’s 1994 vindication: DIO’s initial near-perfect 12-hits-out-of-13 table32 (though its merit is prominently recognized by H.Thurston [Isla] & G. van Brummen’s meticulous standard history of early trigonomony [Princeton University]) — or the final DIO 16 3§ update distributed in 2009, where the ultimate blemishness-less fit perfecion (Table 1 here) of Diller’s 14-for-14 victory is too irrefutable to deal with. Except by fleeing.

27 Jones loc cit. Syracusan 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 Britain 1992 p.29, Non-Hipparchian Alexandria L: Rawlins 2009S fn 30. Obliquity $\epsilon$ found concurrently with $L$ via solstices: ibid §§F2-F3 & eq.8, and Almajest 1.12.
28 Original-text Hipparchian Marseilles $L = 43^\circ0'4''$ (Almajest 2.6), rounded at GD 2.10.8 to $43^\circ1/12', often altered to $43^\circ0'11''$ (defying GD’s consistency with 43°0'). See Rawlins op cit §H. Achromonom: Jones 2002E p.17. Pythaeus gnomon ratio: Strabo 1.4.5#&2.41. Rawlins 2009P eqs.2-3.
29 ibid Summary [1a] and Rawlins 2009S fn 34.
30 ibid Summary [1a] and Rawlins 2009S fn 34.
31 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.Rager 2010 p.128’s Jones-promo sees no problem here.
32 ibid,
33 E.g., Jones’ foreword to recent (long overdue) reissue of Pedersen 1974.
34 Initial Diller-Rawlins table of Strabo data compared to Neugebauer’s: Rawlins 1994M p.56 [aply augmented in later renderings, as noted at fn 19], JHA-uncited for 24’ now, but noted by Thurston 2002p 6.67, and by van Brummen 2000 p.65. Final 13-for-13 version of Diller-DIO klimata table: www.dioi.org/vols/w50.pdf. Diller 1984 Table 6: The table’s subsequent 14-for-14 expansion: here in Table 1 or www.dioi.org/jg03/pdf. Rawlins 2009S Table 2. 35 See Rawlins 2009S Table 2.
36 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.
37 JHAD-unrealized: Hipparchos-Strabo’s data have unexpectedly SPECTACULAR sensitivity to even the most minuscule imperfection in the $\epsilon$ or the constant-shift $A$ assumed for testing fits, which renders it especially astonishing that the Diller-Rawlins theory (even without help from an arbitrary [Jesuanian] 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 14/4.) Jones’ 2 premises, [i] Hipparchos’ $\epsilon$ was Eratosthenes’, and/or [ii] Strabo’s data need alteration by $A = 100$ stades, overkill-wreck any chance of judging Jones’ klimata data. If a difficulty he well knew of, only because he didn’t know how,26 to run the required least-squares (a disability seemingly near-universal among historians-of-science: fnm 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 & least using the $A$ to adjust the other known, several non-fitting klimata ensue, as emphasized to him by phone immediately upon his 2002 publication. The reality is rendered particularly obvious by his failure — unique to this controversy — even to supply a table of data. (Diller, Neugebauer, & Rawlins all tabulate.) This, because neither his $\epsilon$ nor any other can fit his own fiddled version of the data. JHA’s intensive breakfast—lunch refereeing (fn 4) inexplicably never noticed the omission of a Jones table, or his non-citation of the undeniably better-fitting 1994 Diller-DIO table well known to him via Isla (Thurston 2002S) & direct communications (www.dioi.org/biv.htm#jphn). Question: why persist for 16’ to nonact & 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 (Princetitude or JHA) are italicized. The crasher (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, Princetitude&NYU self-disgracing, revealingly sure academe won’t act.
39 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
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 subject to 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-Geekbelian flames it deserves.

C12. Note: it’s been obvious since [the Rawlins 1994M investigation] that \(1\)\(^{\text{st}}\) accuracy is crucial\(^{37}\) for the 18\(^{\text{th}}\) klima’s fit (Table 1 above; or Rawlins 2009\(^{\text{N}}\) 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\(^{\text{st}}\)-accuracy: [a] Table 1’s 18\(^{\text{th}}\) klima, as just noted (fn 37). [b] Below fn 46’s neat hits for attested e&\(r\).

C13. Ironically, D.Juke’s rejection (fn 69 below) of that reconstruction unintentionally highlights the Hipparchan trigonometry tables’ accuracy. It just seemed incredible to Duke that DIO’s analyses (precised at §D here) of Hipparchos’ *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 (fmn 24&52 here) & his output was more accurate than previously believed by Duke or anyone else (including DR, before 1991 testing). So his/kother’s initial conjectures doubtfully just emphasize the shocking newness of the discovery of Hipparchos-era \(1\)\(^{\text{st}}\)-accuracy computation & trigonometry tables: [1] the two eclipse trios (customarily called “A”&“B”) confirmed each other by both producing Hipparchan orbits based on Kalippic motion (period 365.2414\(^{\text{[2]}}\) [2] new papyrus testimony vindicated in 2005 (§F4) both of the novel DIO’s 1991 analyses: [A] Hipparchos’ search for a — 157 Summer Solstice, [B] his contemporary passing adoption of Kalippic solar motion.

C14. None of these confirmations could have succeeded unless Hipparchos’ trig tables were indeed (as already indicated: fn 37 below) accurate to \(1\)\(^{\text{st}}\), 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 \(1\)\(^{\text{st}}\) (from slightly unreliable trig tables), Hipparchos would have found other than \(e = 327.2/3\), the correct value, which is attested (§D1 below) & is found to agree (§D3) with calculation via \(1\)\(^{\text{st}}\)-accurate trigonometry tables. To repeat (§C12): [i] the Diller-klimata table’s 18\(^{\text{th}}\) entry (here, in Table 1 & fn 37) and [ii] DIO’s eclipse-pair-based orbit reconstructive matches, both consistently establish the 2\(^{\text{nd}}\) century BC as the earliest date we know \(1\)\(^{\text{st}}\)-accurate trigonometry tables existed. Concluding this section’s revelations, of sph trig & \(1\)\(^{\text{st}}\)-accurate trig tables & calculations, *3 centuries before Ptolemy*: we recall the mentality that long ago locked-in establishment-wisdom here, Gingerich 1976 p.477 in *Science* (1), blaming his hero Ptolemy’s huge errors on (caps added) “CLUMSY mathematics invented only a generation earlier” (\(1\)\(^{\text{st}}\)% [M1] [b]).

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

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

37 At the 18\(^{\text{th}}\) klima in Table above, 58° 12′ 31″ rounds to matching 58° 14′; but 58° 12′ 29″ wouldn’t.


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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 modern D.Juke) 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 onset 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 was typically ordmag-rounded\(^{42}\) solar distance of 1000\(\text{r}\), as follows:

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

41 Rawlins p.165 §V vs conservative assumption of outixed-for orbit radius \(R\): Thurston 2002S p.60. For Ptolemy’s A&B & DIO’s fits are compared for all 4 parameters at Rawlins 1991\(^{\text{W}}\) [P2].

42 Ancient ordmag-rounded Sun-distance estimates: Rawlins 2008\(^{\text{R}}\) §§D-F. Sun-distance as historical origin of order-of-magnitude: Rawlins 2012\(^{\text{V}}\) §D. Reconstructed distances: [a] \(\text{SE}_\text{Eratosthenes}\) (Rawlins 2008\(^{\text{Q}}\) eqs.6-13; nearly same at Carman & Evans 2015); [b] \(\text{SE}_\text{Ptolemy}\) & DIO’s fits are compared for all 4 parameters at Rawlins 1991\(^{\text{W}}\) [P2].
Data-Fudgery for Myth&Turf

DIO-J.HA 22 3

D.J.H. Rawlins

R = 1000° cot 87° = 52° 24', or, in 60th's, 3144' — thus matching43 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° 24' as 52° 1/2 4', we find, in 60th., 3122 1/2' — exactly matching44 Trio B's R (above). Unable to counter the math, centrist pols (faces eternally, irrevocably invested in shun: fnn 125&127) have, during the decades since 1991, had no reaction to this minimal-precisions double-match of both 3144 1/2' & 3122 1/2', besides implicitly contending45 (by pushing incompatible fudge) that this is all PURELY COINCIDENTAL. Any wonder the above Summary was forced to confront the supernatural? — see fn 2 here.

D3 DIO also found double-consistency with its theory that Hipparchos' calculations [1] had used eclipse-pairs46, not trios (the trio-approach has never yielded unmanipulated data-matches) [46] [2] had sought only one unknown, eccentricity e (or epicycle-radius r), not apogee-at-epoch A0 or mean-longitude-at-epoch m0. Thus computing r & e produced 327° 39' & 247° 30', respectively, each a neat a 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 acknowledgement47 of his 1973 speculation's dubiosuesus), 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-D6) is not 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.dio.org/ptri.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.)48 Contra Duke's attraction to committing fudgery: throughout Rawlins' researches, it proved unnecessary49 to 'correct' any of Hipparchos' calculations in order to draw coherent results from his data, so a historically new conclusion50 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 through no regular elements from long-earlier-lunar-eclipse Trios A&B ([C]13&[2], fn 55). In 1977, physicist Rob Newton detected51 a hitherto-unsuspected 1° error in Trio A's 3rd eclipse, claiming data-restoration here is demanded (fn 91 below) — incredible" consequences will flow from analysis. The orbital elements derived by warning-rejectors Jones and Duke inadvertently double-confirm52 Newton's "incredible" prediction. Like Jones, Duke does not notice Newton's 1° warning, & deliberately (§D6) acts as if the carefully-referenced (§D3) DIO paper that did heed it does not exist, though it was unsuccessfully challenged (§D6) by Duke to arbitrate Thurston (and fn 32 above) precisd in Isis in 2002. Duke's paper independently computes53 best-fit e&k of the Trio B-accordant EH orbit, & of the inevitably weird unrestored-Trio-A-accordant orbit, as if original, though

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

E2 Nonciting Newton’s and DIO’s analyses protects readers from learning that, after 
application of Newton’s unexpectedly productive-predictive 1st correction, TriO is satisfied by 
unzany elements, which turned out — as discovered 14’ later — to be half from the 
EH Sun orbit (already known from TriO B, §E2 below) and half from the familiar 
prime PH Sun orbit. Unless utterly coincidental (as Duke and Jones judge reasonable) or 
an evil miracle, the result reveals, that, at the time of his Trio A calculations (PH’s −145), Hipparchos’ adopted Sun orbit was transitioning from EH to PH, so he temporarily 
retained EH’s eccentricity and Kallippic mean motion (until their new PH tables were 
computed), while immediately adopting PH’s 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 umasks 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 sensorially denied the 
portunity to decide for himself whether or not §E2’s astonishing but Censtars-uncut half/half upshot is meaningful? — and thus whether R.Newton should be credited for a 
finding that triggered unexpected progress, as valid discoveries will.

