DIO

& The Journal for Hysterical Astronomy

Is a University Enhanced by a History of Science Department?

Weak Science and Data-Fudgery Yet Useful Mining of Fresh Ore Diller’s Perfect Fit Shunned 84½ History of Science Society Theft BreakfastLunch JHA Refereeing $100,000 Prize EclipsePair Hunt Alltime Clumsiest Ptolemy Fakes Unseen Aristarchos&Archimedes
Ancient Accuracy Vs History of science Society

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

Your 2015 March issue’s lead paper “The Two Earths of Eratosthenes” by C.Carman & James Evans [University of Puget Sound] Isis 106.1 pp.1-16 [advised by NYU’s A.Jones], www.dioi.org.cev.pdf, is founded totally (abstract-to-conclusion) upon the theory that, though Eratosthenes’ legendary Aswan-Alexandria experiment yields Earth-circumference C = 250000 stades for the Sun at infinite distance, it yields C = 252000 stades if parallactically adjusted for Eusebii’s finite Sun-distance of c.100 Earth-radii. But this result had already been published in uncted “Eratosthenes’ Too-Big Earth and Too-Tiny Universe”, DIO, 2008, 14 11 fn 6, www.dioi.org/jet01.pdf, explored as an alternate explanation, even though reasonable traditional theory is that, whatever its origin, C was finally adjusted to 252000 stades so that 1° = 700 stades, Eratosthenes’ standard scale (Strabo 2.5.7).


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

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

1 Effectively libelling 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, datatitting hypothesis: that the 1.4-factor error (40% overestimate) in Ptolemy’s Earth-longitudes was from just multiplicatively stretching them to shift from Eratosthenes-Almajest’s 700 stades/degree to Marinus-Ptolemy’s 500 stades/degree. Shcheglov discards the 1.4-stretch theory by claiming that his true explnation 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.

2 Curious examples of Shcheglov mischarges: [a] The simple-stretch idea is alleged (Shcheglov p.693) to bear logical fallacies; none are produced. [b] The stretch-solution is said (p.693) to claim accurate land-surveying. [c] Ignorance of alternate theories is implied (§D below). [d] I’m mis­said (p.693) to claim accurate land-surveying. [e] Accurately surveying the earth’s diameter was anything but a bad mistake (end of §F below). [f] Incorrectness of alternate theories is implied (vs DIO 6 [1] fn 47, DIO 20 [1] fn 2). [g] It’s mis-said (p.693) to claim accurate land-surveying underly Ptolemy’s longitudes. (My spare proposal was a simple longitude-multiplication, without any connexon to Shcheglov’s amazing & valubly complete reservoir of centuries of stadelength guesses.)
B Shcheglov (p.705) calls early accurate geography “a quaint illusion” — & his Abstract [captioned Isis by promising] “Ptolemy’s reputation is rehabilitated in part, and the delusion of high-accuracy ancient cartography is dispelled.” The dispelling is effected by arguing that Greeks couldn’t use eclipses for longitude, skipping all the evidence they did (Rawlins 1984 Greenwich). Shcheglov’s [Muffin fantasy] of bumbling Greeks also deflects the broad context of their high physical science as revealed by us [e.g., www.dioi.org/jl09.pdf, for decades]. Our ordmag-estimates of Greek accuracy (check out each for yourself):
1° for big cities’ geogr. latitudes L (Strabo 2.5.7; Isis 73.2 p.264; DIO 16 §3 §3[C1-C2])
1° for scientists’ L (Isis 73.2 p.263; Centaurus 27 p.280; DIO 4.1 §3 [F; JAHN 17 p.326])
0.1° for star declinations (ditto)
1% for Earth-circumn. precision (ArchiveHistExactSci 26 p.216; DIO 14 §1 §A & eq.28)
1% for Earth’s tilt or “obliquity” (Klio 27 p.266; DIO 16 §3 §3[A-B] eq.2 and Tables 1&2)
1% for lunar mean distance (Almajest 5.13-17; DIO 8 §1 [H4: 59 Earth-radius vs really 60])
10° for lunar-eclipse-prediction (DIO 1.1 §6 eq. 32)
1° for time of lunar eclipse (Greenwich 1984 in Vistas in Astronomy 28 pp.258&265)
0.1° for lunar limb vs Sun (Separation DIO 16 §1 fn 24)
ditto or even 1° for star-vs-eclipsed-Moon gap (DIO 1.3 fn 288; DIO 16 §1 §A fn 22)
1° for solstices (Bull.AAS. 17.2 p.583; DIO 20 §2 eqs.21&25&Table 3; P.Foad 267A)
for –145/3 eq/2: even Alexanda Palaearca polar-star-set ring (Isis 73.2 p.263 n.17)
10° for sidereal year (DIO 6 §1 fn 38&§7; DIO 9.1 §3 Table 2; DIO 11.1 §1 fn 14-15)
1/6 century for motion of Mars and arguably Venus (DIO 11.3 §6 fn 26)
0.1° for synodic month (DIO 6 §1 eq.2 & fn 12 & 18; DIO 11.1 §1 eqs.1-8)
1° for anomolistic month (DIO 6 §1 eq.13 & fn 12; DIO 11.1 §1 §A3 & eq.2)
0.1° for draconic month (DIO 6 §1 eqs.2&19 & fn 12; DIO 11.1 §3 eqs.1&3)

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

That ancient geographers’ longitudes were based on eclipses is doubted by Shcheglov p.690 as “too impractical”. I’ve outdoor-eye-ball-timed enough lunar eclipses to know their accuracy is ordmag 1° [anciently somewhat vitiated by sundial graduation limitations, ordmagry] agreeing (at 4/11) with the well-under-1° accuracy of pre-stretch Geography longitudes, D.Rawlins 1985, “Ancient Geodesy: Achievemen and Corruption”, Vistas in Astronomy 28:255-268; p.265 (1984 Greenwich paper). Though eclipses are common (Ptolemy experienced 3 in 3 years: 133-136 AD, Almajest 4.6), Shcheglov’s n.8 accepts

INDOOR (Rawlins op cit §10) astrolag Ptolemy’s giveaway-incredible claim (Geography 1.4.2) that few eclipse data were available. But outdoor Hipparchos (Strabo 1.1.12 or Shcheglov, n.7) says nothing for rarity or impartiality, instead recommending eclipse-comparison as the best method for scientific longitude-difference determination. Yet, revealingly, the sole eclipse-pair Ptolemy provides (Geography 1.4.2.), to illustrate this central method, is half a millennium old, the 330/90/20 “Arabia eclipse”, Ptolemy’s reported time (longitude) gap is 4/3 too big, so Shcheglov’s n.8 tries albining Ptolemy and simultaneously attacking ancient eclipse-longitude-measure by asserting that, of four other ancient eclipse-pair reports, three’s longitude differences “also give badly overestimated results” . Kleomedes 4° Spain vs Persia; Heron 2° Rome vs Alexandria; Pliny 3° Campania vs Armenia (4th pair: Pliny’s correct 2° Sicily vs Arabia).

D But Heron didn’t even try to gauge longitude-gap by eclipse (Neugebauer, History of Ancient Mathematical Astronomy, 1975, p.848). Kleomedes’ 4° gap is virtually correct since Cadiz at 25°W longitude and Persepolis at 35°22E are 35°37‘ apart. Shcheglov just mis-signed Cadiz and found 30°57‘ (comfortingly consistent with Ptolemy’s false 4/3 factor for his Carthage-Arabela gaffe-gap). As for Pliny 2.72.180, Shcheglov knows Campania & Armenia are c.2° apart. (Longitude gaps between Naples & the Geography’s Armenian cities (Diller DIO 5 Table 17; 1984) Dioskourias, Artaxata, Gaggara are 1°47’, 2°01’, 2°32‘, respectively, all indeed about 2°.). So Shcheglov concluded that Pliny’s 3° is too high. Yet Pliny’s “they can’t-campania Arabia 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, has verified Pliny, essentially adding, “Solar eclipses are, of course, without value for longitudinal determinations.”). So, ironically, both of Shcheglov’s eclipse-examples for ancient inaccuracy have backfired.

E Shcheglov’s other Pliny record is the same Arbela lunar eclipse Ptolemy mis-reports as S.P.M at Carthage, 11 P.M. at Arbela. But Pliny has the same event 6 P.M. at Sicily (west Sicily was under Carthage then), 8 P.M. at Arbela, resp, both times correct within minutes. Shcheglov n.8 doesn’t connect the two Arbela-eclipse-reports; & neither he nor any other historian-of-science has noted that “authoritative” (G) scientist Ptolemy has accidentally mis-assigned Arbela’s 8 P.M. to Carthage! A check of his probable source, Pliny loc. cit., reveals how: by grammatical accident, Pliny’s Latin sentence places 8 P.M. nearer Sicily than Arbela, while 6 P.M. is expressed as a word (moonrise) not a number. Unequal to the Latin, Ptolemy thought 8 P.M. was Carthage time. Since his 4/3-stretched map already had Arbela 3° east of Carthage (real gap 2°1/4), he faked Arbela thusly: 8 P.M. + 3 hour, 6/4 P.M. at Arbela thusly: 8 P.M. + 3 hour = 11 P.M. That is, his own map and the astronomical observation in the same paper are mutually contradictory: Ptolemy’s “most astronomer of Antiquity”? Ptolemy’s times mega-disagree with not just reality but his own tables: 2°30’! [Error about as big as quantity sought: like §3 fn 8.] Shcheglov notes no discords nor Arbela-Carthage-mixup, though all are at fn 45 of the same paper, www.dioi.org/je03.pdf, he’s uncomprehendingly consulting in his nn.12&15.

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

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

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

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

No mention that all five ‘foundations’ cities’ Geography lattitudes are seriously wrong (rms 26’ = ordmag 1°); errors –14’, –30’, –43’, –14’, –14’ (mostly quarter-degree negative, from astronomers’ amateurish use of asymmetric gnomon). Meanwhile, statistical stellar analyses by Rawlins (Isis 1982; & DIO 1994, thrice cited in “Secrets”, which Shcheglov read), Y.Maeyama (Centaurus 1984), & J.Brandt (JAHH 2014) show that all 4 real, non-astrologer Greek scientists cited above at §3 [K] knew their latitude to ordmag 1’. This twice-confirmed Rawlins discovery undoes Shcheglov’s entire inaccurate-geography thesis.

He doesn’t mention it. Nor does he mention the contradiction it obviously creates versus Geography’s mean latitude error of ordmag 1°.”

In response to the disjunct, one JHA Editorial Boredperson has offered that geographers must have ignored astronomers! (So, did astronomer-geographer Hipparchos ignore himself?) DR mathematically contends “Achievement” p.260-264 (these hitherto-unexplained errors were instead from forced latitude-uniformization-herdings, for astrologers’ convenient access to tables at each key latitude or “clima” (for horoscopes’ Ascendant and other “house” boundaries: “Secrets”, eqs.2-3), corruption inconsistent with the astronomer-scientist Shcheglov sees Ptolemy as.

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They are 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 doctoring lattitudes by lumping them into discrete klima-cubbyholes where all cities in a cell are force-assigned the same latitude (§K; Geography 1.4.2; “Achievement” p.261; “Secrets” §D) for handy astrologer-access to inevitably-too-widely-spaced klimata tables: Almajest 2.6. (Three centuries later, professional astrologer Ptolemy ruined lattitudes systematically, stretching them by factor 4/3 or 7/5. Summary: §3 [II.1].)

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 lattitudes, so providing the 1st (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 superstitious astrologers’ implication byastronomers that Ptolemy’s occultism is his. Ironically, this well-intentioned discipline has long blinded the field to the obvious: just as his Tetrabiblos was his religion’s handbook for horoscope interpretation, his Almajest and Geography were also world astrologer-handbooks. (The 1st fully competent translations of Almajest and Geography called each a “handbook”. Ptolemy’s exact title of what most now call the Geography was actually Geographical Directory, as DIO routinely calls it.)

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

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 2560000 stades (later nudged to 252000, perhaps for 700 stades/degree-convenience).

[ii] Eusebius’ Sun-distance, 4080000 stades, is thus 100+ (Earth-radii), in the Aristarchos-
Archimedes-Hipparchos-Poseidonios tradition that too-big-for-precision Sun-distance is a power of 10: their 1000r or 10000r, likely origin of the very idea of order-of-magnitude. [iii] By the correct (now generally-accepted, but still sniped-at) 185 meter stade, 256000 stades is 19% high, near 6/5 of real C; Poseidonios’ & Geography’s C, 180000 stades, is 5/6 low. All the three Rawlins papers which Isis’ authors have profitlessly consulted explicitly stress that air’s bending of horizontal light renders high by 6/5 the C gauged by lighthouse-flame-visibility, while the same air-refraction will make C obtained by timing sunsets (at different terrestrial heights) come out 5/6 low — the double-sunset method. See D.Rawlins, “Doubling your sunsets or how anyone can measure the earth’s size with wristwatch and metrestick,” Am.J.Physics, 1979, 47:2:126-128, p.127. Cited to discover for Rawlins for years in the 1990s in the well-known textbook, Halliday, Resnick, & Walker, Fundamentals of Physics, as its kickoff example of applied science, illustrated by diagram (plus frontispiece sunset-photo). See also J.Gerver and Rawlins in Scientific American 1979 May. But uniformly silent Historians-of-science will not so much as admit the existence of the airbend solution, not even when they have provably read it (e.g., J.Dutka at AHES 46 p.64, 1993; F.Ragep, Archimedes 23 p.124, 2010; Isis 2015 & again in 2016). But, as we’ve seen already, Isis & Shcheglov in 2016 exceed their predecessors, by showing that all the above-noted scientists & forums are Deluded in finding precise ancient geodesy credible. [Above paragraph’s conclusion added 2017/5/24&6/22.]

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

Though C = 256000 stades is unignorably prominent in all Rawlins material Isis cites, none of the authors ever mentions that number or DIO’s 1982 discovery of it, nor do any mention that it’s 27 times Eusebius’ Eratosthenis implicit Earth-radius r = 40800 stades: §W [ii], above, finally realized 26 years later in 2008’s DIO 14 §11 eq.11.

None notes DIO’s refraction solution (6/5, 5/6) without noting that its physics [13 §126]. Its triple-consistency (to 1%) with both 19% disparate C (Eratosthenes-Almajez vs Poseidonios-Geography) AND the 185 meter stade (above), should be known, so that scholars can make up their own minds if it should at last mercifully end-the-endless, the ancient-Earth-circumference debate. If so, the vast literature Shcheglov has unexceedably compiled chronicles 2 centuries of pursuit of a METROLOGICAL-solution chimera, while the ultimately-accepted solution should turn out to be not a complex spliced “chain” (p.705) of stade-juggling ad-hocery, but one natural (zero stade-maneuvering) unifying PHYSICAL theory: refraction by air.

Following the December paper, may we propose a 2017 New Year’s Resolution to end the tradition of publishing papers promoting Ptolemy as a scientist by simply omitting all the massive evidence he wasn’t? Persisting in doing so can only degrade our discipline. POSTSCRIPT [Below items originally “Not for publication” but: why shield shunners?]: At least six Experts (p.689) vetted Shcheglov. Besides the foregoing weightier problems, how’s the following mostly-minor but mostly-obvious slips elude 6putative readers?

n.d line 1: Xi’an & Luoyang are located in China not Montana (wrong hemisphere again). p.693 line 2: Eratosthenes’ Earth-circumference is not 25 miles. n.19: Engels’ central Am.J.Philol. vol.106 1985 article is pp.298-311 (as in our §3 fn 110). p.703: Publication date of Piny’s 77 AD Natural History is confused with his deathdate. p.689: Best Isles’ location matches Cape Verde Islands, not the 800-MILE-DISTANT Canary Islands (one called “Kerne” at Geography, 4.6.3r). My mistake: first mentioned by all four centuries since Ptolemy called one Blessed Isle “Kanaria”. (Language over coordinates?) Try a modern map vs Geography 4.6.34; or 2008’s DIO 14 §3 §F, which Shcheglov read. DIO’s Cape Verde Islands discovery just might be cited somewhere, sometime, in Isis. What does it say of tradition’s grip on the field that this simple fact wasn’t noted before?

Afterword: The History of the science Society Hunkers and Bunkers

Due to cultish historians-of-astrology, Greeks’ patient-won accuracy is unknown. (Perverse-ironically: it’s widely believed that semi-literate Mesoamericans were better!) Given Editor H.F.Cohen’s haughty rebutt (p.4.5, a Letter-to-the-Editor [pp.3-8 here], with cover letter www.dioi.org/isa.pdf, was sent 2017/3/20 to the 30-person Isis Editorial Board; separate emails to ordmag 10 board-members (requesting all 30 be informed of the letter), including Maria Portuondo (astro), head of Johns Hopkins University’s History of science Department, plus a message left on her answering machine 2017/6/11. No response. (Asked later to review these doings, her JHU colleague R.Kargon [history of physics] ploed too “rusty”: 2017/9/5.) Having heard from neither Editor nor Board, DR wrote the latter 2017/4/1, www.dioi.org/issb.pdf, hoping (ehm in original) to encourage communication while correcting [Isis 107.4’s] unfortunate December misinformation, unwar Isis publication of which might have been avoided, had Cohen possessed the humility to recognize he didn’t understand Shcheglov’s [2016 December Isis] paper except that it enticingly attacked one who was upsetting* Cohen by asking Isis to publish too-accurate criticisms of his fellow pols. For Shcheglov, Cohen should’ve sought refereeing from not just the usual suspects but from DIO (re, after all, a huge attack on DIO&DR) during a period when Cohen was actually exchanging emails with DIO, but preferred secrecy. Now, instead of owning to errors, he’s covering up for not just Ptolemy but for Cohen, taking you all into hiding with him. The Isis board’s non-reply so far risks being interpreted as . . . doing nothing — about mere plagiarism, and miscomputed demeaning of accurate and scientifically refereed [p.45 below] Greenwich-Centenary scholarship.

Less speculatively, we know exactly what Cohen was up to, when he did not tell us he was sending the large paper [www.dioi.org/qio.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? Trustseeking 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 (Villas-Bablitch-Kelch Distinguished Achievement Professor) of the Univ.Wisconsin History of science Department, reading (in its entirety): I received your note and have read the attachments. In my view, the decision of what to publish (or not) in any specific case is the prerogative of the editor. So I’m afraid I cannot help you out here. Sincerely yours, Lynn Nyhart

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

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*Cohen email to DIO 2016/9/27: “Never ever is Isis going to publish a paper which already in its 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 Ptolemites & Gingerich’s various past slanderous referee reports upon our work, naturally requesting removal of any threatening to his clique, even offering to take another look at the paper (fn 35: “If . . . DR revises . . . I would be happy to look it over.”) to confirm that the censorship he was ordering had been satisfactorily carried out. So our 2015/9/30 resubmission added extensive notes, responding to such typical intrusion by reviewing — at least for JAHH’s info — the long, revolving history of such stifling of open discourse, but giving JAHH permission to delete these or anything else it thought inappropriate, with our encouragement at the prospect of such helpful assistance. In reaction, JAHH has followed the Journal of the History of Astronomy in permanently severing communication with DIO: suggesting, as later confirmed, www.dioi.org/oww3l.pdf, that JAHH’s initial request for cuts was made in vain hopes (encouraged by years of Gingerich-circle slander of DR) that DIO would adamantly refuse revision or cuts, thus killing the paper without JAHH being indictable for censorship. DIO instead agreeably refused to fall into that trap; thus, fleeing was JAHH’s only escape-option to effect pre-ordained rejection. The 2015/9/30 version follows, very slightly enhanced.]

ABSTRACT

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

A INCURABLE DENIAL OF THE UNDENIABLE

Claudius Ptolemy’s Almagest is the central document (§1 below) of our valued heritage from classical antiquity’s mathematical astronomy. Though Princetitute’s Neugebauer 1957 (p.191) has called it “one of the greatest masterpieces of scientific analysis ever written” the present paper will prove that in truth Ptolemy was not a scientist, but a mathematician who faked science. In an astrology-saturated era, he worked for the occultist Serapic state religion at Canopus (near Alexandria) where stood a major temple, which specialized in curing illnesses through astrology&dreams. Ptolemy also authored astronomy’s bible, the Tetrabiblos. His employers no doubt appreciated his consistent demonstrations that reality was in perfect accord (§2) with divine celestial theories. However, for centuries, historically savvy astronomers have known that this famous 2nd century AD astrologer-geocentrist mathematician accomplished said illusion by indoor-faking or plagiarizing all his allegedly-outdoor “observations” of celestial phenomena, to force precise accord with indoor mathematical models (some of which had already existed for centuries) and tables computed therefrom. Ptolemy’s deprivations even included stealing and mis-prescribing Hipparchos’ immortal 128 BC 1025-star catalog (R.Newton 1977 pp.239-242), a deed which for over a millennium plundered 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) archontal historians-of-astronomy keep intermittently trying to breathe life back into their longstanding tradition — e.g., Neugebauer 1975 p.284 & Pedersen 1974 p.258 — of imagining 1 (§& see Neugebauer 1975 pp.69, 119, 205; J.Evans, 1987 to-
date; A. Jones 2010A 2 pxi) that Ptolemy was actually an honest outdoor astronomer (even [below, at §N3] rating Ptolemy a better observer than Hipparchos!); or at least (Schaefer 2013 p.47) that there is still a serious question about whether he observed outdoors: classic the-controversy-continues resort (latest example: fn 1), ever dear to those fearing faces-loss in a prominent controversy. Even after a 50+ avalanche of discoveries consistently (fn 12) contradicting his position, Earth’s most loyal Ptolemist responded to Thurston 2002S p.69’s prominent challenge by unequivocally verbatiem-reaffirming (Gingerich 2002 p.70) his original 40+-ago verbatiem echo (Gingerich 1976) of the once-unoppositely-dominant mantra ([\$M2]) that Ptolemy was “the greatest astronomer of antiquity” (Neugebauer 1975 p.931).