F Solar Orbit Reconstructions and Fruitfulness

F1 Hipparchos’ −145 Prime PH solar orbit (§E1) is famous, because adopted by Ptolemy 
(Almajest 3.2&6) & still worshipped by Julian over 500’ after creation. But DIO recon- 
structured56 two other Hipparchos orbits: his −157 Early EH orbit (§E1) & −134 Ultimate 
UH orbit (idem), each seriously differing from PH & previously unknown. (But Hippar- 
chos’ use of a late non-PH orbit was presciently suspected before anyone by Britton a 
half-century ago.)

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

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 §ib 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±55’ (ibid eq.9), fit BM55555’s writing style.

61 Below, §§9; or ibid §§B3&B4. BM55555’s revelation: ibid eqs.6&8. I will ever be grateful to the 
late Willy Hartner, who was 1st to suggest (letter to DR 1980/8/15: ibid §A5) that scholars (including 
DR) were ignoring Ptolemy’s hour-omission for two of the Almajest 3.1&7 solar data.

& 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) or declinations (Almajest 7.3). 
p.72 vs Rawlins 2008R §A, sardonically at §A3, condensing the most unexceedable of JHAD fantasies 
(e.g., Aristarchos & his heliocentrist-pioneer Aristarchos was an 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 monthlengh 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 
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]88. Given 
their relative merits, one would think that 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, go’v’t) wanted to promote, go-along- 
conservative Ptolemy’s Aquinian-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 Hipparchos data; though all 4 elements of such an orbit had already been published by DIO 
(sources: fn 56 above).
by him from UH, unbeknownst to Tptolemy); however, the 2nd alone agrees (by chance, as it happens), though the underlying mean longitude he lists for it is discrepant by 5′ vs PH — even while tellingly agreeing to the arcm in with UH.

[4] PH orbit periodic error has amplitude 0.4, so it formerly seemed odd that the Sun-based Ancient Star Catalog’s periodic error is 0.2 — until recovery of UH, whose periodic error’s amplitude is 0.2.

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

[6] UH’s 4th interval—127 Autumnal Equinox, follows Meton’s sacred −431 Summer Solstice by exactly 304°/4, so 16 or 2° such intervals just equal the 4868° “Great Year” of Aristarchos.

[7] And perhaps he used a version of the 4868° cycle at 1778021° (not Aristarchos’ 1778022°; §5 below) it embedded an astounding quintuple of geometrically expanding cycles.

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

F4 From fitting EH to eclipse-Trio B, DIO mathematically induced (Rawlins 1991 W §§K4-K9) in 1991 that Hipparchos’ earliest Sun orbit, EH, [a] used a −157 summer solstice, [b] adopted Kallippic solar motion, 360°/365 1/4 for Trios A AND B. Findings [a][b] were both previously unsuspected. But, 14 later, papyrus P.Fouad 267A was examined by A.Tihon (paper 1st presented: Peking 2005) & was found⁶⁴ to explicitly verify 1991 discoveries [a][b][c]. (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 assented⁶⁵ to UH; but nowhere has it been acknowledged that (repeating for emphasis) Rawlins 1991 W and (fnn 27K96) 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°.

F6 NB: These ∼775 AD induction-predictions aren’t side-issues. They are central to understanding the Hypocrite 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, year cycle at 1778021 d (not Aristarchos’ 1778022d: §). Rawlins 1991W presented 2001/6/27 at British Museum conference, “Under One Sky” — condensed version published simultaneously in conference proceedings (2002).

F5 Quintuple succession of doublings (Rawlins 2002A at fn 14’s conclusion): 304°/4 (1° difference between Meton’s 360° (eclipse-calendars); [c] Hipparchos, calendar: 360°/4 (80°) + 1217° (1° return of Sun); 2434° (1st return of Moon); 4868° (integral number of days) — every one successively featuring a fresh characteristic cyclic-return, where each of these includes (like the unfesh 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 halved fields of view, very nearly, side by side (152’), Kallippic calendar (1°) return of Sun; where the 4868° Great Year encompasses about 25 of Meton’s (Earster) 19° cycles. Details: ibid fn 17.

F6 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 1991 W §K8, a point precisely resolved when Rawlins 2018U §K5 discovered both [1] the solstice’s hitherto-unknown hour, 18′ (missing on the papyrus), and [2] the exact origin of the previously-unaccounted-for remainder of the papyrus’ Tihon-discovered novel tropical-Metonic yearlength, 365°1/4 − 19°309. On 2015/4/8, the community was alerted to all this by email to a participant — and to the posting of DIO volume 20, containing the 2015 version of Rawlins 2018U presenting these solutions, plus the 1° formula ever developed for finding solstice observations’ small ordmag-º systematic errors (from Earth-orbit eccentricity), ibid: eqs.10-13. Not to mention DIO 20’s lead paper, with its important fresh discovery (§11 below) of Archimedes’ 3rd century BC use of degrees. There has been no discussion 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 to spell minimal numerical 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 ofkJ-like bigrigibigbig 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 banishes’ paper which had also satirically-advertised such typically-refereed JHA discoveries as the Velikowskian 365th yearlength by Duke’s JHA co-boardmember Jones (Rawlins 1991 W §G9,A DIO 4.1/4, 1994) — so the usual bundled lead-paper
observations by that amount on average (while not affecting its 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 fn ms 61&70 respectively.)

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

F11 He calls the EH orbit “neither conclusive nor satisfying” since (emphasis added) “parameters derived 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 [§5 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 5.3.85 – whichRawlins 2012V calls successively Trios A, B, and C — as do the EH → “Frankenstein),” EH, and UH orbits, respectively; and [ii] whose underlying cardinal points (Vernal & Autumnal Eqinoxes and Summer Solstice) uniformly hit upon Hipparchos’ standard $1^4/4$ 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 Talismanization of Evidence.

G1 The mystery of the superficially-nonsense ancient yearlength31 found on Vat. gr. 191 fol. 170v and Vat. gr. 381 fol. 163v lay unsolved through decades of fruitless disagreements (fn 87 here). The name of Aristarchos of Samos is written beside two of these yearlengths: $\tau \xi \epsilon \delta ^\prime \kappa ^\prime \zeta ^\prime \beta ^\prime$ and $\tau \xi \epsilon \delta ^\prime \kappa ^\prime \zeta ^\prime \beta ^\prime$, or 365 $1^4/4$ 20 60 2’ and 365 $1^4/4$ 10’ 4’.

G2 Taking the numbers exactly as they stand and allowing signage-flexibility, Rawlins in 1980 treated32 both Aristarchean expressions as continued fractions, and swiftly sent the results to the *Journal for the History of Astronomy.*33 Listening to Neugebauer’s perceptive recognition that 60 could signify 600, Rawlins saw that the 1st expression could be viewed as $365\frac{1}{4} + 1\frac{1}{20} + 2\frac{60}{600}$ = 365 $1/4$ – 15/486, a classic Metonic “tropical” year, quite close33 to the known (also seriously false) tropical yearlengths of Hipparchos & Ptolemy. The 2$^{nd}$ expression suggested $365\frac{1}{4} + 1\frac{1}{10} + 4\frac{1}{4} - 365\frac{1}{4} + 1\frac{1}{152}$, differing but ordmag 10’ from the actual sidereal year then (fn 114 below).

G3 Both results’ implicit periods, 4868’ (Great Year) & 152’ (2 Kallicip 76’ cycles between iconic Meton’s and Aristarchos’ Solstices, –431 & –279, respectively), are among the EXTREMELY34 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 close35 to $1^4/4$ century, which presumably later influenced Hipparchos to treat $1^4/4$cy as a lower limit, though Ptolemy eventually adopted $1^4/4$cy exactly (*Almajest* 7.2-4). Note that Aristarchos is the only astronomer on the Vaticans’ Interverifikation for two different yearlengths, obviously suggesting precession. As the 1st astronomer we know who was a public geonomist, 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 126000$^0$16 interval’s felicitous near-constancy (due to near-perfectly-integral return in 4573 anomalistic months), regardless of ecliptic position. That interval’s tiny inconstancy-amplitude36 of c.1/2’ guaranteed the deduced monthlength’s accuracy to one part in ordmag 10 million. (Divide 4267 [4267 months] by $1^2$ to see this; the result is merely an upper bound on the better accuracy attainable by round-the-zodiac averaging.) *DIO*’s exploration of the 4267 cycle vindicated Ptolemy’s oft-doubted contention that it was the historical source of the ancients’ highly accurate monthlength $M$, commonly miscalled the “Babylonian month”. Rounding at the 10s-place of the 2nd sexagesimal term (as we find on cuneiform texts: fn 80), yields the $M$ attested at idem: $126000^01^1/4267 = 29^0191^1/40050^0 = 29^03^1/50^008^9/20'' = 29^012^44^3/3^3 = 29^0530594$, correct (even today!) to a fraction of a timesec. Aristarchos’ 223-month saros expression (idem) will, if divided37 by 223, yield $M = 29^012^44^3/3$, 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-Babylonian authorship of $M$, as does the chronologically ordered Vatican mss’ long-overlooked listing of Aristarchos prior to anything Babylonian. TWICE. [See www.dioi.org/p913.pdf, Tables 1&2.]

G5 We know that *Almajest* 4.2’s saros of 223$M$ agrees38 to 1 part in 24 million with idem’s deceptively-round-looking saros expression, $18^4 + 10^4/2^3$ or $18^4 + 43^4/5$ (where superscript $K$ signifies Kallicip Yearlengths of 365$1/4$ each) — which, times twice 135, so

75 Metonic “tropical” year: *Almajest* 3.1; Rawlins 1999; Tigon op cit; origin $^l \pi$ 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 [§4D & Rawlins 2018U §P7].

76 Censorinus 19.2&18.11 connects Aristarchos to 1623$^0$ & 2434$^0$, which are 1/3 & 1/2 of 4868$^0$, resp; see fn 79 below, & Rawlins 2002A fn 14-15 & eq.?.

77 Ibid fn 14 shows that, whatever one’s sign-choices for the latter digits of the Vatican mss’ Aristarchean expressions, implied precession will still be near $1^4/4$ century, Hipparchos’ lower limit (Rawlins 2018U §K5), verified by Tigon op cit.


79 Ibid fn 14 shows that, whatever one’s sign-choices for the latter digits of the Vatican mss’ Aristarchean expressions, implied precession will still be near $1^4/4$ century, Hipparchos’ lower limit (Rawlins 2018U §K5), verified by Tigon op cit.


81 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.

precisely produces day-interval 1778039, Aristarchos’ Kallippic Great Year of 4868k — results which yield:

Discovery #1: The Vatican ms’ Aristarchos-marked year, 365 1/4 − 15/4868, is certified as Aristarchos’ by its denominator’s match to his 4868k Great Year, as already identified by P.Tannery.

Discovery #2: Merging [A] Aristarchos’ Great Year, [B] his manuscriptic vandalism. (Though see Rawlins 2000A p.13.) I.e., it’s the kind of originality that explains why pioneer Jones adorns his Kallippic 4868K Great Year). Dividing by 4868 y 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.

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 [above]. Philosophically, he is Talibombing! He added: When I wrote a Jones—Rawlins 1999/7/14 letter, he was reading the very DIO issue that introduced his own new research on the督察 monitoring of the 1778039y Great Year, 365 1/4 − 15/4868. [ii] Jones is an active listener when Rawlins’ 2001 British Museum talk presented his now-fully-developed series of astronomical-odds, digit-for-digit matches to UNAMPERATED data, e.g., 1778022 & the multiple confirmation of 4868k (i.e., §§G5’s Discoveries #1 and #2), the culmination of a century of scientific analysts’ ultimately quadruple-confirmation of Aristarchos’ 4868k Great Year. The out-of-the-gate after-lecture commentator was Jones, denying their slightest possible significance. To the immortal discoverer of the Winter Equinox (fn 86 here), it’s all mere Luciferian quadruple (fn 88) coinidence, though his 100% rejection’s plausibility may be gauged from his 2010 paper’s silence on DIO’s Aristarchan numbers (no claim of DIO mis-calculation), even while (fn 85) he defaces the ms data they match.

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 ◦/day (Velikovsky’s 360d/year calendar). [d] A trio of longitudes (e.g., Almajest 5.3.58) 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 [A9] for 365k 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 47◦01’ (ibid p.17, contra the text [fn 28] and Rawlins 2009 fn 40&41). [h] The Diller-Rawlins 1999/7/14 fits, in an ultrasensitive 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 EE4, contra direct testimony of 2005-revealed papirus 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 materials, might suggest non-serious, not-altogether legitimate humility and caution.

Like confusion: fn 28, or Rawlins 2009S JH. Rummaging: Jones 2005 pp.21-22; fn 24&27.8

88 Keep in mind that we have mutually-confirmatory quadruple evidences of the 4868 Aristarchan Great Year. (But not enough for Jones, who at §G8 destroys one of these evidences — below — apparently figuring that, if he can cripple the fourfold-case down to hohum-mere quadruple-confirmation, he might yet swing observers to sharing his occultist rejection of Tannery-Heath-DR reconstructions.)

[1] Censorinus’ testimony (fn 76 above) is almost fully aware of the problem, and item [2].

[2] Almajest 4.2 saros or exeligmos expression (fn 81 above); developed at Neugebauer 1975 p.603, where 1.218 is sexagesimalis for 4868.

[3] Variations: 4868k, 3ου 80′08″ 20′′′′ 235 19′ = 1778022, thus (fn 83) matching the remainder-numerator of item [3]’s Great-Year yearlength 365 1/4 − 15/4868 (fn G5).

(See parallel fn 83.)

And carefully check the galloping successes reviewed at Rawlins 2002A, culminating in its eqs.10-13.)
II Pliny’s Circuli: Deft Ancient-Trig Approximation-Inventiveness

H1 Now for a case of ancient data-tampering that actually happened! — the very sort which modern-chauvinist tamperers have, with dedicated persistence & Creativity, tried misproscripting86 onto valid data (see §§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 “arithmetical”87 scheme, deeming their superficial inaccuracy to be supportive of his contra-reality (fn 8 above) insistence on “the absence of any scientific organization in antiquity”, a conviction which he thought helped (along with his reasoning as covered in fn 1 here) to exonerate accuracy-challenged Ptolemy. But the 1st — and still only available — coherent solution (fn 91 below) ever devised for the circuli indicated they are no more arithmetical than Hipparchos-Strabo’s klimata (which Neugebauer 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 test88 for possible ancient data-alteration, since all seven circuli are clearly a version of the traditional Seven Klimata.

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

H4 Rawlins’ 1984 paper (invited for the Greenwich Meridian centenary Longitude Zero Symposium), featuring its 1°-constant-shift solution (fn 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”89 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-puzzle-tell readers that these restored Pliny data play consistent with (fn 92 here) the very same accurate Diller-discovered 23°/23/ Hipparchan obliquity Jones is busy trying to replace (§C6) elsewhere in the same paper, a paper which qualifies as a pinnacle of JHAD-subtractivity, simultaneously managing to deny Greek accuracy on all available fronts — obliquity & klimata & calculations & trig-tables & circuli (& Diller & DIO) — thereby obediently according with predecessors’ orthodoxy (just-as-reliably as did just-as-careerist Ptolemy’s “observations”: fn 8), ensuring Jones’ rapid elevation to JHA boardship.

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 Brave’s 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 revealed90 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 2000 years. For regular culture have accepted On the Sizes and Distances of the Sun & Moon as by Aristarchos of Samos, namely missing the significance of Archimedes’ accurate contradiction — by a factor of four — of Sizes’ farcical91 fundamental empirical data (2° solar&lunar diameters!), & in spite of its ludicrously contra-reality requirements that: [a] total lunar eclipses last 12h (Neugebauer 1975 p.642), & [b] for Mediterranean observers, the Moon (at distance c.20 Earth-radii) visibly moves in REVERSE among the fixed stars every day around culmination. Item [b] earns a truly special place in the Bizarrity-Hall-Infame, by the astonishing fact that, again: no one has noticed this for the last 2000 years.

I2 In the 27° 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 1/15° of a “part” or μερος of the zodiac as: 1/15° of a zodiacal sign. But μερος (meros) was just an ancient unit = circle/48 = 7° 1/2, of which 1/15° exactly agrees with Archimedes’ correct half-degree testimony.

I3 Aristarchos’ famous 87° half-Moon-elongation wasn’t empirically a precise figure, as modern (and seemingly Hipparchos: §D2 above) assume, but a lower-limit.

I4 From tiny solar declination-motion near equinoxes, uninformedly sneering92 amateur-astronomer-Ptolemist (incl. MacGenius Swerdlow: [22 fn 8] doubt ancient solstices could...


87 Sizes’ 5 farces & Aristarchos’ 1/2-Moon-elongation 87° lower limit: www.dioi.org/jeo02.pdf, Rawlins 2008R fn 37 vs eq.15 from Sun-distance 1000° to 1000° altered limit to 89°. (nearer actual 89°.85.) For “part” (μερος or meros) = circle/48: see Neugebauer 1975 p.652a, & Rawlins 1991P fn 6.

88 Those wondering whether Swerdlow&Gingerich are primarily scientists or carreerists, cannot miss their amusingly indiscriminate alibis for Ptolemy’s fakes, at www.dioi.org/jk02.pdf, Rawlins 2018U §§23&B2&B3 and fn 2&3. (And Duke at fn 69 above, vs Rawlins op cit eq.5$k&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 “37° declination-inferiority.” Besides science’s inclinations see [§A3 on Swerdlow’s & Evans’ innocence of equal-altitudes, the obvious ancient solstice-finding method, these 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, Kalippis, Dionysios, Aristarchos, Hipparchos, BM55555. And these doubters of ancient’s solstice-accuracy (plus fn 27: Jones) are now confronted before with recently recovered papyrus P.Fouad 267A, testifying to a Hipparchos solstice accurate to ordmag 1°(4 fn 20[C]). Preference solstices over equinoxes for year-length-determination becomes...
be good to ordnag 10, which invites equinox-vs-solstice accuracy/comparison (2 ¾)N.

16 Rawlins 1991H found the Babylonian yearlength on famous Astronomical Cuneiform Text 210 was (§F2) based on well-known Greek solstices (¼31/6/27 & ≈ 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 (§37&fn 120).

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

18 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 1200s). F.Ragep 2010 p.121: undeniable but also (for over 300) HIA-unpublishable. Despite null ancient attestation of their speculation, Ptolemaists insist (fn & 127) this is because ancient scientists kept only observations agreeing with theory. (I.e., they cheated.)

A. How could Ptolemy’s solar “data,” all off by a degree happen in the 1st place, outdoors? B. How did it happen that Hipparchos’ Sun observations’ average disagreement with reality were merely two or three times their disagreement with his tables, vs Ptolemy’s fifty?

C. How could Greeks just copying predecessors achieve their many accurate discoveries? (E.g., Greek vs real ancient monthlengths, Rawlins 2017E §§B2-B4: startling proximity.) However, most scholars (A.Pannekoek, R.Newton, Y.Maeyama, J.Brandt, & P.Zimmer among the welcome exceptions) reject ancient high scientific accuracy (fn 1&35 here), undeterable 2016 subtractor D.Scheglov even (fn 13 here) calling it a “delusion.”

19 Aristarchos discovered precession 150 before Hipparchos: §G3.

110 Rawlins’ order-of-magnitude estimates of too-undependable Greek accuracy (§I1F): 1° for lunisolar diameter; 1° for month; for solstices; 1° for equinox taken on Alexandria’s ring (fn 70 here: Almajest 3.1); 10° for sidereal year (fn 114 here); 1° for observer latitude (see fnn 39&100 here and §4’s Table 1 & fn 38); 1° for 500-mile North-South arcs on Earth (fn 111); 0°.1 for star declinations (fn 100); 0°.1 for lunar-libr-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); 1° for lunar-eclipse-prediction (fn 97 here); 1° for lunar eclipse observation (Rawlins 1985G pp.255&265); 1°/century for mean motion of Mars (and maybe Venus); 1° for all three adopted months (synodic, anomalistic, eclipse), each correct to better than one part in a million. Historians-of-science like Scheglov show no sign of awareness of any of these Greek achievements.

obvious from, for instance, Britton 1967 p.29. More generally, §8’s key Obvious Question C jokifies fn 8’s Swerdlov-Gingerich-Scientific-American’s Ptolemy-exculpation-theory. 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 (§18) from a projected Rawlins paper (fn 74 here; precessored text at Rawlins 1999J), along with none scrambler-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 ordm 10°: 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 2018B §B4. Mars (and Venus) mean motion accuracy c.1°/century: Rawlins 2002V fn 26. Contrast to 1° here, and to the umpteenth fruitless metrological analysis of Eratosthenes’ Earth-circumference, Scheglov 2016, www.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-promoted by HS 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 §15 (§D), DIO’s Letter-to-Isis’ hiding-since-receipt Editor H.F.Cohen.

111 At the 1984 Greenwich Centenary, DR presented evidence countering Neugebauer’s denial (above: fn 90, vs fn 13) of the existence of organized science in antiquity, by 3/4-unstratching 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 unstratched 42° longitude-difference between Carthage and Persepolis is correct to ordm 1°. NB: The distorting remains of accurate ancient geographic error 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 seemingly 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), Kalippic, & 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 .75 or 4/3 (1 ¾)F&M.


113 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 (§103 here) that Hipparchos had full spherical trigonometry, see www.dioi.org/cot.htm#tvv.