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

B1 In the history-of-astronomy community, for nearly half a century, the issue of Ptolemy’s honesty, originality, and competence has been a trigger for blackballing (Rawlins 1991W §B), shunning (Gingerich 1990; Schaefer 2002 p.40), censorship (Rawlins 1996C p.4, DIO 8 p.2), and intimidation (idem fn 1). E.g., the field-central and highly esteemed Journal for the History of Astronomy (JHA) may be just a minor step of perfection in its some don’t empathize with the laughter a genuine scientist can hardly stifle, upon carefully examining the goal-directed imitation-science that is (not merely charged here but is) repeatedly proven in detail, throughout the paper, to be what has been consistently and exclusively used for decades to do what a once-ruling cult wanted done: make a case — any case ([\$H2 & \$3 fn 66] — that Ptolemy was an honest, outdoor-observing astronomer. The referee imagines that the bad-old-days still apply, when Ptolemites could keep doubters from being taken seriously just by portraying them as those who had committed the crimes of being losers, and [\$] of merely disloyal. June HJA 1982A p.87 (on the paper which became Rawlins 1998C [sent to DR 1978/4/30], as submitted to the R.Newton-hating JHA, read instead [emph added] “Beyond the shade of a doubt” this renders probable that the [\$Almagest star catalog] longitudines] were plagiarized from Hipparchos. But the original version of the paper [sent to DR 1978/4/30], as submitted to the R.Newton-hating JHA, read instead [emph added] “Beyond the shade of a doubt” this renders probable that the [\$Almagest star catalog] longitudines] were plagiarized from Hipparchos. The referee’s judgment is nearly an exact repeat of frightened Astronomer Royal Geo.Airy’s equally revealing bowdlerization of “shadow” from his 1846/779 letter to Neptune-affair co-conspirator J.Challis: see Rawlins 1992W [J2 & Rawlins 1999N \$H2]. And for more unintended quasi-humor, the referee’s judgment — 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 wannabe-numerate historians-of-science who have repeatedly (\$B2) labelled physicists R.Newton and Rawlins as dishonest crazy incompetents and who have (fn 5) for years duded debating Rawlins (compare to fn 6, below), though having the courage to serially portray his work in the most negative light to the extent of over 100 pages (1987-2008) in the Journal for the History of Astronomy, from which he has been banned ([\$B1] for the last 1/3 of a century, during which time virtually none of Newton’s or DIO’s dozens of positive contributions to knowledge (e.g., R.Newton 1977 & R.Newton 1982, www.dioi.org/vin.htm) have been credited there, a blank entirely (\$2) open for the history of science, e.g., here at §N7 and fn 14. Not to mention rocks at §L2 and Pluto at fn 42. Non-appreciation of these gems and the like fully justifies Rawlins’ banishment (fn 1) from JHA. As an example of just how “impossible to deal with” he is (1970-2013 refereeing (fn 3) but is alert as can be in not permitting any author to argue in its pages that Ptolemy was a liar, even while allowing the defense clique to contend he was not (Evans 1993 p.145; Rawlins 1999 [\$P7-\$F8]). That the controversy has been rife with “unprofessional” acts, such as shunning, has been nationally published (Schaefer 2002 loc cit), followed soon after by a direct appeal (fn 35) to the head of the American Astronomical Society, urging supervision of its shamelessly shunning Historical Astronomy Division (H.A.D.), but the unprofessionalism of 2002 has only worsened since. History-of-ancient-astronomy’s lengthy communal monolithical and canoliolithical insistence upon defensively maintaining — by character-assassination (fn 5) & the threat of exile (idem fn 1 for dissenters — are widespread. Defying, perception-inverting (\$N), logic-doubting (\$1), holey-corpse honest—Ptolemy-myth, a multiple hered-sacred tenet, can only weaken the field’s cred. Same for launching fantastic, irrelevant, and transparently projective descriptions of bemused skeptics as “angry” (Pedersen 1974 p.23) and “hitter” (Evans 1998 p.268). For which side is provably angry, see DIO 11.1 p.2; also the case of an eminent referee and Jesus-devotee (fn 5) whom today’s numerous (idem) Ptolemy-skeptics a tiny bunch of paranoids for believing what he himself privately knows [\$] to be true. Healthy restoration

JHA Editor M.Hoskin, as quoted at Rawlins 1991W [\$B1]: he actually had the fantastic gall, in a 1980/7/30 letter to Hoskin, to suggest that the JHA was refereeing by “the swiftly-gauged, as against the substantial.” Considering the solid scholarship the JHA keeps publishing, like its unmatched Journal for the History of Astronomy’s editor M.Hoskin, as quoted at Rawlins 1991W [\$B1]: he actually had the fantastic gall, in a 1980/7/30 letter to Hoskin, to suggest that the JHA was refereeing by “the swiftly-gauged, as against the substantial.” Considering the solid scholarship the JHA keeps publishing, like its unmatched
will merely (!) require the opening of discourse and minds.

B2 Ptolemy regularly fabricated data from which he perversely claimed he derived his models’ elements, even while practicing the very opposite. Loyalist Pedersen 1974 p.23 originally disagreed but summed up the esteemed astronomer J.Delambre’s view of Ptolemy as “a scientific cheat, swindling with the very method of science and betraying the empirical character of astronomy, setting forth results computed from theory disguised as empirical data in support of this same theory.” Ibid p.258 also originally rejected the charge that Ptolemy stole from Hipparchos the thousand star-positions in the Ancient Star Catalog — even while suppressing citation of Delambre’s 1817 discovery of proof (§K1) that Ptolemy avoided, is, ethically speaking... original. And, then, it’s always stimulating to be diagnosed as insane by one who has for 50+ led a crusade to convince the world [i] that an ancient who was off his latitude by 1°34 and could not find the Sun within a degree, was a regularly observing astronomer, and [ii] that anyone who disagreed was the insane party (§B1). Equally risible: to be blown off-analysis as paranoid by one who thinks the whole universe is run by an invisible mind and who publicly calls on “our Lord Jesus Christ.” A world where sanity is insanity and vice-versa. With respect to its obvious duty to return the Ptolemy controversy to a scientific basis — above politics, smearing, shunning, religious devotion — the American Astronomical Society for a half-century hasn’t lifted a finger. But (fn 35) has generously given it to the skeptics. Soon after Gingerich had brought P.A.S.P. up to snuff on who is and isn’t reliable, Skeptical Inquirer Editor K.Frazier asked him to debate Rawlins on Ptolemy in Skhq; Gingerich (1978/2/2) explained his refusal by calling Rawlins “exceedingly paranoid” for “suggesting that a cabal has been suppressing the consideration of [R.]Newton’s work” on Ptolemy. When finding that Frazier had sent a copy of this helpful character-profile to Rawlins, Gingerich got miffed at Frazier! (1978/6/2 letter) — so exalted is his permanent mental state, assured of ontological immunity from the ethical standards of ordinary mortals, sinless by very definition. Like his image of his inherently unindictable ancient astronomer-hero. E.g., only such an elevated being could — without any sense of hypocrisy, of damage to knowledge, or of harm to the scribe — accuse someone of insubordination when he suppresses heresy as a means of equaling P.A.S.P. at the head of this note). At Gingerich’s insistence. U. Notre Dame’s 1999/7/5 debates on whether Ptolemy observed or stole the Ancient Star Catalog could not be held unless Rawlins was kept from the platform. Evidently inspired, by decades of toadily awesome H.A.D. worship, to standards of equity and consistency incomprehensible to the less godly (and strengthened by the surety with which he can count on fellow poles’ reverent silence on his secret actions, some even going so far as to supplicate for his patronage, even when they never actually publish him). Gingerich shared in a 2000 referee report to Isis on an article (Thurston 2002S) appreciative of Rawlins’ abrasive successes, typically promoted freethought-in-theory while inserting an element aimed at ensuring that heretics’ irresponsible abuse of freedom would be properly understood as what could only issue from a disordered mind: “To say that the paper should not be published will only fuel the paranoid claims of the small [fn 1] group fighting Ptolemy, who believe that a cabal of [Neugebauer]’s [sic] are precognitive [sic] that his findings are being aired in the same forum. On the other hand, he was offered another paranoid’s letter regarding said zany’s own recent book-review (emphasis added): “So far the Neugebauer camp has not been heard from. Perhaps my merely mentioning [R.]Newton in a review of Neugebauer has placed me beyond speaking terms.” (See www.dioi.org/pm1.htm.) And who was this fellow-paranoid, who imagined a closed Neugebauer “cabal”? No other than O.Gingerich! In the midst of decades of documented — ah — “inconsistent” character-assassination fantasy, Gingerich adds one more fantasy, soberly describing himself as “a practicing Christian”: 1978/2/2 to Frazier. Obviously, being religious doesn’t mend vendettas. We recall gullible or cynical saint-mythologist Cardinal John Henry Newman’s politically deft holy war on straightforwardly ethical independent idealist Chas. Kingsley; at Newman’s 1890 death, after the standard public eulogy, Cardinal Henry Edward Manning privately mourned his colleague thusly: “Poor Newman. Poor Newman. He was a great hater.” (Lytton Strachey, 1918, Manning chapter, end of part 9.)

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

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

in fact had stolen it. Pedersen 1974 p.258 added (emphasis added): “Our general impression of [Ptolemy’s] moral and intellectual integrity would be damaged beyond repair if we had to have him that he simply derived his catalogue from a previous work by Hipparchus without the slightest acknowledgement of the fact.” Yet when it later became obvious that the theft had indeed occurred, Pedersen flexibly decided that stealing stars didn’t really prove Ptolemy was dishonest, after all (Pedersen 1993 p.559). Agreeably reverting to the 1974 Pedersen, Evans 1998 p.262 says: “At stake is Ptolemy’s reputation as an astronomer; at issue are his honesty and reliability as an observer.” At stake? Well, not-receeeally — for either 1974 Pedersen or JHA Editor Evans. Each could always be counted upon to evade any 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 who those turned out to be more prescient (§M3 and fn 12) than their denigrators. Pioneer skeptic Robert Russell Newton is deceived. 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 astronomer 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 at fn 35, 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-obscure evidential items set out below will cause Swerdlow or Evans — or anyone else in their shrunken chorus of derision — to retrace their steps.

8 Demonstrating how one earns a MacArthur in certain sub-fields, the principled Dr.Swerdlow has not only called R.Newton dishonest and kook (§B2), he has additionally published a Joe-McCarthyesque incompentence-accusation against him (quoted verbatim at R.Newton 1991 §E2), claiming lots of these-putting-getting errors by Newton here-in-his-hand without giving a single valid example. (Check the revealing cavil-count context data carefully detailed at ibid fn 6.) And the Journal for the History of Astronomy (JHA) Editor — applying its proud smear-review policy — naturally approved this much-celebrated “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-obscure evidential items set out below will cause Swerdlow or Evans — or anyone else in their shrunken chorus of derision — to retrace their steps.

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The Greatest Faker of Antiquity

Venus

(D and the Venus-Made-Do-It Defense)

C1

The Greatest Astronomer of Antiquity’s sloppiness with his Venus swindles was so extreme that he inadvertently reports (Alm 10.1-2) having 1st-hand “observed” the same event — the 136 AD greatest evening elongation of Venus — on 2 different dates 37° apart (136/11/18/12/25), at 2 different positions 37° apart, and at 2 different maximum elongations from the mean sun, differing by 1°5 [nearly the Sun’s semidiameter]! 47°3/13 versus 47°16/30. Not only do these data disagree with the outdoor sky, they also disagree with Ptolemy’s own Venus tables. (Similarly for the Sun [±35] and the Moon [±33].)

C2

This is by far the funniest 14 & most astonishingly inept fake in the entire history of the oldest science. (But watch Ptolemy again/kagain try mightily to match it at [±45].)

D1

The Greatest Astronomer of Antiquity’s four solar “observations” (Alm 3.1 and 7) are on average over 50 times nearer15 to his indoor tables than to the real outdoor sky create Reasonable Doubt, Rawlins 1992V §20 responded: “Beside a range of specific evidence of plagiarism, there is the simplicity of that hypothesis’ fit to the larger evidential situation: if we merely assume that Ptolemy swiped the Catalog, virtually all of [defenders’] central purported ‘Eupraxia’ (ibid §[C22]) of the case immediately evaporate.”

[Bolomists are hereby asked to specify any that don’t.] 14

However, O.Gingerich (2000 Summer referee report on Thurston 2002S) regards the Venus double-dating as a testament to Ptolemy’s “great ingenuity in tackling an otherwise essentially insoluble problem,” admittedly fabrication but innocent since (Gingerich 2002 p.72) there was no other way to solve for the orbit: Venus-made-do-it. At D.Judge’s instigation, three non-fraudulent solutions (by himself, D.Rawlins, and H.Thurston) to “insoluble” Venus swiftly appeared in DIO 11.3 (Duke 2002B, Rawlins 2002V, Thurston 2002V) with the comment (Rawlins, op cit §7) that Swerdlow’s and Gingerich’s straightforward “alibiing of Ptolemy’s Venus fumblefores is akin to a defense-lawyer going into court to defend a counterfeiter who was so stupidly careless that he accidentally printed Ben Franklin on both sides of his attempts at faking hundred-dollar bills. [But would even a lawyer]…try to excuse such inept criminality by claiming that the bungled bucks showed immortal, greatest-technician-of-the-era BRILLIANCE?” Swerdlow 1989 p.36 fallaciously explains-away (to his own, the MacArthur Foundation’s, and the referee-challenged [fn 3] Fallacy 1992V §3’s general Miles Gloriosus as philosopher: Stand Aside. I Take Laaarge Logical Steps.

E5

… Forum’s general Miles Gloriosus as philosopher: Stand Aside. I Take Laaarge Logical Steps.

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

14 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 —
errors displayed at Thurston 1998A Table 1): the mean of the error-sinusoid that best fits the tables, and thus very closely fits the “observations,” exceeds a full degree: ~65°. (See formula at ibid §1.) This, though naked-eye solar measures can be made to ordmate I’ (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 or (§B2) he has broken the law of empirical science by pretending to base theory on outdoor celestial “observations” actually computed, indoors on the sky, from or to fit the very same theory (Thurston 1994P §D & Rawlins 2002V §§B3-B5), such fakes then used to “prove” said theory, a (literally) preposterous sham-process as Delambre saw: §B2.

D2 To appreciate the grossness of the illegality here, consider its sheer enormity (as emphatically noted, and planetary contexts throughout R.Newton 1977): for his 3 equinoxes, Ptolemy is alleging 1°-hand visual sightings of the Sun’s center on the celestial Equator at times agreeing to ordmate I’ 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°.)

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 ~23°/0/cy. (A rate consistently bi-miscomputed at CalTech’s Swerdlow 2010 p.152, item 3.) So: what is Ptolemy’s Sun correct? Obviously that epoch must be 137.547 + (100°/cy)65°/23°/0/cy = −145, which (as seen at Rawlins 1991W §M6; similarly below at §D5) is Hipparchos’ era, and is indeed the regnal epoch (Ptolemy VII Phmoson 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 Hipparchian PH solar model’s big 0°.4-amplitude annual periodic error. (Again: see error formula for Ptolemy’s Sun at Thurston 1998A §1.) For Ptolemy’s mimicry of not just systematic but even random Hipparchian error, see the glaring case of Arcturus: fn 37.

D5 This unsubtle echo connects to the irony that Ptolemy didn’t fake the solar data via tables but by even cruder means. (He fabricated similarly elsewhere as well: Venus [§C1; Rawlins 1991W fn 166], the stars [fn 37], and the Arbelaa eclipse [§G3].) As early as 1639 — the year the Ptolemy controversy should have ended (or been doomed to never begin?) — Christian Severin said Ptolemy had merely computed the alleged solar observations from Hipparchan data, and Delambre 1819 (pp.lxvij-lxix) explained in detail how Ptolemy had fabricated his solstice and equinoxes by merely adding integral numbers of Hipparchan years to Hipparchos’ observations of solstice and equinoxes. (Discussion: Thurston 1994P p.58; and Thurston 1998A §§A.K.S.) For example (by method of ibid §2, reconstructing individual 139 AD Autumn Equinox): to recover Ptolemy’s 132 AD Autumn Equinox (Alm 3.7), just add 278 Hipparchan years (~365°14/1~1/300 each, 6° longer than has been claimed, e.g., in The Accuaitant (§M). And in Hipparchos’ case, a ratio something above unity is to be expected since the data tables were, after all, based upon his slightly flawed observations, of which he [contra same delusional Accuaitant] reports several discordant with theory & each other: [fn 3 fn 8.

15 Ptolemy didn’t have to look skyward to see theoretical data instead of real: his Optike takes perfectly false-theory-accordant refraction angles that are erroneous by up to 2°1/2 or 150 arcmin. (See, e.g., Neugebauer 1975 pp.895-896.)

16 Go to New York Times Science’s 2009/9/8 exam of a century of establishment promotion of another scientifically unverified myth, a study in ceremonial immunity to oncoming evidence, analysed in the context of other DIO-shunning bad-loser cults. Previous day’s online edition: http://tineenlab.blogs.nytimes.com/2009/09/07/who-was-first-at-the-north-pole/. (Ptolemy’s all-time record-success at hoax-longevity is noted only in the 9/8 version.) The honest exception to the pattern described is Gerald Toomer who, though previously much-committed to belief that Ptolemy didn’t take the Catalog from Hipparchos, immediately changed his mind on seeing Graßhoff’s analyses. From chats with Ptolemists over many years, the following alibis are recalled: [1] Ptolemy’s ~65° solar error has been speculated as due to his having constructed his system before most or all of his solar “observations” (pretty dumb, since the system depended on the Sun: §D3), so he was stuck with the error and decided [see Ragep at §3 fn 9] not to re-do his whole scheme. (How is this a defense against a charge of knowing pretense? And plagiarism, since [§D3] the system’s error is just that of Hipparchos’ solar tables, 2/3 centuries later.) [2] The adherence of “observations” to theory (Sun, Venus, etc) is explained as due not to fraud but to mere fudging or “adjusting” of real presumed data, though whatever positions the “observations” were being fudged to agree with were SECRET indoor calculations, so either way it’s fraud. [3] The embarrassment that Ptolemy’s tabular mean Sun was correct only for Hipparchos’ time has been explained by presuming that Ptolemy thought Hipparchos’ observations were better than his own putative indoor observations, so (§D1) he instead reported Hipparchos-accurate data. (Was it honest to commit this appropriation? — and without saying so, which makes it a theft.) [4] See also rocks and asymmetrically unclear air at [L1 fn 42, respectively. Would that the energy and ingenuity expended upon these 4 joke-fantasies had instead been applied to open-minded, undirected, exploratory research.

19 Rawlins noticed this unsuitable point (that no regular celestial observer could be 1°/4 off in his adopted geographical latitude L and not realize so) immediately upon entering the controversy. Thurston 1994P noticed something just as glaring back in the 1940s. How could Ptolemy-specialists,
Moreover, such an error (see math of Alm 5.12 and 13, or Rawlins 1994L eq.1) would carry exactly into the “Clean Dozen” unfudged and unused Ptolemy-contemporary stellar declarations δ reported at Alm 7.3 (the only honest Almajest star data from Ptolemy’s era). These, however, both Rawlins 1994L §F9 that the observer’s error20 in assumed geographical latitude L was +4°±2’, about 9 standard deviations distant from Ptolemy’s −|4°. (Similar disconnect for the 1025 celestial latitudes β of the Alm 7.5-8.1 star catalog.) So the δ were plagiarized from a contemporary20 anonymous observer who knew his L.

E3 Confirmation is achieved via statistical induction (Rawlins 1994L §F8) of the exact latitude L = 31°1/4 as assumed by the observer of Alm 7.3’s Clean Dozen stars — that is, 17’ higher than Ptolemy’s stated latitude of 30°58’ (§E1): a hard conflict which alone shows that he had nothing to do with the Clean Dozen. Ptolemy’s provisional L for Hipparchos-Strabo (Strabo 2.5:39; Neugebauer 1975 p.1313; Diller 1984 fn 23; Rawlins 2009S fn 16). Question: Since his Alm had already (§E1) put Alexandria instead at 30°58’ (consistent with GD 4.5.9’s S’ rounded 31° value) why didn’t an Alexandrian & allegedly-outdoor observer notice he’d thus inadvertently stretched by ordmag TEN the 7-stade-long embankment connecting Alexandria to its Pharos, an embankment explicitly named Heptastadion (πέντεσταδίων): Strabo 17.1.6)?

E5 So by carelessly20 copying disparate data L 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 diprototype serial-embarrassment, by recognizing the obvious right away and promptly moving on to careers of open-ended inquiry instead of sterile (fn 12; Rawlins 2009E fn 7), quasi-theological apologetics, with sacred-cowclusion-set-in-advance. In cement. The answer is revealed by another question: which route has been exclusively rewarded (fn 28) by the JHAD, throughout The Controversy, and which has been burned down with pseudo-science (fn 8), & slandered with fantasies (fn 35)? Excluding no stars when bivariate-least-squaresing the Clean Dozen produces the L-error cited, leading ultimately (carefully trace Rawlins 1994L §§F3-F9) to the observer’s L = 31°11’±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 (160/714). Previously unnoticed vastness of Alm 7.3’s leapfrog anachronism: the Clean Dozen δ were obtained at 160 AD (as just shown) and then merged with the Clean Dozen’s 0.5 which were precess-faked so inadequately for stated epoch 137 AD that their coherent 5 stars’ mean is (fn 37) instead correct for over 100 nmi before 160 AD! There are two unknowns when analyzing 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-outlier results for epochs E yet for all 4 cases messes up the other unknown, x, the error in the observer’s assumed latitude. For the 4 observers, the figures given (op cit p.331) for the latitudes’ “accuracy” (which the authors compute instead of x) are: Timocharis 0.72, Aristyllos 0.18, Hipparchos 0.24, Ptolemy (?) 0.3 — values which are oversmall by an ordmag. Likely-wasted-at-present wakeup to the history-of-science community: outside of DIO (§3 Table 2, or Rawlins 1994L §F9 & Table 3) no paper on the Alm 7.3 declinations has ever correctly understood how to find both E and x [& associated standard deviations]. The 2014 paper also errs in dropping near-solstitial stars for being weak indicators of epoch — forgetting that they are superior indicators of x, and thus matter in gauging the accuracy of the 4 men’s adopted latitudes for their observatories.