114 Rawlins long insisted that the Almajest Mars mean motion was based on ratio 152145/329621. Inspired by Duke’s skepticism, Jones in 2002 discovered it was based on a different cyclic ratio that Rawlins had carelessly neglected, residing, ironically, in the Almajest §G3. For decades, historians-of-science argued (e.g., fn 19) against Hipparchos’ possession of spherical trigonometry, a position now indefensibly obsolete: for a pioneering all-inclusive compendium of four plain evidences (§103 here) that Hipparchos had full spherical trigonometry, see www.dioi.org/cot.htm#tvv.

115 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 motions upon empirical integral-return ratios.

116 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°14’ = 1°309, which Tihon was 1° to reveal.

117 In 2002 K.Pickering discovered that original locations of dozens of erroneously placed stars in the Almajest 7.5-8.1 catalogue can be reconstructed (occasionally via spherical trigonometry), but it was repeatedly found that repairs could only work with coordinates from Hipparchos’ era, not Ptolemy’s; sixteen years later, Ptolemaists 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.
In 1982, Rawlins sought the epochs $E$, as well as the errors $x$ in the observers’ assumed latitudes $L$, and their standard deviations, for all 4 known ancient stellar-declination observers — Timocharis, Aristyllus, Hipparchos, Ptolemy’s anonymous observer — through bivariate least-squares testing upon these observers’ star-declination data.\footnote{In 1994, ex-...}

Rawlins 1994L §F5-F8, where nulls show Timocharis (known observations $c. \sim 300$-700), 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 this, but instead of $DIO$’s bivariate least-squares (Rawlins 1994L Table 3 results), he independently finds observers’ epochs $E$ and latitude-errors $x$ by double-monovariate, 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., Aristyllus’ $L$’s standard-deviation (compare www.dioi.org/bjth.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 §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). $L = 30°59′$ (arctan 3.5; above, in fn 25), erroneous by $−1°$, which obviously is much-too-large for a regular outdoor observer, and also is in $15°$ 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 reversed the reverse, finding $L$’s $σ$ first, and then again $E$’s $σ$ to get the other variable’s ($E$’s) “accuracy” similarly, the result would have been informationless zero! (See §C11.) There are further oddities. $Ibid$’s eq.1 sets $O−C = C$ to C. The paper’s $O−C$ graphs are rendered C−C. 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 — getting the reverse, finding $L$’s $σ$ first, and then again $E$’s $σ$ to get the other variable’s ($L$’s) “accuracy” similarly, the result would have been informationless zero! (See §C11.) There are further oddities. $Ibid$’s eq.1 sets $O−C = C$ to C. The paper’s $O−C$ graphs are rendered C−C. 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ordmag 1° accuracy, before Ptolemy’s fateful stades/degree scale-shift (§111) stretched the map East-West, inflating longitude-differences?

I22 In 1982, it was shown that Eratosthenes’ original Earth-circumference C was neither of the long-accepted (variably rounded) stade-values, 25000 or 252000, but instead was 256000. In 2008, 26 later, all 3 were checked against Eusebius’ long-neglected Eratosthenian Earth-radius \( r = 40800 \) stades, and 256000 was the only one of the 3 that fit this \( r \). (How will Carman & Evans 2015’s authors explain not mentioning this match, to \( I \) part in a thousand, when their own cited sources show they knew of 256000? See fn 10 above.)

I23 It was simultaneously found that Earth-radius \( r \) was the empirically primary datum, consistent with being based on Sostratos’ non-astronomical Pharsos method (§24), which directly finds \( r \) from his Pharsos Island lighthouse (in Alexandria’s harbor), but 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.)

Proposed Sostratos ingenious idea&achievement: mount Pharaoh’s flame precisely \( h = 300 \) feet or \( 600 \) stades 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 overseas 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 §11; Rawlins 2018V).

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

105 Rawlins 1985p.265, taking an idea due to (ibid n.22) Gossellin 1790, suggests an ancient, adjusting for the 700 stades/° — — 500 stades/° switchover, stretched longitudes by 7/5, mis-imagining they were based on land-surveys (11 3°). Or by 43/4 via Poseidonios’ \( C = 240000 \) stades — 180000 stades. Proposing Greeks organized to compare eclipse observations (fn 13): Rawlins loc cit, vs fn 90 above & 2016, why the nearby Verde Isles, westernmost known land, chosen as Marinus’ \( \phi \) longitude (www.dioi.org/p501.pdf, §A), to kill longitude sign-muffs like those (fn 97) cursing Sheghelegov 2016. [Speculation 2018/7/15. Marinus = pseudonym, like “Ptolemy”? Or map-title from maritime Tyre?] C

106 \( C = 256000 \) stades 1° induced from a Nile map’s latitude intervals: Rawlins 1982N pp.212, 214, 216-217; Rawlins 1985p.259; Thurston 2002p.66. For which C fits Eusebius’ \( r = 40800 \) stades: just multiply \( r \) by \( \pi \); 1: 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 than 100 AU fits 256000 fits.

107 Rawlins 2008Q eqs.2&11&28; eqs.14-15&17-18 for \( r \) as Sostratos’ distinct empirical measure. See ibid §A[a] for the Pharaoh’s 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 Pharaoh’s topdeck exceeded a flammable time, unmissably-enormous alert and gauge of \( C \)‘s size. (Elementary illustration-by-extremes that different results ensue for flame vs sunsets: Rawlins 1979 §A.5.) To say it needed direct atmospheric planet-radial sighting data right 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 one implication 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.


118 Of course the transcendent Earth-radius C, the radius of the Sun, must be the same as C, or else Earth and Sun are not geocentric. And the Sun’s radius C must be, as E.M.Standish insists, more reliable than an academic journal Scientifc American 1979 May. Interval between times of sunsets seen from Pharaoh’s topdeck exceeded a flammable time, unmissably-enormous alert and gauge of C‘s size. (Elementary illustration-by-extremes that different results ensue for flame vs sunsets: Rawlins 1979 §A.5.) To say it needed direct atmospheric planet-radial sighting data right 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 one implication 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.

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Phys. 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 cyanosure realize that the 6/5 factor has been standard among navigators&astronomers for over 100? (All scientific navigation manuals have horizon-dip shrunk by \(\sqrt{\frac{5}{6}}\) vs straight-line geometry) and horizon-distance expanded by \(\sqrt{\frac{6}{5}}\) 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 & Poseidon dorontos 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 be rational discussion here when the only theory that fits all 3 data (both C, as well as the standard 185m stade) is not even understood by those who keep prominently churning out forced metrological retreats (as recently as late 2016! — 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.

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 mythical runty “Eratosthenian” stade is obvious to most specialists, e.g., P.Gosselin, E.Bunbury, D.Dicks, O.Engels, J.Lindgren, L.Berggren, A.Jones (more at idem). No one (more at idem) has ever even speculated indisputably 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 2012F fn 2, self-contradicting the titular contention of Rawlins 2008Q & note ibid §Aa(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 ([§26]) 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-Meröe, each good to ordmage 1 for 700 stades/stade: 7°/8 + 7°/8 = 14°/14. (Rawlins 2009S [C notes Philo’s solar work at Meröe, presumably for an important survey.) Testimony for early-Ptolemaic meridians in 60ths: 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 §Aa(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.)

Yet the right solution is: [1] directly for radius (fn 107), not circumference; [2] geographical ([§23], not at all astronomical; [3] physical ([§26], not metrological.

127 But whence arose the linchpin 185m stade? Before imperial standardization, stades varied ordmage 10% from locale to locale, the smaller among the early ones now naïvely, selectively, anachronistically used by Eratosthenes’ mod-groups to rig right-on correctness for his too-big C. In 2014, it was seen for the 1st time that the much-attested (fn 111) early 3rd century BC Greek rule of dividing terrestrial meridians into 60 parts (not 360) — step-one of C’s potential sexagesimalization, conventional Greek fraction-practice — could’ve led naturally to the Ptolemaic empire’s regularization of the “stade” by defining it, parallel to our definitions of meter and nautical mile (fn 111), as C/60/60/60 = 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.

128 This inevitably-uncertain speculation implies that, c.300 BC, presumably while surveying the new Egyptian empire of Ptolemy I (Greek pharaoh – 323-284), Greek scientists astronomically determined accurate Earth-circumference C, before dividing it by 60 to “define” the 185 meter stade (22 fn 49). Who earlier had the required science? Gradual-grade topography? (Camels?!) Was the measured arc along 29.9° E longitude, Alexandria to Meröe’s latitude (nowhere interrupted by the Nile or sharp mountains): 1578 km = (in 60°) 2 3/8° × 14°/14 = 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 ordmage 1 or 1 nautical mile. Was the hypothetical survey supervised by contemporary scientist Timocharis, demonstrably expressly (fn 99) in 1°-accuracy latitude-fx via ringed instruments?

129 For over 100’, at least from H.Berger, scholars have wondered if the early larger 30000 stades Earth circumference C, cited in Archimedes’ Sandreckoner, was due to Dikearchos (c. – 300). In 1994, DIO showed\(^{112}\) that if Dikearchos measured sea-horizon dip from aton conveniently-seaside Mt.Pelion accurately (1°/110) & 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 ordmage 1%.

130 DIO produced hitherto-unperceived & thitherto-unpticed physical evidence that the Galactic Equator appeared on ancient Greek celestial globes: www.dioi.org/fff.htm#phod.\(^{113}\)

112 Kallippos’ 365 1/4 yearlength ought to have been found by him from the gap between his solstice & Meton’s. Meton’s calendric Solstice-hour was Athens’ day-epoch, 18°, for the day containing the solstice, not its exact time (Rawlins 2018U §I4), thus – 431/6/27 3/4. So, adding 102 Kallippos years, or 37255 3/4, that date reveals Kallippos’ epoch as – 329/6/28 1/4, dawn, which is late by 3", thus accurate within traditional 1°/4 precision. Moreover, the New Moon at 4 AM was only 1" after 3 AM solstice, a once-in-centuries ideal conjunctive hour for his lunisolar calendar. Kallippos increased his yearlength in centuries ideal conjunctive hour for 37255 1/2 by 102, finding (as it happened) exactly 365 1/4 days. His solar motion was codified into his famous Kallippic 76-year cycle of four 6940° Metonic 19y cycles minus 1d, that is, 365 1/4 per year. Due to Meton’s 7°-truncation error & his own Solstice’s +3 3/4” error (interval’s net error 3h), he accidentally arrived at history’s 1°-Julian calendar, nearly 3 centuries before Caesar’s Sosigean.

113 Note: Superscripts occasionally used here & below: d = days, h = hours, m = minutes. Lunar: u = synodic months, v = anomalistic months, w = draconitic months. Solar: g = anomalistic years, y = tropical years, s = sidereal years, K = Kallippos years. (Degree-remainders merely signify 360°) Tropical-years here can refer to real ones or the Metonically-defined “tropical” (or Easter) year 235 1/4°.
Sourcing Ptolemy's final lunisolar ratio.\(^{114}\) 105416\(^{6} = 8523^{2}\), occurred 2 decades ago (all 10\(^{[!]}\) digits exactly elicited) by test-exploring Greek awareness of the 800\(^{0}\) sidereal eclipse-cycle-nest (1/5 of 800\(^{0}\) cycle attested: Geminos 8.40-41): solution, awareness, & nest not suspected ere Rawlins 1996C eq.31. (Sidereal year accuracy: \(\text{ibid In 110} \).)

More muteness greeted DIO's 2002-2003 discovery that all 3 previously unsolved, annually used 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 4000\(^{0}\) span (3\(^{°}\) century BC to 2\(^{nd}\) 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 13th BC (§316). 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 shuninity, frustrated by inability (like §33) to find math error or alternate eclipses to show non-unicness, 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 (10\(^{th}\) earlier) estimated Babylonian observational uncertainty as about \(\pm 3500\). Surprise realization that Hipparchos' famous 600\(^{0}\) lunisolar tables effectively went back just that far only occurred\(^{117}\) in 2015.

I should gauge the proposed eclipse-ratio method by comparing it to what may be verrry loosely referred to as the "competition" (e.g., fn 119). Facts: No other method is attested. (Twice: \(\text{idem}\).) No other method is so simple & immediately-direct-to-the-result. No other method could ensure such high accuracy, 1-part-in-10\(^{8}\), 3 times out of 3, eliminating false nearby period-ratios (§337). 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 6,4-digit components — on-the-nose in each case: see www.dioi.org/thr.htm#ctqp.

114 PlanHyp 1.1.6 (Heiberg 1907 pp.78-79 or Neugebauer 1975 p.901 eq.3): 105416\(^{6} = 8523^{2}\). Solved: Rawlins 1996C eqs.20-31. Thanks to K.Moesgaard for a perceptive correction.

115 I36. The admirable exception to Hist.scii 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.574. Initial Muffia tactic vs R.Newton & DIO was non-conviction. But Isis Editor Margaret Rossiter's publishing DIO-respecting Thurston 2002S defined the 30 shun, inspiring how (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's 2002S refereeing (2000), the usual indiscriminate (fn 66) unreviewed anti-friends prepared for launch: Schaerer 2001 (Pb), Schaerer 2002, Jones 2002E (2\(^{nd}\) to Pb), Duke 2005T; Duke 2008W (Pb), Jones 2010B (2\(^{nd}\)-history #1 blackballei no matter how: §§B-G), all now on JHA's certified-Premier (fn 42) board. Re JHA's prior villain, we quote from (fn 28: "Rewards handed out to those who attacked the R.Newton satan include JHA boardship (R.Newton 1991 fn 2) & a MacArthur for miss-man [fn 96 here] Swerdlow. (It's hard to find good help anymore.) . . . maid-men Evans&Schaerer were elevated at JHA not long after their massive bungled 1998&2001-2002 attacks on Rawlins. (The unsubtly here may actually be deliberate.) Selecting boardmembers [thusty] will damage mean-IQ atop JHA for decades to come.")

117 For this recent shock, see www.dioi.org/thr.htm#bbv. Re Hipparchos' 600\(^{0}\) tables, see Pliny 2.9.53.

118 DIO's technique, 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 Anomalous Cycles — is not cited at all in Britton 2007 p.124 (System A ratio misprinted), though the same DIO issue appeared in is inimically cited at Britton op cit n.66. Similarly, defense of Polymny's star-catalog authorship by Pedersen 1974 (pp.249&258) omitted 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. §G5). But cf. R.Newton & Davies' hero-Ptolemy, Jones 2008C (2\(^{nd}\)) (also) as poles outward each in its own distinct use of R.Newton & Davies' "whole" system.

119 Babylonian lunar-six proposals for determining the anomalistic month can't work for the draconitic month — therefore a different faretched explanation needs concocting. Sometime, DIO's — & Polymny's! (fn 119) — uncited (e.g. fn 121) eclipse-cycle method www.dioi.org/thr.htm, solves both. Exactly: A parallel case: to explain Polymny's huge solar errors, defendants 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 resolutely proven by Neugebauer in 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#epcc), or [d] explain why each solution emerges as a ratio, a glaring feature of ALL pre-Ptolemy adopted lunar motions, which by contrast to orthodoxy is accounted-for perfectly inevitably by the eclipse-period-ratio solution. Jones’ blindered private rejection (by subsequently-undercut reasoning) of the DIO draconitic solution, flies all of the overkill-numerous, solid, unambiguous evidences consistent with said theory, particularly its PRECISE match to Hipparchos’ draconitic 5458# ≈ 5923# ratio (Almajest 4.2) by pairing an early apogee eclipse, −1244/11/13, with his Rhodos-observed −1401/27 eclipse, the very same perigee eclipse which he uniquely had also previously (Almajest 6.9) paired with a less early apogee eclipse (−719/3/8) for exactly the same draconitic purpose, with inferior result — inevitably, due to shorter timeframe. Comments: In all history, no astronomer but Hipparchos ever used an apogee-perigee eclipse-pair. Scoffs at the theory’s outrageousness vs orthodoxy-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-impotence in finding DR-errors? Or alternate solutions? Answer (§34): there IS no other umbral lunar eclipse-pair whose integral-months ratio precisely, proportionally, directly yields (by Almajest 4.2&6.9 method) any of [I36’s three attested, never-before-solved Greek integral-ratio motions, with: both eclipses visible in Greek-Babylonian region, latter eclipse within 50’ of discovery-date (c. −262, −140, + 136, resp), earlier eclipse not ere Almajest 4.6’s −720/33/3 (oldest eclipse-data historians-of-science accept that Hipparchos 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: $100000 for 3277#−32152#; $200000 for 6247#−6695#; $30000 for 5458#−5923#; $40000 extra for all 3 relations. [To certify the bet, DIO will deposit $50000 with the National Academy of Sciences, if it will hold same for winners until time’s up.]

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

J1 The foregoing suggests shortcomings in the modern ancient astronomy subjectref: [a] Scrupulous & neutral refereeing. [b] Turfless generosity & citational integrity. [c] Open-minded curiosity & 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 archaeologists with an attitude of nonend, even supplication, to allay (non-imaginary) fear of being unpublished or outrage blackballed, as if such etiquette-issues outrank (§5H) 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 stop the current dramatizing historians-of-science can ever grasp the empiricism & humanism of Hellenistic science. [f] Essential, genuine neutrality and curiosity (hardly compatible with a cult’s insistence on aggressively protecting sacred viewpoints for decades on end), enhanced by willingness to hypothesize-explore — ever subject to evidence-congruity (e.g., fnn 16&83 vs fn 20&25&40). [g] Philosophy-of-science & common-sense Occamite

theory of fabrications explains both and to the Almajest’s 5# precision. Therefore, in each case (lunar or solar): which approach would Occam prefer? 127 128

127 Half-dozen evidences for Hipparchos as author of 5458# = 5923#: Rawlins 2002H §C.
128 Neugebaurians long taught that 6 cuneiform-tablets’ lunar calculations for c. −200 proved chronologically Hipparchos (c. −130) took his draconitic equation from Babylon, until Rawlins 2002H §D1 pointed out: [a] the only 3 early tablets using his equation were the only 3 not dated on the clay, and [b] there’s a 7th tablet that is clay-dated, using his equation, but the date is after Hipparchos. As willfully as in fn 28, Jones ignores (private communications) not only this & fn 123’s flock of coherent evidences, but also rejects an unexpected key eyeopening spinoff-benefit: recommending responsibly increased caution when time-placing non-clay-dated astronomical cuneiform tablets: *ibid* §D1.
References


O.Gingerich 2002. Isis 93:1.70.


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


Alexander Jones 2002E. JHA 33:1.15.


stand indefinitely-uncorrected his own miscalled-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-inheritance’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.
Ptolemy-Defense Cult Lays Yet ANOTHER Egg

On Own Already-Unwipably-Eggregious Faces

ArchonBishop of TruthBury’s Trowel & Slander

We now analyse the latest installment, this time from the Journal of Astronomical History & Heritage 2014, in a half-century serial display of unfailing invalid archonology-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 greatest compilations of establishment pseudoscience literature, all accomplished in the service of attempting to save the reputation of a “scientist” icon who was not a scientist at all, but (§A below) just a mathematician who faked science. Badly. Among Ptolemy’s numerous clumsy Almajest[s] [the silliest on scintillating display here in §2] were, e.g., [a] 2 different dates (37 days apart) for the same Venus maximumelongation, [b] plagiarizing Hipparchos’ star catalog, [c] impossible-for-regular-observer ignorance of his own city’s latitude, [d] 4 alleged solar “observations” which were (as no historian-of-science denies; or admits) many times nearer Hipparchos’ old indoor tables than to the outdoor sky’s actual Sun.

The JAHH paper in question, J.Brandt, P.Zimmer, & P.Jones (below known as Brandt et al 2014B), attempted mathematical analyses of the Almajest’s stellar declinations, observed by four successive ancient Greek astronomers over nearly half a millennium. The journal and authors contend that the dozen Almajest-contemporary declinations could be Ptolemy’s observations, never warning the reader that zero evidence is provided to establish that claim, while simple, definitive, long-published, referee-urged proof to the contrary is below shown (§C5) to have been deliberately omitted. The paper’s “bivariate least-squares” statistical analyses were not bivariate and thus didn’t exactly find any least-squares biv 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 patently absurd historical lie, viciously (fn 4) debunked by those HAD archons who long ago mistakenly decreed Ptolemy “The Greatest Astronomer of Antiquity” and thus have faces so at risk of mega-eggitudinal disgrace that they must forever encourage pseudo-science-for-The-Cause of forever-pseudo-controversy, cult-obediently incapable of admitting that any skeptic has ever made an indubitable contribution to knowledge. Below, at §B, the most recent misfire (Brandt et al 2014B) is put into the context of decades of like uniformly baseless mobaganda (though those interested only in 2014’s 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.