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

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

23 In light of such sloppy-copy, one can only admire Dennis Duke’s witty new translation of the Almajest’s Greek title, Syntaxis (§3 fn 15), as: Cut&Paste. Not in Liddell-Scott-Jones, Yet.
foreign eclipse times couldn’t have supported the longitudinally-stretched geography ([G4] he borrowed (with credit)\textsuperscript{25} from Marinios of Tyre. Instead, Ptolemy’s vast opus provides (GD 1.4.2) but one\textsuperscript{26} example: two longitudinally much-separated reports — 500\textsuperscript{2} old! — of the famous Arbela — 330/9/20 lunar eclipse’s start, saying it was seen there at 23\textdegree\textit{a} and in Carthage at 20\textdegree\textit{a}, thus proving that the 2 places are 3\textdegree\textit{a} or 45\textdegree\textit{a} apart in longitude.\textsuperscript{27}

\textbf{G3} However, Pliny earlier reported the same data very differently: 20\textdegree\textit{b} (8 PM) for Arbela (modern embattled oil-city Irbil) & 18\textdegree\textit{b} (6 PM) for Sicily, whose west end (big city Lilybaeum) was part of Carthage’s empire, & of longitude anciently known to be similar to Carthage’s: www.dioi.org/500.pdf cities D67&D131. Modern calculations\textsuperscript{28} show that non-astronomer Pliny was quite accurate, while The Greatest Astronomer of Antiquity was amazingly wrong, over 2\textdegree\textit{f} off for Carthage, 3\textdegree\textit{f} off for Arbela. The former error nearly equals the entire actual 2\textdegree\textit{f}/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 \textit{own} lunar tables put the eclipse just about as much in disagreement with his reported times as modern tables do: 2\textdegree Carthage and 3\textdegree Arbela. How explain such an entertainingly disastrous fabrication? Start by consulting Pliny 2.72.180 on the – 330/9/20 lunar eclipse. Reading the passage carefully, one sees that no numerical hour is given explicitly for the western apparition in Sicily, merely: moonrise (”exorians’). By contrast, the Arbela time is given as the “2\textdegree\textit{h}r” after sunset, or about 20\textdegree, which is the very time Ptolemy gives for the Carthage report. Why? Well, look carefully at the Pliny passage cited: by a fluke of grammar, “secunda hora” appears nearer in the sentence to “Sicilia” than to “Arbelam”. This obviously suggests Ptolemy used Pliny or his source but (evidently unable to read Latin well) took Pliny’s 20\textdegree\textit{h}r to mean Carthage’s time.\textsuperscript{29}

\textbf{G4} But how did Ptolemy arrive at 23\textdegree\textit{h} for Arbela? Since Glossell 1790, it has been obvious that multiplication by an expansion factor (Diller 1984 SC5) had been applied by Marinios 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\textdegree\textit{f}/4 east of Carthage, then expansion by 4/3 would produce 3\textdegree, 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.\textsuperscript{30}

\textbf{G5} He simply added this 3\textdegree\textit{a} to 20\textdegree, thus arriving at his fantastic 23\textdegree\textit{h}r time for Arbela.\textsuperscript{31}

\textbf{NB:} This solution adds powerful new evidence favoring the theory (still-foolishly-doubted: §G4; §1 §B; Rawlins 2008Q §J & Rawlins 2008S fnm 13&45) that GD falsely corrupted an accurate prior map by expanding its longitudes by a factor of 30\%—40\%. Collecting §§C&K with the present case, we now have 3 separate Ptolemy double-false-fakes on display here.\textsuperscript{32}

\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 eclipic ring off by his notorious — 1\textdegree\textit{h} mean longitude error, the real and instrumental eclipses would be tilted by 1\textdegree\textit{/2} vis-à-vis each other (since the instrument
\footnotesize{\textsuperscript{25}Was Marinios cited partly because (unlike Ancient Star Cataloger Hipparchos) he was still alive to complain if uncredited? This question casts fresh light on the contended issue of whether the GD was out-of-date when completed. See Rawlins 2008S (§K) for further evidence that it wasn’t.}
\textsuperscript{26}Due to modern computational isolation from actual scientists, we find Neugebauer 1975 (pp.367, 667, 938) cornered into interpreting Ptolemy’s non-use of contemporaneous eclipses as having to mean that (www.dioi.org/cot.htm#ckn1) there then existed no empirical scientific community to be isolated from!}
\textsuperscript{27}The Battle of Arbela was fought at nearby Gagamella and 11\textdegree\textit{a} after the eclipse. We find actual Local Apparent Times of the – 330/9/20 eclipse’s umbral start: Carthage 17:43, Lilybaeum 17:52, Gagamella 19:56, Arbela 19:58. So the Gagamella-Lilybaeum difference in geographical longitude \textit{E} is \textit{E} = 2\textdegree\textit{0}40’; Gagamella-Carthage, 2\textdegree\textit{h}13’.
\textsuperscript{28}Rewards handed out to those who attacked the R.Newton satiric book JHA boardship (R.Newton 1991 fn 2) and a MacArthur for miss-man Swerdlov. (It’s hard to find good help anymore.) Among other examples: maid-men Evans and Schaefer were elevated at JHA not long after their massive burnings. 1998 and 2001-2005 attacks on Rawlins on account of Ptolemy authors here may actually be deliberate.}
\textsuperscript{29}Selecting boardmembers by such criteria will damage mean-IQ atop JHA for decades to come.}
\textsuperscript{30}D.Duke’s statistical studies indicate that very nearly all stars were appropriated. If Evans and Schaefer were right that Ptolemy observed a substantial section of the Catalog, then the error-correlation dot-diagrams of Graßhoff 1990 would exhibit an obviously disjunct mix of superposed shapes: circular (stars observed afresh) and elliptical (stars copied from Hipparchos). But the diagrams are instead just elliptical. (Even if otherwise, this would prove only that someone other than Hipparchos — not necessarily Ptolemy — observed the stars whose dots mapped circularly.)

rotates about the equatorial not ecliptic pole: as we can see from, e.g., the educational paper model Evans has helpfully disseminated, so (Rawlins 1982C p.361 & Fig.2) we’d find error waves of amplitude 1\textdegree\textit{2} in the Catalog’s latitudes \textit{β} (cosine waves: \textit{ibid} eq.4) and northern longitudes \textit{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 Ptolemist G.Toomer — as well as the perceptions of Duke 2002C, all of which combine to show that, e.g., errors in Hipparchos’ stars are statistically quite discernable in the Alm’s, including a few ultra-giveaway cases where a star with an error of several degrees is found to have the same sized error, with the same sign, for both Hipparchos and Ptolemy.\textsuperscript{33}

\textbf{H2} If this became obvious c.1990, even to the most religious, that many Ptolemy stars were Hipparchos? However, no archon was ever going to admit in print the plain truth: the establishment had been blindsided by proof that its challengers had been right all along — that Graßhoff’s test had now unexpectedly surprise-vindicated the long-loathed Tycho-Newton Rawlins position that the Catalog was stolen. (The post-disaster spin of some was that the three pioneer skeptics had just regrettably not proven their case — that Graßhoff’s test had now unexpectedly surprise-vindicated the long-loathed Tycho-Newton Rawlins position that the Catalog was stolen. (The post-disaster spin of some was that the three pioneer skeptics had just regrettably not proven their case)

\textsuperscript{33}This question casts fresh light on the contended issue of whether the Catalog was stolen. (The post-Graßhoff era has been especially fertile for indiscriminate fallback appeals of Ptolemy’s (selectively) malleable modern choir, as the politically ambitious realized that the JHA would ever so gracefully publish anything that muddied the clear evidentational situation, in order to save archons from facing apt appreciation for decades of falsely denigrating now-vindicated scholars: simply pretend vindication either never happened or isn’t 100.00000000%. Question: is anyone empathizing with the cornered defenders’ needless pain here, caused by artificial extension of the Controversy? (Puts one in mind of equally needless ongoing misery from other kinds of poverty than intellectual — mass-agony likewise of insuficient concern to those who subsidize its perpetuity, to ensure their own perpetuity in ofce.) This is so literally pathetic — Chauvin’s shade shatters his orbs in shame at what his legacy has come to. [1] Memory-hole-unapologetic for his original 1987 and 1998 arguments that Ptolemy probably outdoor-observed the whole catalog, Evans now just hopes that Graßhoff 1990 hasn’t proven that all\textsuperscript{22} 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 \textit{integrity} plainly founder upon his claim of 1\textdegree-hand observation of all\textsuperscript{1025} stars. And [argument 2] is on the logical and ethical level of defending a bank-robber by pleading
that, well: doesn’t everybody withdraw money from banks? The BSC does not claim 1st hand observation, while The Greatest Astronomer of Antiquity explicitly 30 does claim, at Alm 7.4, in lengthy detail, falsely saying he observed every visible star (§K1). Bottom lines: i) The JHA committed itself repeatedly to the proposition that the Catalog was all or mostly Ptolemy’s. ii) It isn’t. iii) But, simply from shame-factors detailed elsewhere here, our “premier” JHA-H.A.D. (JHAD) solipsistically hallucinates — like Dr.Frederick Cook or Alger His — that if we just never confess, then no one will ever know the truth: that we Experts lost what has correctly been advertised nationally by Schaefer 2002 as the hottest controversy in the field. [iv] But neutral observers increasingly and snickeringly do know — which is marking certain JHADists as losers to scholars they themselves have long been assured the world are crazy dishonest paranoid incompetent cranks. And we’re not supposed to giggle? (You begin to see why the seething losers can never admit it?)

H3 Pickering 2002A (B1 & Fig.1 points out a history-of-science-ignored ultra-simple disproof of the Catalog’s Catalog authorship (Rawlins 2000A fn 177): the 5° gap which should exist between the anticentral circles 31 of Hipparchos’ stars and Ptolemy’s stars (due to their differing latitudes) does not exist. The 2 circles are virtually identical. Controversy over.

H4 Several Sagittarius (Sgr) non-dim stars were missed by Tycho, so Evans 1987 p.168 (like Evans 1998 p.272) tries to create anticentral-circle ambiguity by stressing that these stars — well over 4° high, in what JHA Editor Evans calls “Sag” — are not in Tycho’s catalog, an argument put forth in innocence of the fact that Summer Solstitial non-darkness at the Dane’s northerly latitude \( L = 55.9 \), impeded these difficult Sgr stars’ availability. (By recording Fomalhaut, Tycho actually went down to within 2°6 of the horizon: Rawlins 1993D Table 17.)

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

I1 The Ancient Star Catalog (Alm 7.5-8.1) has an obvious excess of 0° endings and 30° endings in the latitudes \( \beta \), due to ancient Egyptian and Greek proclivity for expressing non-integers by using inverse integers: “unit fractions”. (Cause of both controversies detailed at Rawlins 1994L §B4.) But the most common ending for the longitudes \( \lambda \) is 40°.

I2 R.Newton 1977 (pp.245-254) showed statistically that this odd circumstance was simple to explain, once he’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, the false figure which Alm 7.2-3 claims stars precessed during the 2 2/3 centuries between the Catalog epochos of Hipparchos and Ptolemy, —126.278 (Rawlins 1994L fn 45) and +137.547 (§D3), respectively. From slyding each longitude \( \lambda \) by 2°40′, 0° endings became 40′; 10′ became 50′; 15′ became 55′ and were rounded to 00′; 20′ became 00′; 30′ became 10′; 40′ became 20′; 45′ became 25′ and were rounded to 20′; 50′ became 30′. (Note how the odd endings 25′ and 55′ got eliminated.) The frequencies of endings in \( \lambda \) and \( \beta \) are displayed by Rawlins 194L Tables 1 and 2, and the whole slyde&hyde process is verified via \( \chi^2 \) test (ibid §§B-C).

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

31 A northern hemisphere observer’s “antarctic circle” is the boundary of the segment of the celestial sphere which is ever-invisible to him. Ignoring refraction and extinction, said segment’s angular radius equals his geographical latitude \( L \).

I3 Most critiques of Ptolemy’s chicanery point primarily to the excess of 40′ endings (vs 00′ endings) in the Catalog longitudes \( \lambda \), but (thanks to the 00′ hand observation, while The Greatest Astronomer of Antiquity explicitly s) the most shocking frequency-contrast is elsewhere (Rawlins 1992V §C22 item [e]; Rawlins 1994L fn 5): the spectacularly greater number of 10′ endings than 30′ endings. (Before Ptolemy added 2°40′ to Hipparchos’ \( \lambda \), these were 30′ and 50′ endings, respectively.) Looking naïvely at the tabular distribution, the fact that 30′s are the least frequent endings is bizarre, since 30′ is nearly the most frequent latitude ending, as it should be. Indeed, for the latitudes \( \beta \), 30′s are roughly twice as common as 10′s, 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.

I4 Maintaining the Jekyllian pretense to being a genuine, respectable, outdoor astronomer, required the sneakiness of hyding the otherwise-glaringly-odd 55′ and 25′ endings (which Ptolemy’s addition-thievery had produced from formerly 15′ and 45′ endings), by secretly rounding them to 00′ and 20′ endings, respectively. That is, the Greatest Astronomer of Antiquity deliberately and permanently destroyed data in a legendary work, just to cover his tracks in a theft. Thus, we cannot now tell whether a Ptolemy stellar ending was 10′ or 20′ or 40′ or 45′ for Hipparchos.

So R.Newton (§J2) explained why longitudes \( \lambda \) are near-befit of 15′ and 45′’s (only 32 5 in all: five 15′s, no 45′s), though appearing with roughly expected frequency for longitudes \( \beta \); twenty-eight times more often than for \( \lambda \).

I6 Two other little-known extras regarding the Catalog: [A] Rawlins 1994L §§E4-E7 found statistically (at high odds) that the compiler of the Catalog’s southern stars observed from a place where he had estimated his geographical latitude \( L \) at a value ending in 5/6 of a degree, consistent with the southern tip of Rhodes Island, Cape Prassonesi (latitude \( L = 35.53^\circ N \), but not with Alexandria’s \( L = 31^\circ 12^\prime N \). [B] Shevchenko 1990 p.194) discovered for a specified half of the zodiac, stars’ \( \lambda \) exhibited no particular excess of 40′ endings, later, DIO 10 (2000) fn 177 tested Gem-to-Sgr (roughly Shevchenko’s range) and found that — excepting Sco, whose prime stars’ \( \beta \) reach atypically far south for the zodiac — these stars’ original Hipparchos 00′-excesses were not in ecliptical longitudes but in polar longitudes. The novel and insuperable impediment thus created for Ptolemy’s defenders is found in the footnote cited.

I7 The root of the persistence of the embarrassingly-long (given the evidence’s imbalance) “debate” over the Ancient Star Catalog, is that sneakily (§J4) stealing ordmag 1000 stars is unambiguously, undeniably a scientific crime, verifying the justice of the Newton book’s Neugebauer-klan-hated, JHAD-enraging title, The Crime of Claudius Ptolemy.

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

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

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 three lunar eclipses (Babylon, Hipparchos, and perhaps Menelaos) and twice Venus.
The Greatest Faker of Antiquity

2018 D.Rawlins

J CONCLUSIONS

J1 The Alm is an invaluable resource, our only connexion to much of high ancient Greek astronomy. Given that DIO has induced from it (passim), we are (as distinguished from R.Newton) especially grateful for its survival. But it must be used with extreme caution.

J2 The most educational observation we may end with, regarding the timorous state of the modern history of ancient astronomy community, is this: its fiscal rulership can read all that you have just been heard, though finding not a digit out of place in the ancient world or [A] Ptolemy's work; or done absolutely nothing for [B] DR should continue to be non-cited[33] for [1] his witchcraft (fn 35) in co-hypnotizing scholars (fn 1) into realizing Ptolemy cheated, & [2] exposing the vile tactics of archons'who'll never admit they were wrong to slander[33] 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 real ancient astronomy that he has preserved. Instead, we owe him our condemnation for the large amount of genuine astronomy that he has caused to lose." (Note Toomer 1984 p.1's naive guess: "the work of Ptolemy's scientific [!] predecessors . . . being obsolete, . . . ceased to be copied.") See Neugebauer 1957 p.145 & Rawlins 2008Q K3. Neither van der Waerden nor Rawlins 2008Q fn 223 agree with RRN's estimate (idem) that data Ptolemy reports from others are faked though (as most loyalists don't deny) routinely reduced to produce impossibly consistent "confirmations" of his models. Basic confusions of those who see Ptolemy's derivative astronomy as primary: [M1 b].

34 It is little understood or anticipated that any decision to shun (usually made in anger or fear, initially needlessly and precipitously puts the instigator's integrity at risk. For, the decision is a bet: gambling that the shunnee is forever worthless — oblivious to the possibility that his output may prove valid (or later to start 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: put his money where his mouth is — not knowing (as he gets progressively deeper­in) when if ever the doubt can stop. As we get to the point where evidences CENTRAL TO THE FIELD (e.g., §§111–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 herd­talk compliments toward R.Newton and his solid mathematical analyses has been, "One of the earlier astronomy that has preserved. Instead, we owe him our condemnation for the large amount of genuine astronomy that he has caused to lose." (Note Toomer 1984 p.viii), and (all Swerdlov "silly", "careless and unreliable," "Velikovskian," "absurd," "crank," "[i]ntelligentsia the most naive reader") (sources for all but Field [Greenwich, 1984] at DIO 1.1 1] [C7 & §3] [D2-D3]). And see Scientific American at fn 52. If taking academe's pretensions at faces-value, one might've thought that the JHA or the American Astronomical Society's H.A.D. would insist upon its officers engaging in rational academic discourse. Instead, we've had 44 years of impenetrable non­communication and/or equally intractable ignorance. And the proposal inadvertently trades a charge of plagiarism (Newton, loc cit) against Ptolemy for a charge of plagiarism: yet another example (above, §K1) of Ptolemy's hermeneutically­lawfu­failing to refute one of the hero's crimes while not remarking or even noticing that they're simultaneously stultipulating to another! And, again­typically (above, [§B3, a]), choosing improbable theory over probable. Unconsidered question, quite aside from the issue of theft: from where and with what degree of inexcusable ignorance would Ptolemy (uniquely, even for him) acquire — steal — a small set of star­declinations obsolete by a century (fn 20) while purporting that he observed them himself (Almagest 7.3), and insert them into a set of contemporary declinations? This is proposed as a central new theory in a DEFENSE of Ptolemy's honesty? Innocently unanticipated by the paper's authors: the 2014 paper's proposed 57 AD date for the former is near the date which Peters & Knobel 1915 p.15 found for when the Star Catalogue's mean longitude­error is null: 58 AD. But we know independently, from other evidence (fn 5) or being­obscure­too­obscure­to­believe (fn 31) that the value must lie too far­away Mufa public­relations organ. Not the 1st time cultists trying to save Ptolemy (or pan­Babylonianism: alibing one of the earlier astronomy that has preserved. Instead, we owe him our condemnation for the large amount of genuine astronomy that he has caused to lose." (Note Toomer 1984 p.1's naive guess: "the work of Ptolemy's scientific [!] predecessors . . . being obsolete, . . . ceased to be copied.") See Neugebauer 1957 p.145 & Rawlins 2008Q K3. Neither van der Waerden nor Rawlins 2008Q fn 223 agree with RRN's estimate (idem) that data Ptolemy reports from others are faked though (as most loyalists don't deny) routinely reduced to produce impossibly consistent "confirmations" of his models. Basic confusions of those who see Ptolemy's derivative astronomy as primary: [M1 b].

36 Not the 1st time cultists trying to save Ptolemy (or pan­Babylonianism: alibing one of the earlier astronomy that has preserved. Instead, we owe him our condemnation for the large amount of genuine astronomy that he has caused to lose." (Note Toomer 1984 p.1's naive guess: "the work of Ptolemy's scientific [!] predecessors . . . being obsolete, . . . ceased to be copied.") See Neugebauer 1957 p.145 & Rawlins 2008Q K3. Neither van der Waerden nor Rawlins 2008Q fn 223 agree with RRN's estimate (idem) that data Ptolemy reports from others are faked though (as most loyalists don't deny) routinely reduced to produce impossibly consistent "confirmations" of his models. Basic confusions of those who see Ptolemy's derivative astronomy as primary: [M1 b].

37 But Evans' theory fails anyway since [E] that data Ptolemy reports from others are faked though (as most loyalists don't deny) routinely reduced to produce impossibly consistent "confirmations" of his models. Basic confusions of those who see Ptolemy's derivative astronomy as primary: [M1 b].

APPENDIX I: 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 hypothetical­observer Ptolemy, were nonetheless not in "his" Catalog — and, by­an­unfunny­coincidence (explored statistically in Rawlins 1982C), all these uncatalogued stars were invisible to Hipparchos, who observed 5° north of Alexandria, so that his anticentric-equator (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 anticentric­circle testing, speculating that [none] previous mass­star­cataloger known to us had resided so far south as Ptolemy, there were no constellations to which he could attach stars in the 5°­wide strip of sky he could see but Hipparchos could not (and which no hypothetical early southern constellator had filled), so we must excuse Ptolemy — excuse him, that is, for not doing what The Greatest Astronomer of Antiquity himself actually says he did, namely, record all visible stars (Alm 7.4; Toomer 1984 p.339): "we observed as many stars as we could sight down to the sixth magnitude." Evans' dodge typifies modern Ptolemy's death­agonies: alibing one of the earlier astronomy that has preserved. Instead, we owe him our condemnation for the large amount of genuine astronomy that he has caused to lose." (Note Toomer 1984 p.1's naive guess: "the work of Ptolemy's scientific [!] predecessors . . . being obsolete, . . . ceased to be copied.") See Neugebauer 1957 p.145 & Rawlins 2008Q K3. Neither van der Waerden nor Rawlins 2008Q fn 223 agree with RRN's estimate (idem) that data Ptolemy reports from others are faked though (as most loyalists don't deny) routinely reduced to produce impossibly consistent "confirmations" of his models. Basic confusions of those who see Ptolemy's derivative astronomy as primary: [M1 b].

K2 The epoch, thus the Catalog's naively­indicated 58 AD date is just as much an illusion as defenders' naively assume power­secretaries' naivete) continue to be near­100% buffaloed by our dedicated genii.
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 Polley’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 on that far north of the thicket. Note: no other original naked-eye 1000-star catalog’s lowest star was as high as 3°: Hipparchos, Ulugh Beg, Tycho, Hevelius. That is, Polley’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 Polley’s putative observatory that just-so-happened to block just-enough of the southern sky as to make his putative observations’ declination-range deceptively look as if the observer were at Hipparchos’ latitude L instead of where Polley’s constellations’ prime stars aren’t rare. Cep: & (PK632&636) 12°. Peg: & (PK316&318) 17°. Hya: & (PK916&917) 22°. So the no-available-constellations argument is slaughtered by [2] the obvious explanation for why Polley’s sole Gru star, namely: of α, β, γ, and δ, only γ was visible to Hipparchos.

the SickFive, whose weighted mean error vanishes c.59 AD — since he faked it to co-prove false light 1/1000 from Ptolemy’s Alexandria visible stars were conveniently attachable to nearby constellations. And we know that Polley was (or copied) a star-attacher: Alm 7.5-8.1 in (which we designate as PK110 — meaning star #110 in Peters & Knobel 1915.) Further, the vast constellation Argo (today broken into pieces: Car, Vel, Pup, etc) had already been recognized for centuries, and the Catalog includes 45 of its stars (PK849-893); yet several Argo stars aren’t in the Catalog (but bright unCataloged e Car [m = 1.9] is less than 6° from Cataloged δ Vel: PK886), despite being easily visible from Polley-era Alexandria (per-aurore transient magnitude range between 2/2 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 μγ for capture. Even more peculiarly absent from the Alm catalog are α and β Gru. Both of pre-extinction magnitude μ ≈ 2 — i.e., of Big Dipper prominence! — and quite visible (§K2) to Polley at μ about 3 and 4, resp (though at all hours below Hipparchos’ horizon), they could have just been set aside as a new*99 constellation. After all, [i] There already was a two-star constellation, CMi (PK847-848); [ii] Polley was inventor of the new*100 asterism Antinous, which he formed c.130 AD from six*101 stars “around” Αql (Tooemer 1984 p.357).