2 Prime smear against dissent is Insanity (as with media on pols’ heresy) by megafunded establishment-polishers & darlings Gingerich (fn 16) & MacGenius Swerdlow (fn 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 18 §§C31-C32 effusions is not a seri-

2 fn 52) risking rational debate with DR, written

3 See Gingerich 1976 for 2 prominent examples of Believers (O.Neugebauer & himself) who got way too deep into worshiping 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 stuff him yesterday, if they discovered his security-breath. So he lays his stinking face 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 (fn 18) — has long broken backs (fn 18) the slander that no one but a CRAZY person could suspect dishonesty of the history-of-ancient-astronomy field’s ultimate hero, ancient astrologer Claudius Ptolemy, who 4-times-out-of-4 reported observations of the Sun that were undeniably but captive-journal-unprintably 50 times closer to Hipparchos’ 280’-old indoor tables than to the real Sun. The prime forums permeating this fantastic but profitable joke on academia and the public, for consecutive decades, are the Journal for the History of Astronomy and the American Astronomical Society’s knowingly unsupervised Historical Astronomy Division (HAD) — which we shall refer to as the “JHAD” combine. Seemingly incredible fact of the last 4 decades of the Ptolemy Controversy: not a single published defense of Ptolemy has ever been valid and must have not been particularly smart or honest,2 as we are about to see again in this section below. But as with oft-crafty Rains, the paper below is written exactly the way 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.
A

The Shy Archer Triggering the Present Paper: Politics vs Science

A1 In 2011, DR belatedly responded to much-decorated astronomer Jack Brandt’s welcome request to consult a 1982 unpublished DR ms on the 54 star declinations observed by ancient astronomers Timocharis, Aristyllos, Hipparchos, and (allegedly) Ptolemy — reported and analysed at Almajest 7.3. In 2014, much-too-Shortly before the resulting paper Brandt et al 2014B went to press the Journal of Astronomical History & Heritage’s Editor Wayne Orchiston asked DR to 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] Ptolemites have asked skeptics to help them avoid blunders, even while undeterredly determined to promote more cultism.) And, indeed, no changes were made.

In response to central points challenged by DR’s scientifically detailed 2014/8/26 referee report, www.dioi.org/jauq8.pdf, though an irregular sprinkling of (non-space-expanding) alterations was effected. The timing suggests that the paper’s case for Ptolemy as outdoor observer was not going to be derailed by mere evidence, much less a full discussion of issues. Some other referees might care enough to regard such treatment as insulting — which would only divert from the main point: it’s counter to a journal’s obligation (and own best interest) not to take all pains to make the entire process accountable and competent articles possible. (Not exactly an infectious ideal at brother history-of-astronomy journals, either.) The irony here (as is obvious from correspondence: fn 28): DR went to plenty of trouble in a cooperative, generous attempt to help JAHH be a more accurate and competent journal. It was disappointing to find that such considerations rank nowhere at the Journal of Astronomical History & Heritage, probably (despite JAHH’s pathetic ultimate cultishness and non-bravery) less from iniquity than from JHAish inability (increasingly typical of the whole ever-less-scientifically-skilled history-of-astronomy field) even to begin to tell balanced, competent technical research from cultist apologia.

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

A3 DR’s reply, www.dioi.org/owuq8.pdf, tried [A] to test whether demanding the paper’s softening was in hopes of making DR go away; and [B] to check out JAHH’s biased 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 clique — vainly asking, www.dioi.org/owu2e, 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

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 +157) the split between Clean Dozen & Sick Six stars was overalplessly clean: §C17. (But Brandt et al 2014B didn’t cite any of this.)

As we counsel 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.Aabo, R.Newton, & S.Goldstein, we realize that they are being replaced (as HU’s Harry Woolf warned DR 50 ago) largely by non-scientists. The new breed has proven admirably industrious, but too-often inadequately trained in science’s skills, criteria, standards, principles, and especially approach to evidence.

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 21.1 §§12 & 13] that DR wants to be shunned.) DR’s atheism heaps extra aggravation upon this self-described "practicing Christian" of the Mennonite cult. Students of the psychology of hate might profitably investigate this 40y obsession: bizarre details (& Cardinal Manning’s perceptiveness) at §2 fn 5.

12 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.

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 start was verified by test in the next-last paragraph of www.dioi.org/owu31.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 (isn’t this what competent fields’ editors do & are for?) — the job would’ve taken (mag an hour) & sent the revision back for DR’s OK; but, then, what if DR had replied “Done”? Fixers keep their plans flexible.
tanut 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 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.html/#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 JAH 2015 and [11] Isis 2017.)

Embodying a third of a century of the field’s proud progress:
[a] JAH in 2015 exiles a referee who expended extensive time and labor to respond scientifically to JAH’s S.O.S. for assistance in cleaning-up a paper which was beyond that journal’s technical capabilities, as thoroughly demonstrated below. Also: some among the authors may have found themselves rushed or only partially consulted, and out-of-available space for their paper, as a deadline approached — and out of time, having waited too long to call in expert advice. (But this doesn’t excuse the central omission: Ptolemy’s −14° error in latitude L, emphasized in www.dioi.org/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 JAH 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: § 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 JAH 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/oww2.pdf, to WO and learned of an arbitrary act which at last unveiled a proud new addition to JHAD covering-archondium (§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/oww3.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 JAH of his gross failures of editing (& math!) on DR’s not-yet-written 2015 paper (expanded to §2 here), www.dioi.org/pm.pdf, which ultimately granted total freedom to WO. (See www.dioi.org/iss.pdf for Isis’ like achievement in time-disjunction.)

A6 We will shortly move on to putting JAH’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 squishable 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 ... apologia for Ptolemy’s observership is any more credible than adducing planted eggs to prove the Easter Bunny is real.

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

B1 Does it tell us something about the state of soft academe that the Journal for the History of Astronomy, the “premier”15 journal of its field, has for decades used pseudo-science and deception in the shall-we-just-say extreeeeeemely 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 longevity #2 official, Harvard’s Astronomy 101 teacher & deft Disraeli-esque trowel-wielder Owen Gingerich, has repeatedly bidden16 this ancient faker and superstition-proped17 as “The Greatest Astronomer of Antiquity”. Claiming that all who question this eminently-sensible-to-him proposition are the insane18 parties to the simmering-if-generally-suppressed dispute inevitably triggered by such superlative sales hype. 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 archon-bishops, thereby improving odds for future conference-invites, publication, posts, grants — all with confidence that no matter how outrageously unlikely their arguments, they had nothing to fear from contradiction, in any of their captive journals.

B2 Ptolemy claimed to have outdoor-observed a 140 AD solstice, though his report (Almajest 3.1) typically disagreed with the real Sun by 1°1/2’! — or a degree and a half, which is over FIVE TIMES the angular distance from the Sun’s center to its limb (edge) — this, while his report agreed with Hipparchos’ indoor tables to within V’ or a fraction of an

16 Gingerich 1976, Gingerich 2002. Disraeli (L.Strachey Queen Victoria 8.3, 1921 [Harbrace pbk p.244]): “when you come to royalty you should lay it on with a trowel.” Gingerich summed-up succinctly: 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.
17 Psychoanalyst Gingerich’s intended-to-be-compassionate 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 earlier 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 junior-highschool level. The 2nd was a clumsy fantasy.  

[1] Near a solstice, N7 alleges it’s impossible to measure accurately the time of maximum height of the noon Sun, since from day-to-day it’s virtually not changing then. So referreeing by Phi Beta Kappa (fn 20) and by Reverend Gingerich, as usual (one might almost say: as-always, given the reliable brand of sheeple who man or oldboy Hist.sci’s most prominent forums) 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 outdoor data to agree with indoor theory. (N.B.: This would naturally justify destruction of the unused data.) Comments: [a] Even if it were true, the preferred alibi wouldn’t explain physically-impossible repeated 1° off-the-mark Ptolemy “observations” that could never have been made outdoors in the 1st place! Especially again & again & again & again. (The human eye can see to about two ormdags 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 the 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 knowen within 2%, all 3 monthlengths accurate to 1-part-in-a-million or better, observa-
tories’ 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 reason 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.

[20] 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 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 Rhodes Island observatory (geographical latitude 35°53′) stood c.5° north of Ptolemy’s Alexandria (L = 31°12′). So Schaefer 2001 contended at enormous length, in (yet-another!) JHA-Pb-anti-RN paper, that the catalog could’ve been observed from Alexandria nonetheless because aerosols (atmospheric crud) blocked low stars. Among Pickering 2002A’s unanswerable responses: if this were the problem, the southern limit of the hundreds of stars in Hipparchos’ Commentaries would also be raised, so it should be 5° higher than Ptolemy’s, but “it’s the rules: END OF ANY REAL CONTROVERSY. Why didn’t Schaefer know that? Simple: no JHA pseudo22 referree noted that, throughout his 42pp paper disputing Hipparchos’ Almajest-catalog authorship, Schaefer had never consulted Hipparchos’ Comm’s 100’s of star-positions. His later unembarrassed but embarrassing re-tort claimed that no one could know anyway which stars Hipparchos Comm was referring to. Which revealed he had no idea how Manitius 1912-3 & Graffhöf 1990 had used the various phenomena, www.dioio.iff.htm#gnjm, to pin down virtually every star unambiguously. And Duke 2002C p.33 cleverly noted 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 Graffhöf 1990’s brilliant examination had already shown years earlier. None of these definitive findings has caused Schaefer or JHA to retract anything.

B5  It might seem suspicious to some that Ptolemy’s entire celestial system (Sun, Moon, planets, stars) has accurate mean longitude only for Hipparchos’ time, the mean error growing until it reached ~1° by Ptolemy’s +137 epoch. However, ignoring R.Newton 1977’s devastating new fractional-ending proof (summarized: 2° 11′ 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 astrolobe, 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 astrolobe (Almajest 5.1 & 7.4) doesn’t spin about the ecliptic pole but instead about the equatorial pole; so an outdoor Ptolemy’s longitudinal astrolobe-ops-mis-set by m = −1°.1 would’ve caused the instrumental & actual ecliptics to tilt-separate from each other by m = tan m = 29′ (idem eq.2, where ϵ = 23°.7, the obliquity then), thus causing an error-wave in celestial latitude Δλ = 29′ cos ϵ and an error-wave in celestial longitudes equal

21 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 perms 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: §J2 [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 §3 fn 96.)

[C] Not even recent miraculous recovery of the 1900-old papyrus P.Found 267A, with solstice’s time correct to ordmag 1° (actually to a fraction of 1°, by chance) has yet enlightened any astylist.

See Rawlins 2018U for full details of ancient solstice-determination, and DIO’s new formula (ibid §H) accounting for ordmag 1° errors in such, inevitably but trivially due to asymmetry from Earth-orbit eccentricity, errors which Swerdlow & Evans couldn’t even quantify, so ere prominently (JHA & Oxford Univ Press) displaying their own [A]&[B] double-ignoreance, Swerdlow of course adding a (pricelessly ironic) sneer of imagined superiority: R.Newton 1991 fn 20.

For almost 40°, virtually all journals in receipt of a DR paper on antiquity have not had the imagination to start elsewhere than Gingerich, when seeking refereeing, e.g., PASP, JHA, Isis, Nature, Daham. 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 politicially-uncacceptable expertise, by forever-cutoff of correspondence): JHA and JAHHT & [Isis].

22 Ever-fertile Evans 1987 p.166 even argued that 6° of rocks or trees might’ve blocked Ptolemy’s view. The easily testable flaw in this alibi is explored in mathematical detail at §2 JL.