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

*99 Likewise, Schaefer 2001 proposes that atmospheric aerosols instead of rocks blocked Polley 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 stats applied to ancient nocturnal air, and that the cataloger was so dumb as to not realize that the very clearest nights were those appropriate to searching out dim stars. Since Schaefer (op cit) astonishingly neglected consulting the stars in Hipparchos’ Commentary, the paper doesn’t even realize that (given the virtual identity [§H3] of the anticentric circles of the stars collected by Hipparchos and by Polley) 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 incanously) Evans propose opacities that are obviously over-high for antiquity. Schaefer (op cit) choosing an opacity of 0.2 (P = 0.1, Schaefer’s Seculica at μ = 70) is supported by Evans 1987 p.166 even imaginatively hints that perhaps there were...
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 Rhodes' southern tip, Cape Prassonessi (see fn 42 for geographical latitude $L$ and height $z$ above sealevel), Hipparchos' list [a] exhibits a startlingly clear 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. 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, Alexander, will [fn 43] give any scientist a hearty upchuckle.

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

M1  How did too much of the academic establishment get sucked into promoting astronomical history's ultimate pretender as the "Greatest Astronomer of Antiquity"? [a] Were public attacks on a famous scientist resented by science's politicians as endangering science funding? — but astrologer-mathematician Ptolemy was not a scientist. Not empirical.

any other altitude than $h = 0^\circ$; see the lucid and irresistible discussion at ibid [§11]. Further, thanks to an anonymous reviewer by B. Goldstein, we now have the fact (Rawlins 1993D §8) that Ptolemy said in so many words that 1st magnitude stars (pre-extinction $m_1 = 1$) were visible on the horizon in antiquity. (In the exchanging-frauds tradition noted at §K1, some loyalists disbelieve this, thereby [ibid fn 93] assuming Ptolemy's "horizon-stars-dishonesty [in order to argue] his Catalog-stars-honesty"). But, if we assume Evans' preferred (0.20 mags/atm) modern-model opacity, then 1st magnitude stars' visibility on-horizon entails (ibid §8) ancients' eyesight perceiving 12th magnitude stars ($\mu = 12$); and so that a single case of "visually similar and real" magnitude $\mu = 14$ is impossible.

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

Compare sealevel-Alexandria Ptolemy's §3.3 list [a]: $V = 0.15$ mags/atm, $V = 5/4$ (3.1 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).

44 Among those tested by Rawlins 1982TC 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 PK064 could be a mangled version of a position originally based upon an hypothetical observation of $\mu$ Vel, high by $15^\circ$ in R.A. (5°; 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., the significance of Ptolemy's large error for Alexandria's geographical latitude $L$ (§1.1) and real ancient science's high-accuracy achievements ([M3]), participants would (Panglossianly assuming open minds all around) have soon shaken hands, parted in peace — and moved on to more challenging historical mysteries. (As DIO long since has, most of our research on ancients being reconstruction of lost astronomy.) Instead, Ptolemy's promoters from day-one followed his example by knowing all the answers before consulting either empirical evidence (as against texts) or actual able scientists (as against cult goorners).

Decades of ugly and harmful warfare followed. Again: all needless. But as with many wars, when it becomes obvious to most observers who's going to lose in the long run, there is a bloody period when

[b] Were Ptolemy's math proofs so admirable that it seemed incredible for him to have plagiarized data? — but (Rawlins 2003X p.502): what if he plagiarized the math, too? It was long believed that the $A/m$'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-feeleable attempts to logically back up prominently published one-sided (fn 11) salesmanship, pushing Ptolemy as The-Greatest, put one in mind of Aquinas' voluminous Reformation-germinating mistake of trying to defend by reason that which cannot be defended by reason. [d] Did damage to Ptolemy's sacred-grant-cow value trigger the shunning ([B] of R.Newton's valuable insights? With the Almajest as [i] the central surviving ancient work on mathematical astronomy and [ii] suffused with fraud, grant-raising problem was presumably feared (perhaps needlessly: [j], leading to attacks on Newton but (far more tragic and longstanding) promotion of a now-widely-accepted misperception of all of ancient astronomy — just to cover for Ptolemy's fudges, by deliberately (fn 46), falsely claiming that everybody-did-it ([M2] in antiquity — a distortion that's gatewayed a 180° inversion of truth for a range of ancient-science issues, as detailed at [N], below. Above option [d] was the most likely place for the original flame of rage at Ptolemy-skepticism 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 itself evidence of falsity.]

M2  Admittedly-non-peer-reviewed Scientific American's Swerdlow-Gingerich-inspired premature "Acquittal of Ptolemy" (ScAm 1979), published in anti-Truth-terror horrified 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-premises misunderstanding of Ptolemy in particular and ancient science in general (most of which survive immutably to this day among his remaining band of believers). It verbaitum-echoed the already-echoed (§1.1) claim that the ancients seeing to 14th magnitude ($\mu = 14$) were visible on the horizon in antiquity. If, in Ptolemy's list [a]: $\gamma$ Sgr and $\mu$ Vel (<1 magnitude ($\mu = 14$) magnitude stars ($\mu = 14$) are visible on the horizon in ancient Alexandria: Ptolemy's "Acquittal" promoted Swerdlow-Gingerich's idea of established-accepted myth of 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 Rhodes' southern tip, Cape Prassonessi (see fn 42 for geographical latitude $L$ and height $z$ above sealevel), Hipparchos' list [a] exhibits a startlingly clear 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. 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, Alexander, will (fn 43) give any scientist a hearty upchuckle.

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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 aibi 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 pray for a consumer to condemn the ancient cod’s products for the accuracy of their numerical results. What is really admirable in ancient astronomy is its theoretical structure. . . .”. See also Neugebauer on Aristarchos’ data as non-empirical (Neugebauer, op cit pp.642-643; Rawlins 2008R §A1). How then did Aristarchos and Archimedes (idem & Rawlins 2012T §E1) find the solar diameter to ordmag 1’? How did ancient scientists find the mean distance to the Moon within c.2% (59 Earth-radii: Alm 5.13)? Or find their observatories’ geographical latitudes to ordmag 1’? (See [4 Table 1] or Rawlins 1994L Table 3).  How could Hipparchos measure all of his 3 eclipse-based star-longitudes (Rawlins 2009E to similar precision? (Note, too, the 3 neatly interconnected 1% hits hypothesized at §N10). How were solstices fixed ([N7] to ordmag 1’? Whence arose a Greek stade-length consistent with knowing the Earth’s size to 1% (fn 49 below)? Was it just a series of miracles that all 4 surviving ancient collections of star declinations are (Rawlins 1994L §§F5-F9 & Table 3) consistent with each of the 4 observers knowing his latitude to ordmag 1’? From where (Martian visitors?) did the ancients obtain all 3 lunar months to (Rawlins 2018U §D1) one part in ordmag a million or better? For the synodic&diurnal months: nearer ordmag 10 million! (Most of these accuracies were unknown before DIO.) It would not have been possible for these measures to progress to such admirable accuracy, if researchers had merely tailored data to previous values. Back in 1979, did Ptolemites ever take 5 seconds to contemplate such a self-evident & devastating point, ere committing to an obviously uncertain position so deeply that [fn 34] there could never again be a chance of turning back: with the courage of a Lynch-mob, gang-smearing as an incompetent crank (§ 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 (fn 12) of the inescapable §F2 Mercury inconsistency, or (§H1) the absent-error-waves test? Can Musaffosi show they ever even looked for an explanation of those remarkable million-month-precision lunar-period accuracies (§§110), BEFORE committing themselves forever (fn 9) decades ago to the above fantastic Neugebauer-klan notion that Greek astronomy was more theoretical than empirical? — and, in this connexion, to such a fragile jest as deaf-to-all-evidence promotion of an indoor faker (notorious as such among astronomers for centuries) as The Greatest Astronomer of an antiquity about whose outdoor astronomical empiricism they obviously understood a great deal (§N) less than nothing. Hopefully, these considerations will warn today’s budding archons how a meestablishment — initially from preconception and careless science, then in heedlessly hot outrage at heretical challenge (Swerdlow at §B) — and, in this connexion, to such a fragile jest as deaf-to-all-evidence promotion of an indoor faker (notorious as such among astronomers for centuries) as The Greatest Astronomer of an antiquity about whose outdoor astronomical empiricism they obviously understood a great deal (§N) less than nothing. 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N APPENDIX: 4 UNERRING ATTRACTION TO THE ERRING DOUBLESECRET EMPIRICISM & LURKING BLEAKHOLES

Those archons who for generations have controlled journals, conferences, and funding in the history-of-ancient-astronomy field assume to themselves the god-like prerogative to classify — with that infallible judiciousness so amply sampled hereabouts — and exclusively publish, those who are equally reliable, trustworthy experts, as distinguished from those crazies who should be blackballed, exiled, unpublished, uncited. However, our question to deities isn’t: have they ever had the finest idea of how an ancient astronomer actually worked, observed, reasoned, and achieved? (Or: have they ever sufficiently acquired a scientist’s attitude, for appropriate empathy with the scientists of yore?) No, the germane question is: how have so many of the field’s leaders so often concluded—for and tyrannically insisted-upon the very opposite of the truth — and on the most central issues — virtually across the board. A gang of moguls with such a degree and breadth of misperception of their own field’s realities may be unprecedented. If they are deliberately trying to acquire a reputation as the sore-dumb remnants and fanatics of the history-of-science discipline, their plans could hardly be improved upon. (Which actually would be quite unwise, in that such men as Neugebauer, Aaboe, Britton, and others are brilliant despite their 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 unrelent
N1 Asserting that Ptolemy got his astronomical elements from his "observations" — the central JHAD-inversion of this controversy. Contra: [1] A particularly penetrating yet simple demonstration of the truth is due to Thurston 1994 p4 (§D), who noticed back in the 1940s that Ptolemy’s iterative proofs of planetary orbital eccentricities start with highly precise estimates, but by the final iteration they’re round as can be. Real iterations proceed in the reverse direction. [2] Another instance of JHAD inverse-perception of ancestry-direction is shown below at §N15 item [F]. [3] Mars’ Alm mean synodic motion is accurate to ordmag 1/century, yet the “observations” on which Ptolemy’s proof of it is purportedly based are off by ordmag 1 (Rawlins 1987 p.237). [4] The Alm proof of Mercury’s mean synodic motion was already shown above (§F2) to be pretense, by one who 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 Alm is seldom understood (PHuber the happy exception — Rawlins 1991W fn 224), namely, its near-exclusive use of n observations to solve geometrically for n orbital elements. From the often excellent results sampled here throughout, we realize that ancients’ actual historical evolution towards accurate elements obviously involved repeated adjustments from numerous observations. As a mathematician not a scientist, Ptolemy never sensed the value of over-determination — where the number of equations of condition exceed the number of unknowns sought.

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

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

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

N5 Because of own mis-signing of parallax-correction (fn 47), teaching in an Oxford University Press textbook that errors of ordmag a degree (exceeding the lunar diameter!), as repeatedly found by Newton in Ptolemy’s alleged observations, were ho-hum-normal for ancient instruments, as Neugebauerians believe (e.g., §M3; also: memorable Aaboe conversation, 1976/3/9). (Note: Oxford U. Press was warned by 1997/7/17 letter of the book’s problems, but [see similarly at fn 46] went to press with all errors intact.)

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

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

N7 When a ball is tossed upward at 0° and caught downward at the same height 4° later, most of us know it maxed at 2°. Yet, from his own astonishing failure (R.Newton 1977 fn 20) to understand this high-maximum-height problem, MacArthur-Genius Sverdlow 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 Sverdlow is not knowingly ladling nonsense to vulnerable archcons who predisposed and subserviently to recognize the prank.) Due to just such JHA-published pseudo-science, Ptolemy’s even perversely teach — complete with now-ironically Pompos snorts (quoted, R.Newton 1977 loc cit) at sub-JHA untouchedable — 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 1st effect of deviation from quadracity: [§N19 below], not to mention [b] ancients’ historically uniform choice of Greeks not equinoxes for year-length-determination. Have those who’ve been disbelieving Greeks’ ability to measure solstices accurately (Swerdlow, Evans, Duke) noticed that the newly available papyrus Ptoled 267A (§N19) has tried to enlighten them by directly surprise testifying to an ancient solstice which was accurate to ordmag 1st Just-luck! (Like another trio of just-lucks at idem? Meanwhile, note that Duke not only wrongly doubts that Greek observations were sufficiently accurate for trustworthy solstices [idem], but agreeably if mistakenly proposes that Hipparchos’ 134 solstice was 5 off, when in truth it was only 1°: see van der Waerden at Rawlins 1991H fn 4; also Rawlins 2018U eq. 10 & Table 3.) We can test the point: the mean systematic error of Hipparchos’ equinoxes was 7° (consistent calculational conclusion of Britton, Newton, and Rawlins: summarized at ibid §B4), while in spite of 6° rounding, the errors in recoverable ancient solstices (one by Kallippos; one by Aristarchos, two by Hipparchos: ibid Table 3 & eqs.1&2&7&4) are +3°, 0°, +2°, & −1°, resp, indicating that Hipparchos’ rms solstitial systematic error (1°.6) was more than 4 times smaller than his mean equinoctial systematic error. Unrounded ancient equinoxes doubtless had less scatter than solstices, but the latter obviously suffered smaller systematic problems (remember, too, that the ancients [needlessly] worried about [and corrected-for: §3 fn 97] the effect of several arcmin of solar parallaxes, which would degrade equinoxes not solstices), precisely one of the cult-unperceived reasons why — when ancients sought reliable cardinal points for gauging year-length — they chose solstices.


48 Irony [due to yearlength-estimators’ dependence on truncated (Rawlins 2018U §2 [C1]) prior solstices]: despite 4 known reliable solstices (Rawlins 2018U Table 3), no ancient got an accurate yearlength. In 1977, Brigham Young Univ astronomer H.Kimball Hansen conceived a simple method ancients could’ve used: [1] Find a stable stone point on a hill which near an equinox casts a shadow (to recognize the prank). [2] But on some date around an equinox (no need to be at just one, merely when solar declination-motion is near-maximal), mark where the noon shadow is. [3] Note when it returns there 20 years later, and divide the interval by 20. The result, in just 20°, will be several times more accurate than any yearlength known to have been adopted in antiquity (even though these were based on intervals an ordmag longer) all of which were seriously erroneous, for reasons (analysed at ibid §§C-DQ) which do not apply to the remote Babylonian data which ultimately & fortunately made possible the hyper-accurate Greek lunar periods of Rawlins 2017E §B2-B4.
Historians-of-science unexpectedly ignore the perfectly Occamite 3-for-3\footnote{Only 2 ancient Earth-circumference values were widely adopted: Eratosthenes' (really Sosrtatos', really Sosrotoros', really Sostratos', really Sostratos') 256,000 stades, and Poseidonios' 180,000 later. Eratosthenes adopted each in succession, though they exhibit a previously unexplained gross disparity, the former being over 40% larger than the latter. DIO's revolutionary simultaneous solution of BOTH these C values from the same simple theory (math & sources at ibid eq.28) realizes that each Greek C differs from actual C (216,000 stades) by almost exactly a factor of 6/5 (within 1% in each case), Eratosthenes' high by 6/5, Poseidonios' low by 5/6. Hmmm. It happens that there are 2 very obvious stay-at-home Earth-measure methods (one even semi-attested at Pliny 2.65.164): [1] Pharos-flame-vision & [2] double-sunset (Rawlins 2008Q [A4]. Resolution arises since atmospheric refraction causes horizontal light rays' curvature to be 1/6 Earth's, thus the lighthouse-flame-method's result is expanded by factor 6/5, while the double-sunset-method's result is contracted by factor 5/6. However, to see this, one must be able to follow the mathematical physics. There is as yet no evidence that any historians-of-science have ever done so, despite having a century of a century's summary and citations going back to 1982 provided at Rawlins 1996C fn 47) since DIO published this hyper-neat triple solution that navigators' familiar formula for the horizon's "dip" has been based upon identical 6/5-mathematics for over a century?!?) These 2 DIO matches exceptionally are achieved without judging the stade at all, but just by adopting the standard 185 meter value. [Thus serving as the final, controversy-ending proof that 185m was indeed the true length of high antiquity's stade.] Our achievement here may be compared to the controversy's endlessly wheel-spinning metrological-solution literature: sniffing-out through ancient lore in search of hints of oddball stades, which of course existed all over the place before the Ptolemies presumably regularized the measure by defining their empire's royal stade at 185 meters, commonly misapplied "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 metrologial. Again (§110), mathematically impeccable matches are targeted data within about 1% of all data wees...]: [a] Eratosthenes' C, & [c] the 185 meter stade that is now (Rawlins 2008Q [J1]) accepted by virtually all serious scholars. By contrast, ALL the usual solutions for C (invariably just metrological rehashes), that keep filling journals' pages, can only match 1 out of the 3 (and even that match is usually several times looser than 1%). Notice the astronomer-defiling surprise that the Pharos solution of the famous Sostratos-Eratosthenes C is geographical, not astronomical. [Speculation follows:] But refraction is effect when we realize that the uncluttered (but low-refraction) presumably-Kleomedian-astro-nautical survey-based 185 meter stade implies C = 216,000 stades (since the product equals actual C = 40 million meters) but 216,000 is the cube of 60, hinting that (before Sostratos cleverly but wrongly found for C = 256,000 stades, c.270 BC) scientific surveyors had obtained an accurate C to which Greek science had naturally applied standard Greek sexagesimal division to the Earth's meridians (the process' step 1 is even attested: §3 fn 111) to define the stade so that C = 60° = 216,000 stades. I.e., sexagesimalization (triple division by 60) of Earth-C 40 million meters produces a geodetically correct "sexagesimal" stade of 185 meters, fine for 600 stades/degree, but not for Sostratos-Eratosthenes' later-famous (but seriously-too-high) 700 stades (of 185m each) per degree. But: when did this hypothetical achievement occur? Traditional games at the Olympic stadium began before 300 BC, but when were the stade-long-footrace markers now found there (Engels 1985 p.298) established? (If athletes ran in various positions, there may have been general standards of 185=0.250-200-225 comprises for 5 ancient stades the disparate lengths of the traditional 1-stade footrace, where the sole 18.5m is also the only one (Athena, reconstructed +143: Gibbon Decline ... Chap.2 [ModLibc 1:41]) that's post-Ptolemy 1. In the Hellenistic world, where else than Egypt would terrain allow a long flat North-South arc, of ordmag 1000 km. Given that the Alexandria-Meroe arc's curvature is apt to a meridian circle of C = 39870000 m, a Strabo-attested 10000 stades interval at (700 st/degree) squared, equates (within 1 part in 1000, that is, the cities could be spaced 14°1', that we are able reliably to indicate Earth-C so accurately that hypothetically surveyors' 185m conclusion was trustworthy ± within ± 1 m. Was Kleomedes' famous Alexandria-Asswan legend a myth (DR's former opinion), or a remnant of Ptolemy I's post-conquest land-survey of his empire? [With similar possessiveness, less scientific William the Conqueror rekindled his own new boast in the Domesday Book.]

Philos' at Meroe: Rawlins 2009S §C, the latitude difference between Alexandria (31°12' & Meroe (16°57') was knowable angularly as 2.3/8 sixtieths of C. (At the later standard of 700 stades/degree, this is 9975 stades, only a quarter-percent short of the Strabo-attested distance of 10000 stades.) But, given the habitable Nile Valley's narrow sinuosity and the non-trivial longitude difference between Alexandria & Meroe: how would the survey team measure the 1578 km latitudinal N-S difference between the two cities, in order to divide by 2.3/8 60\textsuperscript{th}, 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\textsuperscript{b} before Ptolemy 1, as witness the Great Pyramid's precision (DIO 13.1 pp.21f). Indeed, Egypt's traditional latitudes near Giza-Heliopolis were better than C.Ptolemy's (Rawlins 1985G p.260). But Kleomedes hints at a direct N-S arc. Pondering this, we note: an arc-path due south of Alexandria, virtually along the 29° 9 E meridian, could be measured without being interrupted by the Nile or tough-grade mountain at any point; the wide path west toward Meroe's latitude (already mapped by Philos: [3] in 11]) on that way. Precisely reconstructing the numerical achievement: presuming measurement c = 300 of the 1578 km terrestrial arc south from Alexandria (31° 12') to Meroe's Philo-determined latitude (16° 57'), 14°1' the stade would have been indicated to be 1578000m/(14° 1/4)/60 = 185m. [Note added 2018/4/24. DR's researches have ultimately realized a hitherto-unperceived 3\textsuperscript{rd} BC century Greek-schience dichotomy: adoption of Babylon's division of the circle into 360\textsuperscript{th} (degrees) for sky (Rawlins 2012T fn 3 & §E), but passing use of 60\textsuperscript{th} 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 clumsily 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 [An]) 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) not, though not necessarily proving, mere derivative degeneration from its conquerors’ superior technology. (See also Rawlins 2018U §J4.) An obvious & devastating point, never previously emphasized for its implications: not a single trig table survives in any form from Seleukid-era Babylon. Babylon made no solstice or equinox observations (Neugebauer 1975 p.366), or meridian or vertical observations of any kind, because (Rawlins 1991W §E3) Babylon had no transit instruments (vs Greek celestial transit data from c.300 BC onward, at Alexandria and Marseilles), and accordingly didn’t know or care what that city’s geographical latitude was. Which may explain why ‘the only really attested figure for it, 38°, was attained in Greek records, not a word on L in anywhere in cuneiform material, another crushing blow to Babylonianist pretensions, and [b] is too far north by 2°28’ = 148 nmi. Finally, while Greek planetary order was physical — Mer-Ver-Mar-Jup-Sat — Babylon’s was astrological, Beneficent—Maleficent: Jup-Ver-Mer-Sat-Mar.