24 Don’t miss www.dioio.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 triumphant transition into lapdog ever nearer the ultimate intimacy it aspires to: lapdancing a needy establishment.
to $\Delta = -29\sin\lambda\tan\beta$, 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-opinioned Ptolemists robo-countered this new shocker with their usual standard-weapon: scientific inability so truly embarrassing as to raise the question of whether impenetrable Ptoleism has become a medical problem. Exhibiting the science-grasp of Ptolemy’s fellow-crank-liar F.Cook, MacArthur-Genius N.Swerdlov 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 §112) or terrestrial (DIO 21 §3 311) longitudes’ conversion to great-circle measure entails multiplication by $\cos\beta$, but gt-circ $\Delta\lambda$ can’t exceed 29°. Swerdlow’s response: he hides (DIO 3 §5 §14). Meanwhile, Evans’ attack on the (SB5) absent-error-waves argument confused sine waves with cosine waves ($2[\sin\beta]$) 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 irreplaceable honesty & ideological loyalty to succeed (as he did in 1933) then-Editor M.Hoskin, by taking-up no less than sixty-four handsome JHA papers 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 ord-mag 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 adding a previously unmentioned 1977 eclipse he hadn’t outdoor-measured with) — and continues inexcusably contending25 for ancient errors of ord-mag 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

25 Seeing his own 1981/7/16 record proved DR right on that observation, Evans 1998 nonetheless repeats his 1987 argument that Hipparchos’ ord-mag 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, both his hugely erroneous placements of Spica were merely bunglings of raw observational data accurate to ord-mag 1°. Later, Rawlins 2009E §E showed Hipparchos had made the very same parallactic reduction-slip when seeking Regulus’ longitude, which got the worst error of all his fundamental stars (−35°). When corrected, the error turned out to be merely ord-mag 0°.1. Evans’ furthering (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 tofake that detection’s non-existence, by cultwise noncitation. Their “reply” is their usual: run away and hide. And all Reputable forums (societies, journals, pop-sci mags, & press), which are seen by the 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.

26 Maeyama 1984 is graphical by trial&error and is monovariate ($\chi^2$ in 100), finding nearly accurate values for $E$, though with estimated (nonmathematically guessed) standard deviations $\sigma$. Maeyama 1984 p.308 acknowledges that he had seen DR’s paper in 1983. It appears possible that, until noticing this, BJZ 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 gueswork 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.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 ($\chi^2$) not told of other data which are both — and which definitively contradict Ptolemy’s observship, all of which were communicated to the JAHJ & authors ere publication.

27 Of Alm 7.3’s 54 star-declinations $\delta$ reported by 4 ancient observers, BJZ’s 2014 project examined 53: Timocharis 11 stars, Aristyllos 6, Ptolemy & 18 each. These data had already been studied by Pannekoek 1955 (1° to appreciate the accuracy). R.Newton 1977; also Rawlins’ 1982 bivariate least-squares study, which Centaurus refused to publish, though this scrupulous 46pp paper was sent to K.Moeggard on 1982/7/14, requesting Centaurus publication. Moeggard’s overseer, Editor O.Pedersen, spitefully published instead a mathematically unsophisticated monovariate paper, Maeyama 1984 (received at Centaurus a year later, 27 in 1983 June), whose standard deviations for the four catalog from Hipparchos by (as long suspected among astronomers) just adding 2°/2 of 1°/2 centurial precession onto Hipparchos’ longitudes, a fabrication betrayed by the precession’s falsity (actual precession then: 1°/8.38/ky) which ensured that, after 2.2 centuries of 0°/38/ky 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-catatalog apology flamed out when several brilliant tests (as neither Newton nor DR had thought to do) for the mass-statistical correlation of Hipparchos’ and Ptolemy’s star-places, proving (as Alex Jones witnessed 1°-hand), even to formerly-pro-Ptolemy-as-cataloger Almajest-editor G.Toomer’s honest satisfaction, that the catalog had indeed been plagiarized — thus vindicating Newton’s & Rawlins’ prior pioneering tests and disproving Evans 1987, Schafer 2001, & Schafer 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 ($\chi^2$) 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 $\delta$ 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 ($\chi^2$) not told of other data which are both — and which definitively contradict Ptolemy’s observship, all of which were communicated to the JAHJ & authors ere publication.

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Greeks’ observers’ epochs $E$ were eyeballed not computed.

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

**C5** But the DIO referee report, www.dioi.org/jau8q.pdf, also emphasized that the paper should not suppress a few extremely germane items, fully known to JAHH (through the referee-report), which point in a direction other than its inexplicable Ptolemaist conclusion. (Though R.Neptune 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 $\delta$ via the equation

$$\delta = L \pm Z$$

(minus-sign for southern transit, plus-sign for northern upper transit, where $Z$ complements altitude: $h + Z = 90^\circ$), so $L$’s error carries directly, additively, fully into the $\delta$ data, the systematic error of whose mean is therefore the error of $L$. This would seem to be obvious, but the $L$ researcher ever to perform the test upon star data, to show contra-conventionally the admirably small error in ancient star-observers’ $L$-error, was DR, for the History of science Society: Rawlins 1982G. From the $\delta$ data contemporary with Ptolemy, all analysts since (including Brandt et al 2014B) have concluded that there is but tiny error (ordm $L$’) in the observer’s assumed geographical latitude $L$. So it should not be hidden from the reader (as it is, through Brandt et al 2014B) that when Ptolemy reduces transit data (via eq. 1), he uses an Alexandria $L = 30^\circ$8’ (Almajest 5.12.13), which rules him out as the declinations’ observer since this $L$ is in error by $-14^\circ$ (Alexandria being at $L = 31^\circ$12’).

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

else say that DR was (ibid) merely “interested in checking” the latitude-errors? — as if the discovery of these had been around for years. Why, throughout, is Maeyama 1984 usually cited ahead of DR’s earlier 1982 works, when both are mentioned? It seems especially strange to find DR’s unambiguous priority, in computing separate dates for Timocharis & Aristyllos reported thusly at Brandt et al 2014B p.334, www.dioi.org/bjrz.pdf, “Until the early 1980s [their dates] . . . were taken to be the same. Currently, the dates are considered to be different (Maeyama, Rawlins, 1982a, 1982b, 1994),”

which becomes even harder to explain when we find that the earlier, refereed (otherwise nearly identical) version of the paper, www.dioi.org/bj3z.pdf, has the verbatim-same wording except for the citations, which were parenthetically chronological back then: “Rawlins, 1982a, 1983, 1994; Maeyama, 1984.”


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

had they faked them by adding 2 2/3 centuries worth of his false 1°/cy precession, namely, ticking 2°40’ onto all Hipparchos’ stellar longitudes. (Closely agreeable date, assuming the star-declinations were faked similarly.) No mention of this Coincidence in Brandt et al 2014B! — though urged by referee DR. It has been explicitly well-known for over a century that virtually the same date matches the date for which Ptolemy’s 1025-star Catalog’s fakes would seem correct, were they real, which few scholars believe anymore. E.g., Peters & Knobel 1915 p.15 noted that +58 is the date when Ptolemy’s misprescribed 1025-star Almajest 7.5-8.1 catalog is correct. There is no sign that BZJ knew of this match prior to DR’s referee report, nor does such vital information appear in Brandt et al 2014B subsequent to it, either. This positively belongs at the paper’s p.333, where we instead hear that beside the star-declinations (emphasized): “Ptolemy offers additional evidence for his [(false) precession] value elsewhere in the Almajest (e.g., [Toomer 1984 p.338])” — innocent of the A.Jones-witnessed fact that upon viewing 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 JAHH.

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C9 The erroneous figures for “accuracy” $rs$ in Brandt et al 2011 and Brandt et al 2014B were an ordmag too small, presumably because they were mistakenly found (as hinted at in Brandt et al 2011) by [a] searching monovariately for the $E$ that minimizes the sum $S$ of the squares of the residuals, [b] subtracting the subsequent mean residual from each datum, [c] with the adjusted data, re-computing the problem 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 $0.003$, this translates to $14\&11$ ARCSENconds, 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: $iden$ lists rms values for methods of Maeyama, Rawlins, & BZJ — that agree with each other to a $100^{th}$ of a degree!)

C10 Interlude: From where did BZJ get [C9 [d]]’s wacky idea that one should simply mean the residuals? Answer: from misconstruing p.283 of their preferred prior analysis, Maeyama 1984, where it is stated that (following determination of epoch $E$), “The epoch [where $S$ is minimal] will be our first approximation. The resulting mean deviation at this epoch will then correspond to the mean systematic error in $\theta$. …” 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 0.3333$\cos \alpha$. 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 $1$ to $4$ to $E$. In Rawlins 1994Al Table 3 and below in Table 1, this is roughly true. But no such symmetry appears anywhere in Brandt et al 2014B (abstract) or Brandt et al 2013. (N.B. 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 $E = -258.6$, 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 differ $rs$: Timocharis 0.012, Aristyllos 0.003, Hipparchos 0.004, Ptolemy 0.009. Our speculative reconstructions (via $\sigma y$’s [a]-[d]) alter the experiments but (in a delicate problem) get agreements with some among BZJ’s above false values. So maybe this or something like it was BZJ’s procedure? [Accurate bivariate least-squares result follows each observer’s reconstructed BZJ data, in brackets; plus actual minimal residual-square sum $\sigma x$, to show that most BZJ solutions do not approximate it.] Timocharis 12 stars: $E = -295, rs = -0.022, S = 2745^{2}$.$[E = -277^{\pm 18}, x = -0.076^{\pm 0.077}, \sigma x = 2441^{2}]$. Aristyllos 6 stars: $E = -258, rs = +0.003, S = 147^{2}$.$[E = -258^{\pm 10}, x = +0.001^{\pm 0.045}, \sigma x = 147^{2}]$. Hipparchos 17 stars (Alioth $\delta = 0^{m}75$): $E = -128, rs = +0.004, S = 446^{2}$.$[E = -133^{\pm 8}, x = -0.001^{\pm 0.021}, \sigma x = 392^{2}]$. Ptolemy 18 stars $E = +111$ (Brandt et al 2014B Fig.5 no-prop-mot), $rs = -0.005, S = 259^{2}$.$[E = +115^{\pm 13}, x = +0.004^{\pm 0.052}, \sigma x = 525^{2}]$.

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 (unlike BZJ) he realized that the leftover residuals after the 1st monovariate test were to be fed into the 2nd such, to find the value of $x$ which minimized the sum of the residuals’ squares. However, for $x$’s standard deviation $\sigma x$, Maeyama 1984 Table 1 column d wrongly lists $\sigma x$, the mean error of a single observation. The resultant errors range as high a factor of nearly 7 (the Hipparchan 44-star sample).
Table 1: Ancient Observers’ Epochs E, Adopted and Actual Geographical Latitudes L

<table>
<thead>
<tr>
<th>Obsrvr</th>
<th>E ± σE</th>
<th>Adop L</th>
<th>Its Error x</th>
<th>Actual L ± σL</th>
<th>σδ</th>
<th>στ</th>
</tr>
</thead>
<tbody>
<tr>
<td>Timoch</td>
<td>−302±08'</td>
<td>31°12'</td>
<td>+1°5.1±1.9</td>
<td>31°10.5±1.9</td>
<td>±6'1</td>
<td>±5'S</td>
</tr>
<tr>
<td>Aristyll</td>
<td>−258±10'</td>
<td>31°15'</td>
<td>+0°2.0±2.7</td>
<td>31°14.0±2.7</td>
<td>±6'1</td>
<td>±4'2</td>
</tr>
<tr>
<td>Hipp</td>
<td>−131±05'</td>
<td>36°08'</td>
<td>+0°2.1±1.2</td>
<td>36°07.8±1.2</td>
<td>±5'2</td>
<td>±5'S</td>
</tr>
<tr>
<td>Anon</td>
<td>+159±09'</td>
<td>31°15'</td>
<td>+4°4.2±2.0</td>
<td>31°10.6±2.0</td>
<td>±6'0</td>
<td>±5'6</td>
</tr>
</tbody>
</table>

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

C16 In 1994, 12' later than 1982, DR discerned a new method for finding each observer’s assumed geographical altitude: from nulls in his data’s fractional-endings’ frequency-profiles (as explained in Rawlins 1994L §F) which, by subtraction of x, easily produces each observer’s absolute actual latitude L. All four least-squares-fitting E and epochs L (Timochal 11 stars; Aristyllos, 6; Hipparchos, 19; Anonymous, 12), along with their standard deviations (σE & σL), as well as single-datum standard deviation, raw (σδ) and with the effect of rounding removed (στ). All these desiderata are produced here in Table 1, slightly40 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 various41 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 disputed with JAH’S “accuracy” values, the published article did none of these things.

C17 Following such odd doings, Brandt et al 2014B performs somasultas 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 refeshued four at Fig.10) — groupings 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 exactly-equally 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’s Figs.7&8 (C et al 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 (Calculated) with C (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 mis-step in Brandt et al 2014B that one of the SickSix stars among the Unhelpful 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 atRawlins 2009S §K; 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 groups of stars different from the skeptics’ simple acceptance of Ptolemy’s own groupings. In the final paper, no correction occurred for the above-cited ref-noted fact that Aldebaran was eliminated from BZJ’s groups L&E though, again, it was moving faster in declination than non-eliminated stars: the selection of the Unhelpful Threscore was published unaltered at p.332. (The paper’s last version of grouping [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”) — which we are now that 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.

40 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/iff.htm#bvm). And Brand et al 2014B’s eq.1 (p.331) is founded upon a confusion of errors with residuals (possibly just a misunderstanding of the Rawlins 1982 ms’ eq.2), thereby equating Observed-minus-Calculated with what is actually just Calculated. If taken seriously, this makes Observed equal to twice Calculated!
Clean Dozen, eliminating ever-problematic\textsuperscript{44} Arcturus — leaving a consistent set we might as well call the “SickFive” — the resulting (unweighted) residuals are mostly about 1/2 degree, the smallest\textsuperscript{45} being 17′. No overlap at all. A lovely split. So there’s just no need\textsuperscript{46} to get fancy over dividing the “Ptolemy” 18 stars. Unless one is extremely, extremely determined to undermine acceptance of R.Newtonian skepticism about Ptolemy — by any sleight necessary.

C18 For finding epoch $E$, Brandt et al\textsuperscript{2014B} adheres to depending on each star’s “crossing time” (the year when its residual is zero) & “slope” (rate of change of declination/year). Thanks of some interest and utility as rough checks (on better procedures), these approaches are sub-prime (especially when compared to standard approaches — which are probably avoided by Brandt et al 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), to another section.

C19 During their $E$-search analyses’ odd-option dependence on crossing-times (instead of obviously-preferable measure by residuals: reminiscent of www.dio.org/lff.htm#twsa), Brandt et al\textsuperscript{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 $\S$17’s L-vs-E regroupings that replaced Ptolemy’s simple split) which tests for clusterings in stars’ crossing-times. This is a patently poor basis for eliciting anything valuable, for the obvious reason that the crossing-times’ reliability are highly disparate ($\S$19), due to slopes that vary from nearly the full possibility (0.3338/yr) to virtually zero — the latter producing nearly valueless crossing-times, which lead to exclusions and inclusions based on virtually random happenstance. The cluster-analysis deletes (p.335) Castor, Altair, Betelgeux, & Sirius (not consistent with earlier deletions [p.332] of Aldebaran, Betelgeux, & Sirius [\S$\S$19]), yet in both cases, stars with slopes weaker than some of these are retained. (See, e.g., fn 42 above.) E.g., why does the clustering section of the paper eject Castor but keep Pollux, whose slope is smaller? — probably because their mutually wan slopes (nearly horizontal) in Fig.7 magnify a tiny difference (just a few arcmin: less than $\sigma_o$!) 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\textsuperscript{2014B} repeatedly ignores (fn 45 above) the request, by the DIO referee report, www.dio.org/jauq.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′ difference, which matters, as we saw. Brandt et al informs us in Appendix C19 that the misrendering of 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 the Clean-Dozen-vs-SickSix split (by contrast to any of the many other Anonymous epochs that were flirted-with in the article or the referee report), and in doing so implicitly jettisons as needless (fn 46) Brandt et al\textsuperscript{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′ ago, by R.Newton & D.Rawlins. The 2 papers historical journals (Centaurus & JAHH) have published on the issue since have just messed it up, while discovering nothing new that’s valid. Indeed, as seen from $\S$3 Summary’s conclusion (p.47), R.Newton would judge these efforts primarily “subtractions from the sum of human knowledge”. Which anyone could’ve foreseen by noticing that both papers inexplicably got deeply involved in graphical solutions by trial&error, the latter paper soberly treating the former as the prime prior research!

C23 As a final quietus to JAHH’s monumentally stubborn 2014 adventure, we now show how easily a scrupulous journal could have checked on whether its or our solutions for $x$ were correct. All JAHH needed to do was: vary the $x$ &c of their solutions to see if their residual-squares-sum $S$ was minimal, i.e., equal to our minimum, $S_m$. (Which is the square of the appropriate $\sigma_o$ 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\textsuperscript{47} $x$ for that astronomer, and then [2] just re-run Brandt et al\textsuperscript{2014B}’s monovariate test for him. BZJ will then encounter a sorta-pleasant surprise: all four astronomers’ values of $S$ are just 0-4 arcmin. The size may be small, but the issue isn’t: the declinations issue was solved with full competence over 30′ ago, by R.Newton & D.Rawlins. The 2 papers historical journals (Centaurus & JAHH) have published on the issue since have just messed it up, while discovering nothing new that’s valid. Indeed, as seen from $\S$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!

\textsuperscript{44}To understand why Ptolemy faked his era’s Arcturus longitude to equal the exact false value he gave at Almajest 7.3, see $\S$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.

\textsuperscript{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 −81/2 (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 $\sigma_o$ (6′ vs 5′); an unattanked 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 $\S$13 on simultaneity). And, since such automatically assumes $z = 0$, we have $L = 31° 15′$ (see $\S$16 above, & Rawlins 1994L $\S$58), which is 3 nautical mi north of Alexandria’s $L = 31° 12′$, whereas the +159 bivariate solution $x = 4′$ closely reflects the +3′ error in the observer’s overlap assumed $L = 31° 14′$, and so is effectively right-on: $L = 31° 11′ \pm 2′$ (ibid Table 3). All of these neatnesses render it doubly strange that Brandt et al\textsuperscript{2014B} persistently refused to recognize DR’s discovery of +159, misprinting it (over warnings) again&again as +131. See $\S$21 below.

\textsuperscript{46} See at www.dio.org/jel01.pdf, DIO 14 §1 $\S$2, & www.dio.org/jel03.pdf, ibid $\S$3 fn 13, the parallel case of now-needless methodological theories that keep getting proposed to weakly explain the already strongly explained ancient Earth-size of Sostratos-Eratosthenes.

\textsuperscript{47}DR’s latitude-errors $x$ for Timocharis, Aristylls, & Hipparchos have for decades been available, for all check, to Rawlins 1994L pp.44-46 & Table 3 — virtually the same as in his 1982 ms, to which Brandt et al\textsuperscript{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.dio.org/jauq.pdf, the DIO 2014 ref report. (Due to minuscule differences in adopted star-places, the $x$ value that will produce minimal $S$ 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 §8) of L = 31°1/4 for Alexandria, which is 3° (close to 4°±2°) larger than the reality: L = 31°12′.

To go further, in order to find an integrated-probability 2σ locus in x-e space, the student might profitably consult www.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 unprincipled48 — sometimes (e.g., fn 18) even vicious — stubbornness50 of those determined to protect Ptolemy from public exposure by any means (notably in JHA’s pages) that they are not protecting anyone continue believing/or their goorooes — resorting to any spopy argument, any curation of 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 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] bellowing (§2 fn 19) their opposites as fools, knives, and nuts (before discussing evidence — if ever doing so at all), while [2] pointing59 innocent onlookers to the bemuddled, Reputable people who’ve taken their side: after all (as we ask at above p.87, in the Text—For-The-Day intro to this article), how could such cynosure seem so Enormously Stupid — as they must be or act, if skeptics are right?

D4 Well, here’s exactly how: just [a] keep smearing heretics behind their backs (details & photos @ §B1 above) while continuing to [b] publish pseudo-defenses of Ptolemy’s honesty — no matter how ridiculous (§3 fn 66). Meanwhile, disallow — as too Disrespectful51

48 But, to be fair, let’s admit that it’s not just the Ptolemy-defender side that uses Dirty Tricks in combat. In those apologists’ eyes, DIO uses Dirty Tricks just as cruelly and frequently — that is, whenever we resort to outrageously outreme extremes like competent scholarship, ethical dealings, and defining Infallible Archons. (To pols, it just doesn’t get any dirtier — or extraterrestrially unfamiliar.) Don’t forget boldly-unturworthy DIO’s prime motto (www.dioi.org/mot.htm#gbcs): 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 fn 1, 3, & 5.

50 Without citing the various powerful evidential proofs that Ptolemy stole the star catalog, Schaefer 2013 p.47 instead repeatedly resorts to sociology to aver that we can’t KNOW so because herd-loyal Ptolemists (like B22) 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 confuse should yet be so difficult to convince.” Another DIO motto (DRI), which extends also to other truths: “Why does anyone continue believing/or their goorooes — resorting to any spopy argument, any curation 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 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].)

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References

J.Brandt et al 2014A. DIO-refered 2014/7/8 paper; too-soon became Brandt et al 2014B.


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