N14 Failure to learn anything from the first of DIO’s eclipse-cycle solutions ([§J33], 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] 623/13/14) neatly recovers all 10 digits of Ptolemy’s previously mysterious final lunisolar equation (Rawlins 1996C eqs.21-31): 8523 tropical years = 105416 sidereal 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 1° irreputable evidence for ancient use of sidereal—tropical transformation of period-relations (Rawlins 1996C eqs.26-27), a process later extensively employed in DIO 11.2, which allows (here, in fn 8 [A]) reconstruction of Venus’ accurate pre-blunder synodic motion.

N15 Among the most egregious of all inversions of ancient astronomical procedure: our uniformly-on-the-nose huge-cycle-solutions of no less than 4 long-mysterious periodic lunar mysteries (§§N14 and §§N16-17) fall upon locked-up minds, disbelieved without a glance by jeering JHADists who (frustrated by inability to find error in heresy’s math but determined [§B3] to reject the obviously probable in favor of the preconception-accordant improbable) resort to whatever dodge will serve. Besides jeerleader Jones’ private carelessly unchecked & glaringly one-sided misfire ([§N16], there’s a long-popular theory (item [E]: put into writing privately by P.Huber, likely inspired by Neugebauer [also primarily a mathematician, like Ptolemy] that is so inverse-contrary to sensible scientific practice as to gain special popularity among gaping cult-minds, thiry for any refutation of undeserving outsiders’ proposals, thus not just suggesting but insinuating JHADists’ pure speculation that very long period relations which originated from splicing together a few much shorter relations, & no other hypotheses need apply. (The truth was 180°-opposite from this rigid position, of course: ancients’ more-convenient short relations descended from less-handy ultra-long empirical ones [DIO 11.2], for reasons about to be explored, below.) Which demonstrates yet again our JHAD’s unerring attraction to the erring. Considerations:

[A] Without even being told, all positional astronomers instinctively know that the secret of ensuring high accuracy for a 2-event-based celestial period P is just to wait for a large enough number N of returns, ensuring a huge time-interval, t1 to t2, so that the error in deduced t2 - t1/N caused by the errors in t1 & t2 is trivialized by the enormity of N. (How else could the ancients determine [Alm 4.2] the synodic month correctly to within well under 1 time-sect?) [B] Even Ptolemy knew enough ([§3 fn 119) to use very long intervals when faking non-periodic arc/time estimates of solar, lunar, & planetary speeds. [C] The short planetary periods of Alm 9.3 are obviously not directly measured since they are [i] not integral (there is a remainder of a few days, from the cycle’s imperfection), & (see item [D]) [ii] tropical, not sidereal. [D] Genuine, huge, observed integrals, were planetary period-relations listed in Ptolemy’s Planetary Hypotheses (Neugebauer 1975 p.906 Table 15), mostly on the order of 1000, verifying to all but spulse-dreaming JHADists that long cycles were recorded by all civilizations. These vast planetary cycles are all listed by Ptolemy in sidereal years, which is JUST the type of cycle one obtains directly (no need for indoor splicing) from centuries-separated pair of raw outdoor observations of stationary stars at the same time, as explained by Neugebauer 1975 p.390, producing period relations without remainders, just as in Plan.Hyp. (Neugebauer loc cit also supplies centuries-long sidereal planetary periods for each planet: again, no remainders.) [E] There are many ancient attestations (§N16) to direct determination of long celestial periods, but no attestation (or purpose!) for stringing-together short ones to fake very long ones. Ibid p.555 produces an atypical ancient text that for Mars splices sidereal cycles of length 32° (5° short of 15 synodic years) and 47° (4° beyond 22 synodic revs), to produce 79° (1° short of 37 sidereal revs) with the advantage of reduced (but still non-zero) remainder. (Neugebauer or source mistakenly renders the three-day-remainers as degree-remainers.) But there’s zero evidence 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 (unremendered) period-relation of the direct outdoor-obtained type which was already empirically available anyway ( & more accurate by an ordmag) without such needless fiddling, & upon which (Rawlins 2003J) all Alm 9.3’s short periods were ultimately based. [F] The Alm 9.3 Jupiter 71 tropical cycle’s superficially-inexlicable 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) from an unremendered outdoor-observed 427-sidereal yr integral period-relation, not the reverse (a fiction made fact by Neugebauer 1975 p.391). See Rawlins 2003J §J4.

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 ([§§J34-J37) 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 remnants), with common integral factors removed, as at Alm 4.2&6.9 where factors 17&10, resp, are divided out to simplify the ratio. (Our-long-cycle teaser at §N14 lacked such divisibility.) Consult esp. the half-dozen next evident and fits (Rawlins 2002H §C3-C9) backing the theory that the draconitic month was determined by Hipparchus, by using the very same back-end — 140/11/27 eclipse he’d used (Alm 6.9) when first applying (almost as accurately) the same eclipse-period method. (Debate-averse Jones privately produced a single-item retort [which insta-melted upon examination: Rawlins 2002H §D], while ignoring all 6 shocking obvious positive evidences, possibly a non-scientist’s receptivity to data contrary to invincible preconception?) These empirical RATIOS at last explain how ancients determined lunar motions (which Ptolemy&cuneiform express as RATIOS, after all), all accurate to 1-part-in-ordmag—a-million or better. The proposed method: [1] is bi-attested (Alm 4.2 and 6.9), while no other pre-100 BC method
is attested at all; [2] is the sole ancient method even nearly capable of such hyper-accuracy; [3] automatically spits out ratios; and, [4] inducing the three solutions without manipulating a single digit, one finds ratios that are exact matches for all six 4-digit factors (24 digits!) in the previously-unexplained ancient records. During the 1 1/2 decades since publication of this unexpected and fruitful discovery, no scoffer has found in the analyses [a] any math error, nor [b] any alternate eclipses also producing the data we have solved-for (which would show our solutions’ non-uniqueness), nor [c] any below-horizon eclipses used in the DIO analyses. Nor [d] the courage to cite or debate this theory in print: fn 6. NB: Whereas the selected pairs’ back-end eclipses are spread across 4 classical-era centuries, the front-end eclipses are 4 times more tightly grouped, in a single productive century: the 13th BC.

N17 It is revealing that the JHAD clique has not only failed for decades to understand the central ($\S$M3) significance of the Almajest’s three lunar periods’ high accuracy — but to then let shunning dictate refusal to cite the 1st solution ($\S$N16) anyone has yet achieved, for how these periods were obtained?! That’s non-citation with an impressively unambiguous lockstep… . . . (Is there a prize we don’t know about, for this special brand of perfecction?) To summarize, cultists keep secret from JHA readers and the larger public BOTH: [1] the historically revolutionary empirical significance ($\S$M3) of the inescapable fact that all three ancient lunar-speeds are micro-accurate, and [2] the eclipse-cycle source ($\S$N16) of such accuracy. (Let Animal House’s Dean Wormer top THAT Double Secrecy.) And don’t miss the saddest&sadist-masochist part: all disbelieving archons are religious Babylonianists, but their coherent disdain for target-heretics trumps even worship of Babylon, as their rabbotic texts (ibid $\S$N18) of such voluminously and naively issued his enormous double—article (Evans 1987 — ibid) Aristarchos’ 157 solstice & adoption of Kalippic motion, both of which (after same JHADists’ scoffs) turned out ($\S$N19) to be anciently attested. In this field, what is intelligence for, if not to revive lost antiquities? Were the earlier data extant, there’d be no inductive mysteries here. Who deserts 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 48888’ Great Year, Aristarchos originated the “Babylonian” month, 29’19”00’50”, decades before Babylon ($\S$3 [G4] or Rawlins 2002A eqs.4-8). [2] DR’s 2001/6/27 British Museum lecture showed (ibid: eqs.9-11; or $\S$3 [G5]) Aristarchos applied to this the Metonic cycle (235 months $\equiv$ 19’), resulting in his Metonic “tropical” year $\text{y}_{\text{A}} = 365'4'/4 - 15/4868$ (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 $\text{op cit}$ p.601, $\tau \xi \epsilon \delta \gamma' \kappa' \beta' \delta'$ or 365’4’20’60’2” if expressed as the continued-fraction $365'4'/4 + 1/20( + 2/260)$) (Rawlins $\text{op cit}$ p.12-13), this is $\text{y}_{\text{A}} = 365'4'/4 - 15/4868$, verifying above item [2]. All without altering any Vatican document number and much aided by Neugebauer 1975 p.602’s perceptive interpretation of $\xi$ as sixtieths. [4] The sidereal-year companion data, Aristarchos, $\text{op cit}$ p.12, $\tau \xi \epsilon \delta \gamma' \kappa' \beta'$ or 365’4’10’4’ from Vat. gr. 381 fol. 163’ (Neugebauer 1975 p.601), we write as continued-fraction $365'4'/4 + 1/4( + 1/10 - 1/41)$ ($\S$3 [G2]; Rawlins $\text{op cit}$ fn 14-15), yielding sidereal year $\text{y}_{\text{A}} = 365'4'/4 + 1/152$ (good within a few time-seconds), again altering no Vatican-ms digit; and, again, a hit: the interval from Meton’s —431 solstice to Aristarchos’ —279 solstice is just 152’. [5] The difference between $\text{Y}_{\text{A}}$ & $\text{Y}_{\text{A}}$ IS PRECESSION, suggesting geomobilist Aristarchos’ apt pre-Hipparchan discovery of it. His value is near 1’/century, a much-too-low false rate, later rounded to exactly 1’/century & adopted by Ptolemy. Having adamently 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) vandizes 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 are unreferenced data encourage.

N19回归结果源于从新近出版的文献$P$.Foud 267A.

[1] The papyrus’ vindication of the previously unknown Rawlins 1991W ($\S$K8-K9&M4) discoveries that Hipparchos sought a —157 S.Solstice and that he (twice: idem) used Kalippos’ 365’1/4-year solar motion. Both his —157 solstice (a different day: Rawlins 2018U $\S$L-M) & use of Kalippic motion are explicitly provided on the 1900’ old papyrus. (Kowal & Van Brummelen have enjoyed similarly unexpected years-later vindications, which the reader should look up to, share the felicity.) [2] Simultaneous solution (ibid $\S$K4-K5 & eq.25) of P.Foud 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, 365’4’/4 – 1/309? The field was informed of these discoveries in 2015. Inert reaction: $\S$ fn 66.

N20 Confusing the almanacs and handbooks of extant derivative science — cuneiform texts ($\S$13) and Almajest ($\S$M2) — with primary, which is not very extant. Possible practical reason: it’s easier to raise grants for analysing existing works than lost ones, and it helps hype to push the former as central — though the probability is minuscule that ancient watershed-research astronomical manuscripts would be numerous enough (compared to handbooks: $\S$M2) to have survived to the present. (Archimedes is the rule-proving exception in mathematics, but even his work on solstices [noted at Almajest 3.1] is lost.)

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

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

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Alexander Jones 2002E. JHA 33.3:15.
O.Neugebauer 1975. History of Ancient Mathematical Astronomy (HAMA), NYC.
D.Rawlins 1992V. DIO 2.3 9.

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

D.Rawlins 194L. DIO 4.1 9.
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D.Rawlins 2000A. DIO 10 [co-published with the University of Cambridge].
B.Schaef er 2002. Sky&Tel 103:2:38.
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Gerald Toomer 1984. Ed. Ptolemy’s Almajest, NYC.

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 [N? and fn?] trio cited in the piece he published [Swerdlow, Gingerich, & V.Thoren], reflecting the kind of muttered slander (more at fn 35) created and spread behind backs by parties many of whom even today keep believing that if they can just preserve or salvage some sliver of doubt that Ptolemy faked, they are thereby not utterly convicted of the vilest brand of academic misbehavior, in their decades of gang-smearing those who were — the ultimate irony — on the side of truth right along. Why merely adds ethical incomprehension to scientific. And why would a party have ever in the 1st place resorted to slander & shunning & running instead of inviting debate, 52 [§1] it genuinely believed evidence & competence backed its position? Why did peace never break out? DIO 16 p.2 fn 1 (2009), emphasis in original: “Rational, pacific discourse shows who’s right & numerate, so: why would archons tolerate peace?”
Astronomer in Wonderland: Historians-of-science

The technically & ethically-rockbottom brand of “research” skewered in the following pages recalls DIO’s prescient summary during NatGeoSec’s 1989/12/1 issue ex-officio-launch of NGS’ amateur [Rawlins 2017b] data-juggling defense of its dying Peary North Pole hoax:

Orchestrate more fiddle factors than the New York Philharmonic.


Universities’ science departments deserve to know the kind of math-mish (fn 13), herd-think (fnn &10), data-tampering (§§B-G), & idea-grabs (fn 10, §C8) too often passing for scholarship in prominent but joke-refereed (§ fn 3; Rawlins 1991W fn 6) & coup-over-prone (fnm 10, & 97) journals in history-of-science, a field rife with smearings (fn 8), shunnings (fn &k;16; Rawlins 1991W fn 17&173), threats (fn 109), & rejection of normal science (bizarre details: idem & §126; fn 100) if favoring heterodoxy, with research-advances’ acceptance contingent upon whose clique the discoverer belongs to. (Repellent examples: Rawlins 2017E §G3.) Therefore, there’s mighty evidence that archons teach, value, or even understand (§§G5 & J 1 [f], fnn 42&106) exploratory hypotheses’ use, tempered by Occam (§125, fn 33, §1 A, §2 fn 49), to expand&refine knowledge. The result (p.45 & §§B5&C-G) regarding advances in ancient astronomy, is inevitably more destructive than constructive.

However incomplete, the following chronology is a start towards top academic institutions’ enlightenment re contemporary history-of-science’s frailties. (Even while DIO values the field’s finds [e.g., fnn 42&127&§114, §2 §F2&fn 42], from which scientists have learned. Despite won 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 gagging-smeared math-competent, even eminent intruders as van der Waerden, R.Newton, H.Thurston: despite physicist D.Rawlins’ half-century of astronomical-history researches (samplings above & p.2), a staid version, www.dioi.org/gjir.doc, of the following please-clean-your-house paper (with amiable cover letter), was inflexibly (fn 100) spurred in 2017 by the History-of-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 demonstra-tions of the most prominent historians-of-scienceALTERING DATA (esp. §§C-D&F-G), uncorrected-unretracted math-batches (§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 eaily the most mathematically and astronomically competent journal in the science-history field. vol. 10 even openly exceptionally co-published (with the University of Cambridge), long supervised by boards of that rare minority of scientifically able historians (e.g., astronomer-legends E.M.Standish, emeritus CalTech-JPL, & Chas.Kowal, late of STSI), so obviously feared by the democratically-ruling majority, whose mathematical and ethical shortcomings DIO has been patching-up for decades without the slightest discernable (positive) effect on the field. E.g., three cornered History of science journals cut contact with DIO, when, e.g., (fn 97), asked to print the embarrassing but unquestioned fact that their icon Ptolemy’s four Sun observations were FIFTY TIMES closer to Hipparchos’ 280-old indoor tables than to the outdoor sky, none doubting (§2 §N8) Neugebauer-Gingerich’s-Science’s decree that an astrologer & clumsy faker whose frauds damaged & retarded predictive astronomy for 1000 years (§2 A) was “The Greatest Astronomer of Antiquity” (fn 1 here). Have shunning, 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 insensibly-normative? Simultaneously with Isṣ’s resistance to the below history, it was learned (see §1 here for links to all papers) that: [1] a 7-odd DIO discovery (Rawlins 2008q fn 6) had been unattributedly published, www.dioi.org/cev.pdf, as Isṣ’s 2015 LEAD article (repair request repulsed), and [2] Isṣ’s pseudo-refereed final 2016 LEAD article had extensively attacked Rawlins 1985G (Greenwich Meridian Centenary 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 Isṣ even yet realizing it sorta matters. (See hist sci icon Neugebauer’s able 1975 analysis at §1 §D; so Isṣ’s 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, Isṣ Ed. H.F.Cohen fled (“I will not read, let alone respond to, any further messages on your side.”), play unanimously endorsed by his 30 Adv.Editors, www.dioi.org/isb.pdf, as Isṣ 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 catalogued at www.dioi.org/jhb.htm, the oddest being “science” as credible as the Earth’s East Pole (Winnie the Pooh Chap.9): [a] the 1976 Dictionary of Scientific Biography 13:321 discovery of the Autumn Solstice and [b] JHA 22.2:119’s 1991 discovery of the Winter Equinox. See §§C-G for data-tampering by top pols, incl. the NYU Institute for the Study of the Ancient World’s Director, fitfully brilliant Alex Jones. Isṣ’s 2017 coverup of its 2016 sham-refereeing disaster (p.8 here: 7 largely-obvious undetected errors, 2 of them crippling) is just the latest example of the level of equity, ability, and openness at history-of-science’s most eminent & incestuous forums, which now exist in a state of such evidence-immunity and no-consequences self-rule that they have for a 1/2 century been tragically & punitively insisting (awful details: §2 §N8) if favoring heterodoxy, with research-advances’ acceptance contingent upon whose clique the discoverer belongs to. (Repellent examples: Rawlins 2017E §G3.) Therefore, there’s mighty evidence that archons teach, value, or even understand (§§G5 & J 1 [f], fnn 42&106) exploratory hypotheses’ use, tempered by Occam (§125, fn 33, §1 A, §2 fn 49), to expand&refine knowledge. The result (p.45 & §§B5&C-G) regarding advances in ancient astronomy, is inevitably more destructive than constructive.

After decades of observing science-shy historians-of-science and watching limited mentalities (fnn &k) like careenists 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-gooness by history-of-science archons, have led to a defensive pattern (gameplan?) of curling-up into a self-protecting world of classical-historical-skilled-think quoted historians 1994/8 §4b: “We don’t want the history of physics to be written by senile physicists.”

Its 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 retrogressed, 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 leaned research, and [b] if historical investigation in the mathematical sciences would be more openminded & technically able if it were hence to proceed within, or sometimes in supervisory association with, the relevant science dept’s of those universities that value it.
History-of-science — Data-Tampering, Idea-Theft, Seminumeracy, Smearing, Shuns, Club-Preferences

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

Muffia Cult’s 84\textsuperscript{2} 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: \cite{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 \cite{b} are being smothered by a chauvinist battery of destructive, data-disrespecting — even data-fudging — papers, whose logic ranges from desperate to supernatural,\cite{2} displaying scant evidence of refereeing or such epistemic canons\cite{3} of scientific evaluation as simplicity, minimal-premises, Fruitfulness, and predictivity. Auto-rejection has been inspired by durable grantmagnet (Diller 1984 fn 26) orthodoxy that the famed ancient data-faking ([JHAD] = cartel of JHA Journal for the History of Astronomy, O.Gingerich principal editor for 40\textsuperscript{th} & HAD (Historical Astronomy Division, Gingerich long-dominant co-founder) of the American Astronomical Society, whose Ethics Statement has just demoted [2017/10/1] 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 ([JHAD; Rawlins 2017E] vs. Hist.sci reverence for alluring inaccuracy, to ameliorate Ptolemy’s gross fabrications, e.g., inverting relation of theory&evidence (Ragep cruelly: fn 9), & focusing on Greek “theoretical structure, erected in spite of the enormous difficulties that beset the attempts to obtain reliable empirical data” prominently quoted by Gingerich 1976 p.477; see fn 8, 62, & 97 here. Neugebauer 1975 p.931 crowned indoor astrologer (Rawlins 2003X) Ptolemy “the greatest astronomer of antiquity”, echoed verbatim by Gingerich 1976 [AAAS!] & Gingerick 2002. Since Ptolemy’s Almajest contains much of what survived from ancient mathematics & math-astronomy, it has become accepted-in-practice that grantmanship requires continuing pretense that this invaluable astronomical handbook (the 1\textsuperscript{st} great modern translations call Ptolemy’s Almajest and Geographical Directory “handbooks”: see each’s title in References below) was primary science (fn 9), not derivative (which it obviously was: 2\textsuperscript{nd} 52\textsuperscript{nd} & 200 here, or Rawlins op.cit), whatever the cost to plausibility and ethics. Another JHAD promotion of derivative science as primary: fn 120. 2\textsuperscript{nd} Consistently ininvincible auto-rejection of high-odds, perfect-hit solutions, which have the effrontery to contravene current orthodoxy, encourages vulnerability to adopting embarrassingly unlikely alternate theories, and thus (effectively) escaping into the miracle world of the supernatural, palming off — as valid scholarship — notions unworthy of a rational enterprise. For a JHAD-wayhouse of ultra-outré occultisms, see here at: §§C11, D2&D3, E2, G7-G9, G11, H4, I22: fnn 12, 33, 44&45, 55, 68&69, 89.

\textsuperscript{3} DIO’s principled approaches to knowledge are brought together below, at §§I [g]. See, too, fn 10, hugely contra-reality super-adherence to 280\textsuperscript{th}-old indoor tables, is uncriminal since Greek astronomers were theorists not empiricists, who suppressed — i.e., destroyed — data inconsistent with prevailing models (fnn 8&9 below). The Princetuitive’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/dioiind.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 heretofore remarkably unappreciated high-accuracy Greek measures. (Below, compare §§10 [& §§D] to craniolithic cult-insistence on Greek inaccuracy: fnn 1, 8 [1], 69, 93, & 58B.) Also investigated: ahistorical myth of “theoretical” Greek non-empiricism & data-selection; Occamite resolutions of such problems as ancient Earth-measure by Pharsos flame & double-sunsets; the method explaining all 3 Greek-adopted monthlengths becoming undeniably accurate to 1\textsuperscript{st} or better (!); how all 3 hitherto-unsolved lunar speeds were based on classical-era use of 13\textsuperscript{th} century BC Babylonian eclipse data ([§34]); pseudo-Aristarchos’ daily retrograde Moon & Archimedes’ degree-use (both obvious, yet unnoticed for 2000\textsuperscript{th} : §§11&12 below); Ptolemy’s celestial fakes; Hipparchos’ elaborate and 1\textsuperscript{st}-accurate trig tables; his use of spherical trig; Archimedes-admired pioneer in heliocentricity & spatial-vastness, Aristarchos: P’Tannery’s and DIO’s quadruply-verified (fn 88) reconstruction of his temporally-vast 4868\textsuperscript{th} Great Year, 1\textsuperscript{st}-accurate monthlength, and pre-Hipparchos discovery of precession. One of R.Newton’s favorite expressions for counter-revolutionary mis-scholarship: A subtraction from the sum of human knowledge.

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

\textsuperscript{4} Wikipedia’s article on the virtually unrefereed Journal for the History of Astronomy [JHA] actually claims the journal is “peer reviewed”!! (See fn 109 below, also re Wikipedia’s 2008-2014 war upon Rawlins’ Wik-biography.) Meanwhile, the best-refereed journal in the field, DIO, is repeatedly, aggressively-clossed by Wikipedia as Unreliable (not deserving an article, with bio-references to Rawlins as “publisher” persistently suppressed), though neither Wikipedia’s CSICOP-soldier administrators and associated threatening cult-vandals (repulsive details also at fn 109) nor JHA have in years of trying and seething, managed to find incompetent scholarship anywhere in DIO’s score of volumes. Meanwhile, by a contrast that would be embarrassing to honest forums, DIO has (without even having to try) discerned dozens of flagrant examples of prominently-published history-of-astronomy catastrophes that nobody referred to (or cared to referee): see fn 50&86 below and above at §1’s POSTSCRIPT; also www.dioi.org/jhh.htm, www.dioi.org/fil.htm#exe. 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 incumenzous, 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 mufury is JHA’s rigorous refereeing of sufficiently
[A] Noncite the heretical paper primarily targeted. **Deliberately:** §§C10, D6, G11, fn 10, twice more at fn 121. (This pattern goes back at least to the prominent Sky&Tel attack on R.Newton by dutiful Muffiosha Janice Henderson 1976, without citing his name or papers.)

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

[C] Effect sterile destruction by demeaning or even 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 Janice Henderson 1976, without citing his name or papers.)

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

impunitively ignore mathematically and logically solid but cult-displeasing findings for decades. This transparent behavior continues even despite 2002 national attention to “unprofessional” acts characterizing the Ptolemy Controversy (ancient astronomy’s hottest). Are debate-averse conservatives justified in unprofessional contempt towards unprofessionally uppity heresy? That question is explored below, along with the field’s domination by dissent-intimidation & shunning, which has only solidified (fn 125) since 2002, perhaps from scientifically-shy historians-of-science hanging shy of scientific critics. Which suggests several questions which may lurk behind superficial arrogance:

1. Are their deeds like French deps staffed by pros who don’t speak French very well?
2. And pretend it doesn’t matter. (It does: §§7; Rawlins 2017E §K2; Rawlins 2018V end.)
3. Is this too many historians-of-science cannot (e.g., §B4) admit mistakes (as scientists routinely do: R.Newton, B.L.van der Waerden, DIO 11.2 [on cover!], S.Goldstein, etc) & can be 100% sure teamplay fellow historians-of-science won’t ever ask them to? Does that relate to the inverse: science-historians’ endemic reluctance to acknowledge non-club-members’ vindications? — a reflex which can reach such extremes (fn 17 & §F5) as to defy any known academic ethical code. (Outside sororities: Rawlins 2005R Epilog.)

B Hiding Modern Empirical Data: Boomerang Irony & Lawlessness

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

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In 1987, in order to justify the modern Ptolemaist vision of antiquity, JHA-Editor-in-progress J.Evans published an unexceptionally polite, technically pathetic, but politically brilliant Step-One towards becoming a Mufa Maid-Man by assassinating R.Newton’s credibility — anticipating full well the boost he’d achieve towards his ultimate Editorship by publishing JHA Editors’ bête-noire-Newton, i.e., telling ‘em what they wanna hear, regardless of the cost to truth and to the reputation of one of ablest scholars ever to grace the field. (And regardless of whether Evans’ paper was valid. All that mattered to JHA was the attack’s teamwork-contribution to the pretense that Newton was as crazy as its cult’s unanimous goosestep-slander was insisting: In 34, Which is why the parties soon proven right [14 §4 and 32] Catalog of all ve pseudo-Aristarchos farces), and none in this cringing B2

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

Among the gymnastic hysterical-astronomy pratfalls enlivening JHA’s hefty (64pp!) James Evans double-lead-paper attack [Evans 1987], upon (then-minority) Ptolemy-doubters, was Evans’ lordly illustration of [skeptics’] dumb overestimation of ancient [observational] accuracy . . . . [Evans op cit] n.50 (p.275) presents his own non-telescopic (cross-staff) 1981 July 16 Seattle observational determination of the longitude of a star (λ Sgr) by using a lunar eclipse (as Hipparchos had) [measuring the star’s angular distance from the Moon when [it was] 180° from the Sun’s already-tabulated position] — which after Evans’ reduction produced a longitude erroneous by −2°/3, thus according to his idem (showing that the huge errors in some ancient observations were so ordinary that such were a poor basis for learning anything about ancient science [i.e., condemning Ptolemy’s gross errors]). As further examples, Evans specifically mentions (idem & p.235) Hipparchos’ two hugely disparate Spica data [also eclipse-based] . . . which disagree by over 1°. He then draws for us a [Ptolemaist] lesson (empth 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-valueable J.Evans, History and Practice of Ancient Astronomy (Oxford: Oxford Univ., 1998)] p.259 (“This shows the size of the possible errors in ancient measurements of absolute star longitudes”), he slyly deletes mention of his formerly prominent 1981 eclipse-star measures — which shows that (during the 1987-1998 interim) Evans had read [the 1991 revelation]10 . . . that DR had discovered

9 Rawlins 2009E §A (emph in original), criticizing Evans 1987, http://journals.sagepub.com/doi/pdf/10.1177/00228268701800401, also Evans 1998 (appreciatively reviewed for its considerable merits by Thurston 1998D in DIO 8). Overview-question-in-passing: is there any reason other than ambition that would lead a scholar to look for a way to defend Ptolemy’s honesty where (as for the Moon-star case at hand) the evidence is too obscure for non-specialists to understand? A reader has already for at least 1200-1800 years known, correctly, clearly, uncomplicatedly, high-schoolishly, arithmetically indoor-computed his four alleged “observations” of the Sun: §8 below. A textbook case of politics overwhelming reason — as it has, for ordmag a century of history-of-science’s ubiquitous, naked promotion-for-grantprofit of a known scientific criminal. (See, e.g., §2 or Pedersen 1993 p.559’s justification of Ptolemy’s massive indoor plagiarism of Hipparchos’ catalog of all 500 of Eratosthenes’ 25000 stades, which coverupper Isis is refusing to even correspond on much less undo (1° here), while Evans hides from his obligation to obtain any of ID’s published works. (Recall: J.Boye appropriated “Bode’s Law” though it was actually 1st published in a footnote to a non-astronomical work by J.Titus.) Specifically, Carman & Evans 2015’s proud Pb-paper’s central equation, Eratosthenes’ Sun-distance $S_T = 102°$ [102 Earth-rad] (fn 42 below), was already discovered & published for the 1st time 700 years earlier at Rawlins 2008Q’s waypoint eq.9, which Isis’ leaned authors couldn’t see past. (To connect to a new world of Greek scientists’ ingenuity & precision which DIO’s paper delights in exploring: curious readers will enjoy sharing this journey into the previously unknown, far past where Carman & Evans 2015 stopped.) Incredibly or typically, the authors reveal to readers no hint of: [A] Airbending’s effect on geodesy (fn 25 below; Rawlins 2008Q). [B] Pseudo-Aristarchos’ “p-A” factor-of-four giveaway slip (1°2 here). [C] Archimedes’ contradiction of p-A (idem), saying Aristarchos’ Sun-diameter was 1/4 of p-A’s (correct half-degree vs ridiculous 2°), which C&E certainly knew about since it appears on the very Archimedes page cited (for other cause) at Carman & Evans 2015 n.1. (Shades of other knowing archonal non-citations noted here at fn 121.) [D] And p-A’s lunar 3° parallax’s follies (§2; Rawlins 2008R §C1). [E] Also p-A’s daily-retrograding Moon (idem), caused by lunar distance 1/3 of reality. (Which Carman & Evans 2015 p.101 wished to brushoff of Ptolemy’s connexion to astrology, believing that only unenlightened, “paradigm”-insensitive scientists could suppose it reflects negatively on him.

10 A 1999/4/2 Evans letter boasted of ashscanning DIO 8, allegedly not reading it (& its p.2 exposure of his citation-integrity’s consistency; also fn 127 below). [Equalh honestly Peary doade at Bryce 1997 p.602.] Groundwork for further bibliographical sins? E.g., Carman & Evans 2015, www.dioi.org/ae.pdf, prominently published as original, without attribution, Rawlins 2008Q fn 6’s discovery, ignoring by him at Pedersen 1991 p.258 by assuring us that the Sun’s angular distance from the known-position of a star’s position, from measurement of its angular elongation from the known-position Moon at mid-eclipse: Evans’ own 1981 Seattle observation of the star λ Sgr — the record of which has since disappeared without explanation — and two ancient observations of the star Spica vs the eclipsed Moon (Almagest 3.1) by Hipparchos in −145 and −134. The errors were all ordmag 1°: respectively, −40°, −33°, +33°.
field has a word to say in criticism. Parallel Evansiana: despite denial of reading DIO, Evans’ clumsy unannounced sly-try 1998 eclipse switch (unsuitable details: fn 11 below, or fn 47 shows) he read Rawlins 1991W fn 288. DIO’s detection-revelation of Evans’ 1987 parallax-misuse, ever-uncited by him during 3 decades of hiding from publicly facing this central demonstration of his cult-engendered fallibility. Also ever-Evans-uncited: DIO 3, www.dioi.org/3001.pdf, the standard critical edition of Tycho’s 1004-Star Catalog. Rawlins 1993D (fruit of DIO’s 7 years of scrupulous investigation & math-reconstruction: 1987-1994) flagrantly deliberate citation-avoidance in U.Press. Evans 1998 pp.271-272 & n.28 thereon (p.459), 57 after DIO 3 appeared. (Will scholars not following suit office the human-hyper-tender sensibilities?) In April 2009 published because by a journal correcting an Evans mistake he pretended was never made? (But doesn’t say so. [Indeed, doesn’t say anything.] And no historian-of-science asks. A field ruled by fear for decades. But note that B.Schaefer has admirably ranked works on JHA-shunning of DIO 3.) More Evanscence appreciated in Rawlins 1992T §[H-H7 & fn 65, and at Rawlins 1993D §L8, where Evans is shown to unwittingly require Ptolemy saw 12º magnitude stars. (Ptolemy-Flamekeeper Evans in 2013 succeeded in calming as JHA Editor. Utterly aptly.) Interminably listing at www.dioi.org/hh/fdjm, not listing further examples of deliberately-ignored (recall 1999/A/2 letter, above) revelations of undeniably erroneous but never-retracted Evans scholarship. See also DIO § 84 fn 4 on the unsuitably-ambiguous not-entirely-technical—empirical-argument at Evans 1991W p.72, ultimately adopting (non-citationally) yet another DR original discovery (§4, DIO 1.1.7 §C1; Rawlins 1991W fn 99, fn 263&272; Rawlins 2008R fn 17): Aristarchos’ 87º half-Moon elongation as not precise but a lower limit. And don’t miss www.dioi.org/hh.htm#cdjm, on Evans twice copying J.Dreyer’s prose without quotation-marks. irony: DIO’s Tycho star catalog & the differently-important Hipparchos parallax-sign discovery, were both triggered by Evans’ own mis-apologia for icon Ptolemy. At Rawlins 1992T §H8 & Rawlins 2009E §A6, find our gratitude to Evans&Hoskin for each of these gifts. (Like thanks to Jones&Toomer at Isis & our gratitude to Evans&Hoskin for each of these gifts. (Like thanks to Jones&Toomer at Isis &(B11 & fn 207&292; to Duke, §C14 above & Rawlins 2012V fn 22; to the whole Maffija at Rawlins 1991W §S3.) Evans’ modesty precludes him from citing any of these thank-yous. Compare to DIO’s rule of always correcting its errors — for both integrity and refusal to mislead readers: www.dioi.org/err.htm, as well as below at §I14 & fn 98&110, also DIO 1.1.4 §A2 (1991) & DIO 11.2.2003) front cover & p.30. JHAD’s perverse reaction to the contrast neon its priorities for all to see. And eyeroll. 11 Rawlins 2009E fn 4: “Both inquirers were told by [now JHA Editor] Evans that he would look into [DIO’s correction to his eclipsed-Moon-vs-star experiment]. But he never communicated what he found. Except by implication” when in 1998 (fn 7) he published another, on a new eclipse. 12 Rawlins 2009E fn 5: “E.g., Rawlins 1982g p.263 & n.17 (dissed by Swerdlow, rejected by 13 Rawlins 2009E fn 5: “E.g., Rawlins 1982g p.263 & n.17 (dissed by Swerdlow, rejected by
seriously.

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

C3

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

C4

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

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

C5

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

...
of Hipparchos. Jones does not merely pollute Strabo’s klimata with these alien data from scattered other works of Hipparchos, but uses them as an excuse to shift the entire dozen, suggesting Strabo’s data could have been anciently corrupted by addition of a constant, A = 100 stades (meanwhile Jones consistently miscomputes Syracuse’s 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 knows that Hipparchos changed adopted parameters over time.

C6. Further, Jones (ignoring, 23°2′3′′ confirmations: [JH4: Rawlins 2009S fn 23 & 54]) accepts Ptolemy 1.12 testimony that Hipparchos’ sun’s parallax was Eratostenes’ ϵ = 23°51′20″, a value which Jones imaginatively attributes to a speculative Hipparchan computation from a conjured-up non-Hipparchian Alexandria L = 31°, without realizing that obliquity ϵ would already be known since ancient found it concurrently with L — and via solstices, not (as Jones curiously assumes)27 equinoxes.

C7. Jones also-traditionally alters28 the Almajest’s text for Hipparchos’ Marseilles latitude, from L = 43°0′4″ (consistent with 43°1/12 of the Polemy Geographical Directory [GD]) to L = 43°0′1″, in order to reconstruct (using Eratostenes’ ϵ) 2nd-century BC BC Hipparchos’ Jones-suggested indoor calculation-invention (for unstated reasons, and counter-clockronomically) of Pytheas’ well-known longago (c.-300) solstitial noon gnomon ratio, 41:42,105 (whose precision argues it was an outdoor29 datum); and, to force the speculation’s success, Jones begs tolerance of an odd-but-convenien Hipparchian miscalculation,30 yet another ad hoke2 wretching of ancients’ data. [While rejecting Table 1’s normal roundings!]

C8. Jones’ promotion31 of such jigsaw juggling seeks at least a half-share of Diller’s discovery. Worse: by fantastically alleging that Diller used invalid data, Jones (Neugebauerian: §C2) lodges his half-fitted theory — unnented (§C10) and untabulated-whimsical — as SUPREIOR to Diller’s ultimately-perfectly-fitting one. Though willing to refer uce 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 [Isis] & by G. van Brummen’s meticulous standard history of early trigonometry [Princeton University]) — or the final DIO 16 §3 update distributed in 2009, where the ultimate blenchless-fit perfection (Table 1 here) of Diller’s 14-for-14 victory is too irreal to deal with. Except by fleeing.

27 Jones loc cit. Syracuse miscomputed: Jones 2000e n.10. He also controversilly if traditionally altered a key Alexandrian datum: fn 24 here. His highly Creative obliquity-speculation: op cit p.16. His preferred for equinoxes over solstices for L: Jones loc cit (the obliquity was determined by Thurston); and below fn 96 vs Britton 1992 p.29. Non-Hipparchian Alexandria L: Rawlins 2009S fn 30. Obliquity ϵ found concurrently with L via solstices: ibid §§F2-F3 & eq.8, and Almajest 1.12.
28 Original-text Hipparchos Marseilles L = 43°0′4″ (Almajest 2.6), rounded at GD 2.10.8 to 43°1/12, often altered to 43°0′1″ (defining GD’s consistency with 43°0′). See Rawlins op cit JH. Achronology: Jones 2002e p.17. Pytheas gnomon ratio: Strabo 1.4.5.2 & 5.4.1. Rawlins 2009S pgs.2-3.
29 ibid Summary [1a] and Rawlins 2009S §F4.
30 ibid 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 near. F.Ragep 2010 L.128’s Jones-promo sees no problem here.
31 E.g., Jones’ foreword to recent (long overdue) reissue of Pedersen 1974.
32 Initial Diller-Rawlins table of Strabo data compared to Neugebauer’s: Rawlins 1994m p.56 (aptly augmented in later renderings, as noted at fn 19). JHA-uncited for 24′ now, but noted by Thurston 2002S p.67, and by van Brummen 2009 p.65. Final 13-for-13 version of Diller-DIO klimata table: www.dioi.org/vols/w50.pdf. Diller 1984 Table 0. The table’s subsequent 14-for-14 expansion: here in Table 1 or www.dioi.org/jg03.pdf, Rawlins 2009S Table 2.
33 Sensitivity: caption to Rawlins 2009S Fig.1, graphing Strabo’s 14 klimata (Equator & 12°34′-10°4).
34 DIO method of estimating multivariate-probability loci: www.dioi.org/biv.htm#xmxw. Neugebauer vs Diller-DIO, shows that above Table 1’s col.5 “Conv/Stads/Sc” (for Diller’s ϵ = 23°40′, A = 0, with L rounded to nearest 5′) counter-intuitively fits the data (col.7: “Strabo”) over 25% better than ϵ & A produced by least-squares (where also klima 14 fails). Neugebauer 1975 uses 4 unknowns (Rawlins 2009S fn 7). Jones, 2 (ibid §3). Do snurners note Diller needs only one (ϵ) to produce column 6 (“Round/Nearst/100 St”), the Diller-DIO table’s subsequent 14-for-14 expansion: here in Table 1 or www.dioi.org/jg03.pdf, Rawlins 2009S Table 2.
his theory’s doubtless-illusory woes: they are the unreliable party, having committed the offense of disagreeing with the theory of the most authoritative expert (here, in fn 86), adding that Hipparchos’ trigonometry tables are suspect of a parallel disloyalty. Meanwhile, DR contends that the glad & enlightening opposite is recommended — both for Strabo’s climatic data 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 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, comparing it to the Orwellsian-Goebbelsian flames it deserves.

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

NB: We now have consistent confirmatory double-evidence for Hipparchos’ 1”-accuracy: [a] Table 1’s 18th klíma, as just noted (fn 37). [b] Below fn 46’s neat hits for attested &r.

C13 Ironically, D.’s rejection (fn 69 below) of that reconstruction unintentionally highlights the Hipparchos trigonometry tables’ accuracy. It just seemed incredible to Duke that DIO’s analyses (precised at §D here) of Hipparchos’ Almagest 4.11 eclipse-trios could possibly be reliable, from sensitivity to tiny uncertainties. Yet we have multiple-verification of those analyses’ validity, because Hipparchos’ calculations (fnm 24&52 here) & trigonometry tables were more accurate than previously believed by Duke or anyone else (including DR, before 1991 testing). So his/her/their now-known inaccuracy doubtfully just emphasizes the shocking newness of the discovery of Hipparchos-era 1”-accuracy computation & trigonometry tables: [1] the two eclipse trios (customarily called “A” & “B”) confirmed each other by both producing Hipparchos’ ancient tables based on Kallipic motion (period 365½/4) [2] new papyrus testimony vindicated in 2005 (§F4) both of the novel inductions of DIO’s 1991 analyses: [A] Hipparchos’ search for a ~157 Summer Solstice, [B] his contemporary passing adoption of Kallipic solar motion.

C14 None of these confirmations could have succeeded unless Hipparchos’ trig tables were indeed (as already indicated: fn 37 below) accurate to 1”, as later were Ptolemy’s (Almagest 1.11). Specifically, if DIO’s 1991 elicitation of lunar eccentricity ε from Trio A had computed with a key trig function off by 1” (from slightly unreliable trig tables), Hipparchos would have found other ε than ε = 327/23, the correct value, which is attested (§D1 below) & is found to agree (§D3) with calculation via 1”-accurate trigonometry tables. To repeat (§C12): [i] the Diller-klimata’s 18th entry (here, in Table 1 & fn 37) and [ii] DIO’s eclipse-pair-based orbit reconstructive matches, both consistently establish the 2nd century BC as the earliest date we know 1”-accurate trigonometry tables existed.

Concluding this section’s revelations, of sph trig & 1”-accurate trig tables & calculations, 3 centuries before Ptolemy: we recall the mentality that long ago locked in establishment-wisdom here, Gingerich 1976 p.477 in Science (!), blaming his hero Ptolemy’s huge errors on (caps added) “CLUMSY mathematics invented only a generation earlier” (12 §M1 [b]).
DIO-J.HA 22 3 Data-Fudgy for Myth&Turf 2018 D.Rawlins

R = 1000°cot70° = 52°24′, or, in 60ths, 3144′ — thus matching 43 Trio A’s R (above). Now, a common slip (ancient & modern) is confusion of unit-fraction (inverse integer) & arcmin, since each is signed by a prime-marker; so if we test the hypothesis that a Hipparchan-school computer later misread 52°24′ as 52°12′, we find, in 60ths, 3122′1/2 — exactly matching 44 Trio B’s R (above). Unable to counter the math, centrist pols (faces eternally, irrevocably invested into shun: fmn 125&127) have, during the decades since 1991, had no reaction to this minimal-premises double-match of both 3144′ & 3122′1/2, besides implicitly contending 45 (by pushing incompatible fudge) that this is all PURELY COINCIDENTAL. Any wonder the above Summary was forced to confront the supernatural? — see fn 2 here.

D3 DIO also found double-consistency with its theory that Hipparchos’ calculations 41 had used eclipse-pairs 46 not trios (the trios-approach has never yielded unmanipulated data-matches). 42 & 43 had sought only one unknown, eccentricity e (or epicycle-radius r), not apogee-at-epoch A₀ or mean-longitude-at-epoch ω₀. Thusly computing e&r produced 327°39′ & 247°30′, respectively, each a neat match to the above (§D1) corresponding attested Hipparchan data: 327°2/3 & 247°1/2. (Finding no mathematical error, Duke regards these matches, too, as just ANOTHER spooky double-happence.) H.Thurston & John Britton carefully verified all the 1991 paper’s supporting math & recommended publication. In 2005 soon-after-JHA-boardmember D.Duke defied those recommendations (despite Toomer’s honest acknowledgement 47 of his 1973 speculation’s dubiousness), resurrecting-recycling it by altering — explicitly altering — Toomer’s numbers, to ensure its success by inventing teleologically convenient calculations. DR, recently expanding 48 his 1991 orbit-reclamations, compares DIO-vs-Duke simplicities:

that History-of-astronomy’s present Ultimo Archon mis-signed his 1981 parallax-correction (§B4), but won’t admit so since silence helps keep alive the JHAD sacred cow of Greek inaccuracy. So far, his goal of draining academia’s honesty has proved perceptive. Ability-consistency of JHA Editor Evans’ recent here (faithfully carrying-on the exemplary tradition of 1970-2013 Editor Hoskin’s grasp of mathematical astronomy: www.dioi.org/fff.htm#fffy); [i] 1981-1987 failure to sign parallax correctly, and [ii] 2015 failure (on solar-distance) to understand that if Greek observing accuracy was on the order of 1° as Gingerich insists (Rawlins 2018U fn 3), and as Evans echoes (item [i] or §B4 above), then Eutrophonous’ parallel’s is given an orbit 1° (or 56′ at Carman & Evans 2015 p.14), solar parallel’s uncertainty was orbit 100% of its size [like §I 5E]. (Rawlins 2008R §C5 argues that Aristarchos suspected an even higher one.) So only non-scientist pols like Ptolemy or JHADists wouldn’t realize that the JHA-isis 102° distance’s orbit 10%-1% precision is way-larflably too exact — when uncertainty is 2 or moremfs looser. All this reveals an even looser grasp of Greek astronomy (& elementary mathematica astronomy) by two of the world’s most deliberately-eminent history-of-science journals. Among famous classical Greek astronomers, only faker Ptolemy insisted on solar-distance numbers from long-earlier solar-distance Trios A&B, & of the inevitably weird unrestored-Trio-A-accordant orbit, as if original, though 49 its dominant Greatest, has warped history-of-science’s perception of actual science in antiquity. 43 Rawlins 1991W eq.23.

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


46 Eutrophonous’ orbit [§N7]. This should be obvious since [a] Almajest presents the data in pairs, after all!, & [b] pairs-analysis yields matches to the trios’ attested elements, while other approaches don’t. Inducing that A₀ and ω₀ were pre-assumed, not sought: Rawlins 2012V §J. Pair-calculations’ matches of e&r to Hipparchos’ attested values: ibid §§F2&G2, & Rawlins 1991W §N14, & Thurston, loc cit. (Doubling double-occultism: implicit in Duke 2005ST. Summaries in Thurston op cit pp.60&66-67.


D4 DIO’s reconstructions [A] are consistent in method (for both trios); [B] are rife with ancient-typically round-number elements (Rawlins 1991W eqs.5, 8-9, 11, 21-23); [C] change no Almajest 4.11 numbers, these already long-established by Newton’s learned 1977 analyses (§E below), & invent no convenient Hipparchan mechanical mis-calculations. D5 By contrast, Duke: [A] like Toomer, calculates R first for Trio A, then reverses course 49 to satisfy Trio B, which doesn’t work, anyway, unless an extra variable d (hitherto not in evidence, in Ptolemy, India, Toomer, DIO, or elsewhere) is arbitrarily brought in to rescue the situation; [B] finds no round elements; [C] alters extremely precise numbers like 51°30′23″ and 8°48′28″ to instead become extremely precisely 51°19″37″ and 8°48′06″, respectively, of e’re’s no resemblance of eke-fudge, or anything dependent justification of fudges so shamelessly explicit, besides rigorous issuance of The Right Answer. All to smother DIO’s natural-flow-multifit coherent solution under a pillow fluffy with special assumptions&tampering, resurrecting the spirit of co-subtractor Jones, above (in [C] & [B] in fn 85). Like Jones (§C10, Duke has refused DR’s request to withdraw the paper. D6 The non-manipulated Rawlins 1991W fourfold-fit reconstruction (above, in §§D2-D3) is never cited throughout Duke’s prank (did hypothetical referees even know of it?), though his paper originated as a challenge to that very reconstruction, vying for a DIO prize, www.dioi.org/prl.htm, but evaluated and rejected by DIO prize-judge Thurston. It was later published by Centaurus. (After refereeing whose superficiality is shockingly obvious for math and even text.) Contra Duke’s attraction to committing fudgery: throughout Rawlins’ researches, it proved unnecessary 51 to ‘correct’ any of Hipparchos’ calculations in order to draw coherent results from his data, so a historically new conclusion 52 emerged:

Hipparchos’ purely mechanical computations are dependably flawless.

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

E1 DIO shows (fn 56 below) Hipparchos’ – 157 Early solar orbit “EH” was succeeded by his – 145 prime-years’ orbit “PH”, in turn replaced by his – 134 Ultimate orbit “U1”. He adopted EH for his long-earlier solar-elements from long-earlier solar-distance Trios A&B ([§C13&E2], fn 55). In 1977, physicist Rob.Newton detected 54 a hitherto-unsuspected 1° error in Trio A’s 3rd eclipse, warning that data-restoration here is demanded (fn 91 below) & “incredible” consequences will flow from analysis. The orbital elements derived by warning-rejectors Jones and Duke inadvertently double-confirm 54 Newton’s “incredible” prediction. Like Jones, Duke does not notice Newton’s 1° warning, & deliberately (§D6) acts as if the carefully-referred (§D3) DIO paper did heed it does not exist, though it was unsuccessfully challenged (§D6) by Duke tothurston and (fn 32 above) precisely in Isis in 2002. Duke’s paper independently computes 55 best-fit eCA of the Trio B-accordant EH orbit, & of the inevitably weird un-restored-Trio-A-accordant orbit, as if original, though


50 Rawlins 2012 fn 17, and especially fn 22, where we find that no referee even read the Duke paper’s TEXT. For practices & business enterprises that lead to similar disasters, see fn 4 & fn 72.

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

52 ibid §A3.


55 ibid fn 293. The Ake for Trios A&B, which are independently computed and presented at Duke 2005T fn 5 and Duke 2008W loc cit., were published years earlier at Rawlins 1991W fn 208&162, respectively. Half&kalf discovery (§E2 below): ibid §M5; and §M6 discerns that the 2 elements
all 4 solutions had appeared earlier in the very DIO paper he is non-citing while trying to exile & replace it.

E2 Nonciting Newton’s and DIO’s analyses protects readers from learning that, after application of Newton’s unexpectedly productive-predictive 1st correction, Trio A is satisfied by unzany elements, which turned out — as discovered 14’ later — to be half from the EH Sun orbit (already known from Trio B, §E2 below) and half from the familiar prime PH Sun orbit. Unless utterly coincidental (as Duke and Jones judge reasonable) or an evil miracle, the result reveals, that, at the time of his Trio A calculations (PH’s –145), Hipparchos’ adopted Sun orbit was transitioning from EH to PH, so he temporarily retained EH’s eccentricity and Kalippic mean motion (until their new PH tables were computed & prepared), while immediately adopting PH’s zeno-point & apogee (nether needing tabulation). Subtractors must see as further mere-coinidences 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 unmocks who’s behind sinon DIO’s outrageously incredible, still-accumulating concatenation of impossible accidents: could it be — SATAN?!?

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

F Solar Orbit Reconstructions and Fruitfulness

F1 Hipparchos’ –145 Prime PH solar orbit (§E1) is famous, because adopted by Ptolemy (Almajest 3.2&6) & still worshipped by Julian over 500’ after creation. But DIO reconstructed85 (two other Hipparchos orbits): his — 157 Early EH orbit (§E1) & — 134 Ultimate UH orbit (idem), each seriously differing from PH & previously unknown. (NB: Hipparchos’ use of a late non-PH orbit was presciently induced a half-century ago before anyone by Britton 1967 pp.45-47.)

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


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

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.

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

BM55555 [ACT #210] (c. –100) bears a yearlength computed from Greek solstice data, an unexpected, shockingly-contra-orthodoxy discovery: 1st definite proof of what must have been substantial Greek influence on Babylonian astronomy (e.g., fn 120 & §2 [N13]). Backed immediately by Britton and long accepted near-universally; but lately unnoted, except opposed by Duke (non-citationally), using a faked Almajest 3.1 report: fn 70 & §F9.

F3 Recovering the UH orbit cleared up a dozen-halves-longtime-hanging mysteries simultaneously (resulting DIO papers never cited by JHA):

[1] Why the Aristarchos –279 and Hipparchos –134 Summer Solstices are the only61 two among the twenty-eight solar records62 of Almajest 3.1&7 where Ptolemy (who abhorred discrepant data) suppressed the hour — which we’d never have known, absent Babylonian cuneiform text BM55555.

[2] Hipparchos’ final three calculated positions63 of the Sun at Almajest 5.3&5 generally conflict with PH, but are all consistent with UH. (Jones appears to accept this analysis.)

[3] When Ptolemy recomputes those true longitudes (via the PH orbit he adopted throughout the Almajest), he twice finds disagreement with Hipparchos’ reported values (all computed below, §F9; or ibid §§B3&B4. BM55555’s revelation: ibid eqs.6&8. I will ever be grateful to the 2008 article’s author, journal, & putative referees: should the reader be censorially denied the opportunity to decide for himself whether or not §E2’s astonishing but Centaurus-uncut half & half upshot is meaningful? — and thus whether R.Newton should be credited for a finding that triggered unanticipated progress, as valid discoveries will.

61 Below, §F9; or ibid §§B3&B4. BM55555’s revelation: ibid eqs.6&8. I will ever be grateful to the 2008 article’s author, journal, & putative referees: should the reader be censorially denied the opportunity to decide for himself whether or not §E2’s astonishing but Centaurus-uncut half & half upshot is meaningful? — and thus whether R.Newton should be credited for a finding that triggered unanticipated progress, as valid discoveries will.

62 See, e.g., the bizarre attempt at Neugebauer 1975 p.284 (followed by Evans 1998 pp.273-274 & n.32, etc., contra ibid p.209, as noted below, at fn 127), to claim that Ptolemy was a BETTER observer than Hipparchos, oblivious to their relative errors, random & systematic (Rawlins 1999 §E — the section of this paper which was suppressed by JHA Editor Hoskin, without showing error of any sort). This joke-inversion is based merely on roundings in Hipparchos’ semi-popular Commentary which are cruder than for his regular longitudes (Almajest 3.1&7.2) or declinations (Almajest 7.3).

63 Neugebauer 1975 pp.642-643, deems Aristarchos’ data nonempirically faked (similarly Evans 1998 p.72 vs Rawlins 2006R §A, sardonically at §A3, condensing the most unexceedable of JHAD fantasies (empirical) & heliocentrist-pioneer Aristarchos was a non-observing fabricator, while go-along-geocentrist & data-faker Ptolemy was antiquity’s ABLE observer. . . . If some oddities are more unique than others, then this one is uniquely unique.” Bringing the foregoing up to 2018: JHAD perception is that 1st known heliocentrist Aristarchos — who discovered precession and the scale & mechanism of the Solar System, knew the stars were at least thousands of times more remote than Ptolemy did, sized the monthlength and sidereal year to high precision — was a minor, confused figure. But a fabricating, handbook-generating astrologer was the Greatest Astronomer of Antiquity. Or, as our counter to S&T’s gratuitous 2002 Feb smear noted, www.dioi.org/st56.htm, “Aristarchos was (among other credits) a heliocentric pioneer in promoting realization of the Earth’s place in a huge universe. (Also, he evidently was aware of precession well before Hipparchos: www.dioi.org/jb24.pdf, ibid p.134 Summer Solstice) caused


57 Hipparchos’ ultimate improved data (–142 Autumnal Equinox, –134 Summer Solstice) cause 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 DOI 6 3 §D9. For JHA subsequent insistence on prising Thurston’s followup, to again avoid crediting the shunned discoverer, indeed entirely deleting his name from Thurston’s note: see ibid §H. Alex Jones’ retraction late but exemplary.

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

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by him from UH, unbeknownst to Ptolemy); however, the 2nd alone agrees (by chance, as it happens), though the underlying mean longitude he lists for it is discrepant by $5^\circ$ versus PH — even while tellingly agreeing to the arcmin with UH.

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

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

[6] UH, at or before the 127th Autumnal Equinox, follows Meton’s sacred $-431$ Summer Solstice by exactly $304^d1/4$, or 16 or 2 such intervals just equal the $4868^d$ “Great Year” of Aristarchos. And perhaps of Hipparchos himself: if the latter invented a version of the $4868^d$ cycle at $1778021^d$ (not Aristarchos’ $1778022^d$; §5 below) it embodied an astounding quintuplet of geometrically expanding cycles.

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

F4 From fitting EH to eclipse-trio B, DIO mathematically induced (Rawlins 1991W §§K4-K9) in 1991 that Hipparchos’ earliest Sun orbit, [a] a used a $-157$ summer solstice, [b] adopted Kirkallipic solar motion, $360^d/365^d1/4$ for Trios A AND B. Findings [a] & [b] were both previously unsuspected. But, 14 later, paper P.Fouad 267A was examined by A.Tillon (paper 1st presented: Peking 2005) & was found to explicitly verify 1991


56 Quintuple succession of doublings (Rawlins 2002A at fn 14’s conclusion): $304^d1/4$ (1st difference between 3001/4 and $150^d1/4$ Hipparcian calendars); $360^d/365^d1/4$ (Hipparcian cycle); $360^d/365^d1/4$ solar eccentricity. Each of these includes (like the uneven song, “The 1 Day of Zmas”) all the features of the smaller cycles preceding it in the quint-sucception. Check it out: you’ll be fascinated at Hipparcian’s hypothesized cleverness. And (idem) successively halving the 1st, yields nearly, sidereal (152’), Kirkallipic solar motion (1st cycle) as where the $4868^d$ Great Year encompasses about $2^d$ of Meton’s (Easter) 19 cycles. Details: ibid fn 17.

66 Titon op cit. The papyrus’ Solstice-day $-157/266$ (correct) seriously differs from Hipparcian’s original false indoor-computed S.Solstice ($-157/268$), as reconstructed at Rawlins 1991W §K8, a point precisely resolved when Rawlins 2018U §K5 discovered both [1] the solstice’s histerto-unknown hour, $18^h$ (missing on the papyrus), and [2] the exact origin of the previously-unaccounted-for remainder of the papyrus’ Titon-discovered novel tropical-Metonic yearlength, $365^d1/4$ — cf.309. On 2015/4A, 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, the 1st formula ever developed for finding solstice observations’ small ordinal $15^d$ systematic errors (from Earth-orbit eccentricity). Ibid: eqs.10-13. Not to mention DIO 20’s lead paper, with its important fresh discovery (§1 below) of Archimedes’ 3rd century BC use of degrees. There has been no engagement on any point as yet, except for a somnambulist-refereed JHA paper, Duke 2008W, which (at its pp.293-294) doubts Greek observational accuracy by centrally confusing systematic error with random error, causing misuse by a factor of ordmag 10 (see §F8 here, or Rawlins 2018U §B). The paper’s author, though unable as usual to find mathematical error in the shunned proposal, nonetheless earns his place on JHA’s board in traditional (fn 116 below) fashion by attacking it, albeit frustratingly reduced to merely non-quantitatively implying that his $3^d$ hits (upon the right year, and twice on the right solar motion) must have been just another trio of of three hits big, big, big, and coincidences! (Now do you understand the advantages of dispensing with real refereeing? Another at fn 4.).

This Duke claim appeared soon after Titon 2010 was presented at CaTech in 2007, timing which suggests the possibility that the JHAD perceived danger right away: the nightmare of general recognition of the foregoing triple-miraculous papyrus-vindication, of a banishee’s paper which had also satirically-verifiably through a collection of errors (Rawlins 1991W §G9,a DIO 4.1 §4, 1994) — so the usual bungled lead-paper discoveries [a] & [b]. (Titon has further shown that, c. 150, Hipparchos experimented with previously unknown versions of solar motion, epoch, & precession.)

F5 Reaction to DIO’s vindications has been less than inspirational — but valuably revealing, nonetheless. While still under the influence (fn 59 above) of the late Curtis Wilson, Jones graciously assented to UH; but nowhere has it been acknowledged that (repeaing for emphasis) Rawlins 1991W and (fn 278/96) Rawlins 1985H revealed, years in advance of P.Fouad 267A’s surfacing in 2005: [a] Hipparchos’ search for a $-157$ solstice; [b] his tables’ use of Kirkallipic way-out-of-date solar motion; [c] ancient solstices were accurate to ordmag 1.

F6 NB: These DIO induction-predictions aren’t side-issues. They are central to understanding the 277th years of Hipparchos’ evolution from amateur-observer-astrologer into an immortal empirical scientist. And substractors have been uniformly oblivious to a central steel connexion, revealing his original resort to calculating not observing his earliest, grossly-incorrect solar motion, Reconstructing Solstice in $-157$, indoor-computed using the obsolete Kirkallipic cycle: this is the most conspicuously odd building-block of the lopsided EH orbit, accounting for most of why EH’s eka were so flagantly awful: $e = 3^d1/4$ (vs $2^d1/2$ PH, & $2^d1/10$ actual), $A = 4^d4$ (vs $6^d5^d$ PH, & $6^d1/2$ actual).

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

F8 This is but a jawdropper case of confusing systematic error with random. The Duke paper’s Table 1 displays admirably well-computed times of Hipparchos’ equinoxes, a wise precaution, to prevent anyone from checking anything — also without mentioning that the Vernal Equinox’ O$-1$ return’s epoch, the Autumnal O’s return’s epoch, the Great Year: Rawlins 2002A (its fn 17) presented 2001/6/27 at British Museum conference, “Under One Sky” — condensed version published simultaneously in conference proceedings (2002). Furthermore, Duke’s historian-unusual (§4 fn 43) listing of C is all positive, since the Rhodos equinoxes were subject to $7^d$ systematic error (found independently by 4 different scholars) which corrupted all these equinox-deignations (equally well-refereed: see challenge here at fn 70) were both previously unsuspected. But, 14 later, paper P.Fouad 267A was examined by A.Tillon (paper 1st presented: Peking 2005) & was found to explicitly verify 1991

68E.g., Thurston 1995 added note by Jones & (also creditable) Jones 2005. But here, in the neighborhood of above §c, and fn 85 below, there is double-irony in Jones 2005’s perfectly chosen titular quote from brotherfudger Ptolemy.

69EH&PH elements compared at Rawlins 1991W: §§K9 vs §K10. Duke 2008W pp.293-294 calls DIO’s reconstructions “spooky-lucky” due not to DIO errors in the unremarked math but because said math is too “sensitive”, implying (fn 66) that §F4’s double-vindication by papyrus was merely spooky-lucky. These inexcusably (esp. §D6) righteous attacks are met in fn 37 above, and indeed had anticipated decades ago in Rawlins 1991H §JH & Rawlins 1991W fn 205.


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

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

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

F11 He calls the EH orbit “neither conclusive nor satisfying” since (emphasis added) “parameters deduced from trio analyses [fn 46 above], are very sensitive to small changes in the data.” CHANGES?! It appears that orbit-challenged Duke expected professors to view it as well as at [55 above, item c], still couldn’t find alternate orbits [i] which fit all the relevant data of Almajest 4.11 and 5.3 & 5.8 — while RAWLINS 2012V calls successively Trios A, B, and C — as do the EH—PH (“Frankenstein”), EH, and UH orbits, respectively; AND [ii] under whom leaving cardinal points (Vernal & Autumnal Equinoxes and Summer Solstice) uniformly hit upon Hipparchos’ standard 1°/4 precision — dawn, noon, evening, midnight — as all 9 cardinal points for EH & PH(UH, not to mention P.Fouad 267A: fn 66 here) conspicuously do. Duke has been publicly challenged (fn 70) to produce his alternate orbits. Nothing has come forth.

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

History-of-science Archon’s Talismanishment of Evidence.

G1 The mystery of the superficially-nonsense ancient yearlengthsstant found on vat. gr. 191 fol. 170v and vat. gr. 381 fol. 163v laid unwound through decades of fruitless disagreements (fn 87 here). The name of Aristarchos of Samos is written beside two of these yearlengths: \( \tau \xi \zeta \varepsilon \delta \kappa \varepsilon \beta \) and \( \tau \xi \varepsilon \delta \varepsilon \varepsilon \varepsilon \varepsilon \varepsilon \), or 365’4’20’ 60’ 2’ and 365’4’ 10’ 4’.

G2 Taking the numbers exactly as they stand and allowing signage-flexibility, RAWLINS in 1980 treated both Aristarchan expressions as continued fractions, and swiftly sent the results to the Journal for the History of Astronomy. Listening to Neugebauer’s perspective extrapolation of 6000, RAWLINS saw that the 13 expression could be viewed as 365’1/4 + 1/2(0+2/601) = 365 1/4 — 1/54688, a classic Metonic “tropical” year, quite close27 to the known (also seriously false) tropical yearlengths of Hipparchos & Potlomy. The 2° expression suggested 3651/4(1-1/10-1/4) = 365 1/4 + 1/152, differing but ordmag 10 from the actual sidereal year then (fn 114 below).

G3 Both results’ implicit periods, 4868’ (Great Year) & 152’ (2 Kallhipic 76’ cycles between iconic Meton’s and Aristarchos’ S. Solstices, – 431 & ~ 279, respectively), are among the EXTREMELY28 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 close27 to 1/century, which presumably 29 earlier influenced Hipparcos to treat 1/century as a lower limit, though Potlomy eventually adopted 1/century exactly (Almajest 7.2–4). Note that Aristarchos is the only astronomer on the Vatican motto, “for two different yearlengths, obviously suggesting precession. As the 1st astronomer we now was a public geonomist, he is an apt candidate for true discoverer of Earth’s precessional wobble.

G4 To measure the Moon’s motion & apogee, ancient scientists wisely chose (Almajest 4.2) the 4267 month eclipse cycle for its 126007.530594, correct (even today!) to a fraction of a timesec. Aristarchos’ 223-month idem: 0.530594, correct (even today!) to a fraction of a timesec. Aristarchos’ 223-month idem: 0.135, correct (even today!) to a fraction of a timesec. Aristarchos’ 223-month idem: 0.530594, correct (even today!) to a fraction of a timesec.


G6 Rawlins 1999, with welcome 1982 assist contributed by Owen Gingerich (cited ibid fn 7), regarding the utility of negative signs in continued fractions.


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28 The 4267 cycle’s crucially & conveniently trivial inconstancy: 1° roughly quantified by Rawlins 1996C fn 18 & 56, en route to verifying it’s the empirical source (Almajest 4.2) of ancients’ discovery of the key relation 251 = 269°. Note revealing Muffato inversion at Toomer 1984 p.176 n.10. 29 Ibid fn 14 shows that, whatever one’s sign-choices for the latter digits of the Vatican ms’ Aristarchan expressions, implied precession will still be near 1/century, Hipparchos’ lower limit (Rawlins 2018U §K5), verified by Tithon op cit.

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Data-Fudgery for Myth&Turf

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

Discovery #2: Merging [A] Aristarchos’ Great Year, [B] his M (\(\frac{0}{0}\)A) above, & [C] Meton’s famous lunisolar relation

\[ 235^x = 19^y \]

next arrive at a vindication for the kind of exploratory hypothesizing ([3] if) that can possibly move knowledge ahead: 4868 years of 235/19 each equals Aristarchos’ Metonic Great Year, 1778022\(\frac{1}{5}\) less than his 4868\(\frac{1}{5}\) Great Year. Dividing by 4868 to find the cycle’s yearlength\(\frac{1}{4}\) produces 365\(\frac{1}{4}\)-1/4 = 15/4868, perfectly matching the figure (Discovery #1) independently found [\(\frac{0}{0}\)G2] two decades earlier from Vat. gr. 191 fol. 170v: classic predictive success. Nonetheless, Muffiosi typically refuse to cite (e.g., [\(\frac{0}{0}\)G11 below]) the confirming evidence. Jones even goes out of his way to destroy ([\(\frac{0}{0}\)G8 below) evidence for Aristarchos’ multi-obvious possession (c.280 BC) of a monthlength accurate to a fraction of a time­sec, the better part of a century before Greek-conquered Babylon is known to possess such (c.200 BC) — possibly, just possibly, because pan-Babylonians have made a living pretending that the origin of such wisdom and precision is to be found only in the cuneiform tablets of a plainly inferior, scientifically-unsophisticated and tridgetless civilization (fn 120; \(\frac{0}{0}\) ii [\(\frac{0}{0}\)N13], which their cult has become permanently, undeterredly in-love with?

G6 We can also merge both cycles found from the Vatican ms’s ([\(\frac{0}{0}\)G2]) by noting that 152\(\frac{1}{2}\) is virtually 1/32 of 4868\(\frac{1}{4}\), which allows us to see that [\(\frac{0}{0}\)1976/6/6] that Aristarchos’ Sidereal Great Year is 32\(\frac{1}{2}\) longer than his Kallippic Great Year, thus 1778069\(\frac{1}{2}\). (Heath 1913 p.315’s reconstructed year, showing Aristarchos toyed with a day-rounded exeligmos, is an admirably clever revelation, but not sidereal.) The sidereal year must therefore be 365\(\frac{1}{4}\)-1/4 + 32/4868 = 365\(\frac{1}{4}\)-1/4 + 1/(152+1/8), or about 365\(\frac{1}{4}\)-1/4 + 1/152 (which of course matches \(\frac{0}{0}\)G2’s Var. gr. 381 fol. 163v’s yearlength). [Since 15\(\frac{1}{2}\) + 32\(\frac{1}{2}\) = 47\(\frac{1}{2}\), we see (2017/12/27): by Aristarchos’ Great Year scheme, precession is 47\(\frac{1}{4}\)-4868\(\frac{1}{4}\), roughly a degree per century.] Summarizing: the two continued-fraction solutions we found to be embedded in the Vatican ms’s data, were derived by him from his Metonic Great Year of 1778022\(\frac{1}{2}\) & his Sidereal Great Year of 1778069\(\frac{1}{2}\). The latter’s obvious parallel to the former just adds to the astonishing multiple-vindication\(\frac{1}{2}\) of the two solutions drawn from the Vatican ms.

G7 But pre-knowing that such redundant success is just superficial DIO witchcraft, teamplayer Jones volunteered to confront an awesome challenge:35 how to alter evidence, to undercut a paper that doesn’t, and how to trash into chaos coherent &kantampered inductions which twice accurately extract cycles (4868\(\frac{1}{4}\) & 152\(\frac{1}{2}\)) connected to the very astronomer — Aristarchos — explicitly named in each instance right on the Vatican documents.

G8 Though Rawlins’ math is ineluctably rigorous, subtractor Jones won’t be denied & so nimblly sidewinds to a sly Gordian tactic: erase all accents on the ms (the cripple—triple ploy of fn 88), which automatically, deliberately wrecks the data-basis for Rawlins 1999’s refined, precise, inductive journey from Vatican ms to [1] ancient science’s ingeniously constructed (Rawlins 2002A [\(\frac{0}{0}\)A] & modern induction’s 4-way (fn 88) reconstructed 4868\(\frac{1}{4}\) Great Year [itself from superaccurate M: \(\frac{0}{0}\)G5, www.dio.org/ib11.pdf, eqs.5-7], & [2] Aristarchos’ sidereal year 365\(\frac{1}{4}\)-1/152. How does such holy warfare differ from the warring of Matt. \(\frac{0}{0}\)16:14 for Jesus’ 22\(\frac{1}{2}\) years of this world (query: en-passant: would ancient astronomy historians accept NYU–InstStudAncWorld Director Jones’ deleting all accents from the ms of the Almagest?)

Like-Talibanish is his authoritative justification for across-the-board wiperout: decreeing accents on Greek mathematical data are destructively worthless in HIS expert judgement.

\[ ^{43} \text{See Moesgaard 1983 or Neugebauer 1975 pp.354f.} \]
\[ ^{44} \text{Derivation at Rawlins 2002A, www.dio.org/ib11.pdf, eqs.5-13.} \]

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(Which expert mathematician Jones tends to treat as a kind of evidence. Whenever out of the real kind.) From Islam to Aquinas to the JHA to Isis to the Berkeley vandal-shouters: heretical material is valueless junk, so why not cage or destroy it?

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

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

G11 Though Jones’ paper does not even recognize the existence of DIO’s inductions based-upon the material he’s Talibombing! (\[\frac{0}{0}\]J: cf. Aristotle’s Kallippic Great Year of 4868\(\frac{1}{4}\) — [\(\frac{0}{0}\)G8, prob. Jones’ Kallippic 999/7/14 letter, he was reading the very DIO issue that 1st disseminated DR’s continued-fraction analysis (of the now-Jones-de-accented Vatican ms data) resulting explicitly in Aristarchos’ Metonic yearlength, 365\(\frac{1}{4}\)-1/4 = 15/4868.) Jones was an active listener when Rawlins’ 2001 British Museum talk presented his now-developed series of astronomical-odds, digit-for-digit matches to UNTAMPERED data, etc. 1778022\(\frac{1}{4}\) & the multiple confirmation of 4868\(\frac{1}{4}\) (i.e., [\(\frac{0}{0}\)G5’s Discoveries #1 & #2), the culmination of a century of scientific analysts’ ultimately quadruple-confirmation\(\frac{1}{2}\) of Aristarchos’ 4868\(\frac{1}{4}\) 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 gained from his 2010 paper’s silence on DIO’s Aristarchian numbers (no claim of DIO mis-calculation), even while (fn 85) he defaces the ms data they match.
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-chaunvinist tamperers have, with dedicated persistence & Creativity, tried misprojecting85 onto valid data (above §3C-G) that commit the crime of being inconvenient to prevailing Muffa preconception.

H2 Neugebauer classified the seven “circuli” of Pliny 6.39.211-218 as a primitive “arithmetical”96 scheme, deeming their superficial inaccuracy to be supportive of his contra-reality (fn 8 above) insistence on “the absence of any scientific organization in antiquity”, a conviction which he thought helped (along with his reasoning as covered in fn 1 here) to exonerate accuracy-challenged Ptolemy. But the 1st — and still only available — coherent solution (fn 91 below) ever devised for the circuli indicated they are no more arithmetical than Hipparchos-Strabo’s klimate (which Neugebauer deprecated similarly: fn 18 above), proposing that Pliny’s circuli are instead trigonometric (as are Hipparchos’ klimate) and a case where it is reasonable to test91 for possible ancient data-alteration, since all seven circuli are clearly a version of the traditional Seven Klimata.

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

H4 Rawlins’ 1984 paper (invited for the Greenwich Meridian centenary Longitude Zero Symposium), featuring its 1°-constant-shift solution (fnn 91-93 here) of the circuli, has never been cited by Jones, though his own pale constant-shift ploy for the Hipparchos-Strabo klimate (§§5-10 above) is either suspiciously or parapsychologically similar, with the difference that Rawlins’ constant-shift solution for circuli turns chaos into coherence, while Jones’ constant-shift for klimate is subtractively designed to accomplish the very reverse, leaving such a poor fit to the data that he doesn’t even tabulate them (fn 34 above). Jones mentions Pliny’s circuli but simply calls them “crude”93 (in the Neugebauer tradition, fn 18 above), never telling readers that Rawlins’ restorative-correction procedure — which is just as elementary and trivial as his own failed (fn 34) klimate-echo of it — reveals a trigonometric scheme whose cleverness is crudeness’ antithesis. Nor does Jones’ circuli-puttown tell readers that these restored Pliny data are consistent with (fn 92 here) the very same accurate Diller-discovered 23°2/3 Hipparchos 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 & klimate & calculations & trig-tables & circuli (& Diller & DIO) — thereby obediently according with predecessors’ orthodoxy (just-as-reliably as did just-as-careerist Ptolemy’s “observations”: fn 8), ensuring Jones’ rapid elevation to JHA boardship.

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

I Unmet Challenges — Advances in Understanding Ancient Science Endangered by Hate: JHAD Braves Glummed 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 revealed94 in 2012 that Archimedes’ original unprocessed measure of the Sun expressed its diameter sexagesimally 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 years94.

I2 Scholars have accepted On the Sizes and Distances of the Sun & Moon as by Aristarchos of Samos, namely missing the significance of Archimedes’ accurate contradiction — by a factor of four — of Sizes’ farcical95 fundamental empirical data (2° solar&lunar diameters!), & in spite of its ludicrously contra-reality requirements that: [a] total lunar eclipses last 12° (Neugebauer 1975 p.642), & [b] for Mediterranean observers, the Moon (at distance c.20 Earth-radii) visibly moves in-REVERSE among the fixed stars every day around culmination. Item [b] earns a truly special place in the Bizarrity-Hall-Infame, by the astonishing fact that, again: no one has noticed this for the last 2000 years94.

I3 In the 27th 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’ 1/2-Greek doublings of the Sun’s fictitious half-degree-testimony. From tiny solar declination-motion near solstices, uninformedly sneering (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 years94.

I4 Aristarchos’ famous 87°半月-Moon-elongation wasn’t empirically a precise figure, for moderns (and seemingly Hipparchos: §D2 above) assume, but a lower-limit. From tiny solar declination-motion near solstices, uniformly sneering96 amateur-astronomer-Ptolemaists (incl. MacGenius Swerdlov: §2 fn 8) doubt ancient solstices could

85 Conversely, our tamper-projectors staunchly spurn restoration for either of our cases here where its need is screamingly obvious: Trio A’s 3° eclipse (fn 54 above) and Pliny’s circuli (present chapter). An awesomely perfect psi-missing record for perceiving when data-correction is and is not appropriate, and, as ever, implying that DIO’s simple, neat fits are mere décaree’sorcy.


87 Rawlins 2009S Fig.1 & fn 46-48&50 belatedly weigh indica that Hipparchos himself probably designed the circuli (contra this: one would expect Pliny to have attributed), are more accurate than one might expect from their numbers’ roundness, a familiar ancient circumstance (e.g., §D4[B] above) & see evaluations at Rawlins 2002A §§4&6A11.

88 See Rawlins 1985G, comparing eq.11 vs eq.12, where an ancient scholar’s well-intended but uncomprehending 1°-shift-of-M-data is explained at p.263. All of the circuli shadow data are expressed in feet except for the Rhodos shadow, which is listed as 100 inches. Resolution (ibid n15): 105 in Latin was written “ct”. The “v” was mis-taken by an ancient scribe as an abbreviation for “vnciae” [inches] thus 105 corrupted to “cvnciae” (100 inches). As we now find in Pliny. Once this is realized, and other data are checked, it’s obvious that 105 is the common denominator to all seven shadow ratios (but Rome) & is key to the equation (ibid eq.11) that originally generated the Pliny circuli (ere ancient alteration to eq.12).

89 “Crude”: Jones 2002E fn 11. Or clever? See restored circuli’s smoothly and flawlessly sprocket-tracking curve, graphed in Rawlins 2009S Fig.1, in hollow dots: close proximity to the curve of the data-filled one (Diller-Rawlins klimate-shadow) over the restricted Mediterranean range (much smaller than Diller’s) for which the circuli were designed.
be good to ordmag 1°, which invites equinoxx-vs-solstice accuracy-comparison (§2 §7).

Rawlins 1991H found the Babylonian yearlength on famous Astronomical Cuneiform Text 210 was (§2F) based on well-known Greek solstices (−431/627 & −154/626), the 1° datable transmission of an orbit-element between Babylonian & Greek astronomy, going Greece→Babylon, thereby gutting the Muffia’s holiest tenet (§373&fn 120).

Aristarchos&Achimedes put minimum solar&stellar distances at 10000 &10000 AU, resp, because humans can see (as no historian-of-science seems aware) to c.10/10000 rad. Rawlins 2017E §K2: “For advancing history of science, knowing science matters.”

Pottery’s allegedly-outdoors 4 solar “observations” (132–140 AD: Almajest 3.1&7) averaged over 1° error (not even 1 shot encroached anywhere on the solar disk) and were fifty times nearer Hipparchos’ 3-century-old indoor tables than to outdoor reality (as known for 1200 & F.Ragep 2010 p.121: undeniable but also (for over 300) JHA-un Battled 1360 resp. to null ancient attestation of their speculation, Ptolemites insist (fnn &127) this is because ancient scientists kept only observations agreeing with theory. (I.e., they cheated.)

A. How could Pottery’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 Pottery’s fifty?

C. How could Greeks just copying predecessors achieve their many accurate discoveries? (E.g., Greek vs real ancient monthlengths, Rawlins 2017E §§BB2-B4: startling proximity.) However, most scholars (A.Pannekoek, R.Newton, Y.Maeyama, J.Brandt, & P.Zimmer among the welcome exceptions) reject ancient high scientific accuracy (fnn &35 here), undebatable 2016 subtractor D.Scheglov even (fn 13 here) calling it a “delusion.”

Aristarchos discovered precession 150 before Hipparchos; §G3.

Rawlins’ order-of-magnitude estimates, of too-ununiform or impossible accuracy (§1 §F): 1° for lunisolar diameter; 1% for moon-dance; 1° 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-limb-vs-Sun gap (fn 12 here); 0°1, even 1° for star-vs-eclipsed-Moon gap (§B7); ocular error 1±1° (fn 97 here) for Hipparchos’ mean equinox, with 2° scatter (fn 12); 1° for Earth-circumference-measure precision (fn 108); 10° for lunar eclipse-prediction (fn 97 here); 1° for lunar eclipse observation (Rawlins 1985G pp.258&265); 1°century for mean motion of Mars (and maybe Venus); 1° for all three adopted months (synodic, anomalistic, eclipse), each correct to better than one part in a million. Historians-of-science like Shchevglov 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 Pottery-exculpation-theory.

97 On 1982/7/17, ever-Pottery-worshipping JHA, unable to argue facts or math, merely belatedly excised Pottery’s sensational 50-1 to 1 indoor-vs-outdoor ratio (§18) from a projected Rawlins paper (fn 74 here; precensored text at Rawlins 1999E), along with nonselector outdoor observer Hipparchos’ parallel ratio of just 2 to1 or 3 to1. (Even that tiny ratio reflects just that his tables were founded upon his own slightly imperfect solar data. His UH tables’ eclipse-prediction accuracy was ordmag 10°: Rawlins 1991H eq.32.) The 7° mean error of Hipparchan equinoxes is mostly not due to eyeball inaccuracy: when one removes the effects of his presumed solar parallax and (like fn 70 above) the errors from refraction in the Sun’s zenith distance, and his instrument’s setting-tilt from polestar-refraction, there’s only 1° (±1°) unaccounted-for: see Rawlins 2018U §B4. Mars (and Venus) mean motion accuracy c.1°/century: Rawlins Rawlins 2002V in 26. Contrast to 1° here, and to the umpteenth fruitless metrological analysis of Eratosthenes’ Earth-circumference, Shchevglov 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-proctected by HsS fundamentally non-uncorrection), ultimately undone by his crudely confusing a solar eclipse with lunar and falsely putting Spain into the eastern hemisphere (and China’s Xi’an & Luoyang into the western). Yet another history-of-science journal’s all-too-common Pb-paper duf-science: details of these last Isis disasters can be found here at 1 (§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 subtracting the longitude of the naively 3/4-stretched map of Pottery’s 160 AD GD, the Geographical Directory (often called just Geography or Geography), finding that Greek mean longitude error was well under 1°, thus indicating that ancient scientists had cooperated in creating (as recommended by Hipparchos; Shchevglov 2016 n.7) local times of lunar eclipses at even far-distant sites (fn 105). E.g., the unstretched 42° longitude-difference between Carthage and Persepolis is correct to ordmag 1°. NB: The distorted remains of accurate ancient geography in Pottery’s GD were ruined in two widely separated stages, and in two very different ways (but sharing an asterologist-source in each case): [1] Latitudes were semi-randomly wrecked by Hipparchos (contra Rawlins 2009S fn 18); his discrete tabulation of them (GD 1.4.2) for professional reasons. (See sources here at fn 13 for details; Hipparchos was publishing tables [Tihon 2010’s valued direct revelation] which served 3 astrological traditions simultaneously, sidereal (or anomalistic), Kallippic, & Meteoric, 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 asterologist Pottery’s ignorant stretch of correct eclipse-based longitudes by 7/5 or 4/3 ([§FF&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 (& fn 103 here) that Hipparchos had full spherical trigonometry, see www.dioi.org/cot.htm#tvcc.

114 Rawlins long insisted that the Almajest Mars mean motion was based on ratio 52145/329621. Inspired by Duke’s skepticismism, Jones in 2002 discovered it was based on a different cyclic ratio that Rawlins had carefully neglected, residing, ironically, in the Almajest itself. In the same article, Rawlins gave a similar explanation of Almajest Jupiter mean motion, equally false historically, reflecting even less-exclusively overconfidence. The correct cyclic Jupiter solution was simultaneously realized by Jones and Duke. Rawlins faxed double-congratulations to Jones the hour he heard of the news, announcing them in 2003 on DIO 11.2’s cover, establishing for years a $1000 prize for each overturning of himself (www.dioi.org/pri.htm), thus retroactively sending $2000 to Jones (no reply).

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°1/4 – 1°309, which Tihon was 1° to reveal. 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 Pottery’s; sixteen years later, Ptolemites have yet to produce a collection of such reconstructions based upon the opposite theory, leaving the independent implication that Hipparchos’ catalogue was observed by Hipparchos, later plagiarized by Ptolemy.
In 1982, Rawlins sought the epochs $E$, as well as the errors $x$ in the observers' assumed latitudes $L$, and their standard deviations, for all 4 known ancient stellar-declination observers — Timocharis, Aristyllos, Hipparchos, Ptolemy's anonymous observer — through bivariate least-squares testing upon these observers' star-declination data. In 1994, ex-

100 Rawlins 1994L §F3-F8, where nulls show Timocharis (known observations c. −300-271), earliest of Alexandria's 3 observers of extant star declinations by surveying instrument, alone knew his exact $L$. Later studies get virtually the same $E$ for each observer. Mayeama 1984 finds thisly, 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 confining 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/2014B/pdf, says most of Rawlins' geographical latitudes $L$ of 0.8 ± 0.4 "come to our values". But their $L$ are merely assumed, so DIO's JAHH-requested 2014/08/26 referee report, www.dioi.org/jahh3q.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., in placing Ptolemy's standard-deviation (computed at www.dioi.org/bjx0.pdf, p.5) to 0.5 instead of DIO's 0.4. 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 Swedlow 2010 p.151). $L = 30°58'$ (archaeoastronomy 3.5; above, in fn 25), erroneous by −14', which obviously is much-too-large for a regular outdoor observer, and also is in 17° conflict with latitude $L = 31°15'$ which is indicated by (nulls to be) the virtually-correct value that was adopted by the stars' actual observer. Brandt et al 2014B's standard deviations $\sigma$ were allegedly calculated bivariate; 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 ordm, as referee-specified. NB: Had Brandt et al. 2014B, finding $L$'s $\sigma$ first, and then again meaned leftover residuals to get the other variable's ($E$'s) "accuracy" similarly, the result would have been informationless zero! (See §C11.) There are further oddities. Ibid's eq.1 sets $O-C$ equal to C. The paper's $O-C$ graphs are rendered C-O. Two strange JAHH attempts to undercut skepticism by splitting Ptolemy's 18 stars differently from R.Newton (and, by-the-way: Ptolemy and Pannekoek and DR — C16 below) find those low-recently-announced estimates equal to original errors, re which three journals have been ordmag [258] than their own behavior. None of these three & fnn 4&97) encourages non-closeted investigation of DIO's accuracy or of their own behavior. None of its receipt by journals instead published solutions seriously mistaking in part (due not to invalid methods). Coincidentally, their authors defended Ptolemy, while Rawlins insisted on citing long-journal-suppressed evidence he didn't observe his suspect Almajest 7.3 declination data, namely: adopted-$L$'s clash with reality AND with these declinations (see just above, in this note, evidence crucial to the controversy, known for forty years, but (as also in fn 97) never-ever found in centrist journals. Same for Ptolemy's solar observations' 50fold closer adherence to old tables than to the real sky, re which three journals have severed correspondence (1983-2016) rather than print. In 1983, JHA lawsuit-threateningly cut contact over the 50-factor issue (Rawlins 1991W §B; 1991; DIO 6 §3 I, 1996; Rawlins 1999 §F-E). In 2015, faced with DR's request to print the same 50-ration, likewise-cornered JAHH unilaterally ended contact with DIO, www.dioi.org/owwl.pdf, and in 2016, as we see from www.dioi.org/jshf.pdf, Isis Editor H.F.Cohen suppressed the same 50-revelation by cutting all correspondence — including in regard to DIO 2015-2017 unb done. done a priori and, on the contrary, their own behavior. None of Isis' bunker editors' emails on what ultimately became www.dioi.org/qjo.doc, nor Isis' 2017 Match "referee report" ever mentioned the paper's history or science or mis-math or factual errors. Not what really matters anymore in history-of-science. (After demanding the paper's muting, Cohen finally sent an earlier unedited version to his referee, ensuring the negative verdict he sought.)

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

<table>
<thead>
<tr>
<th>Observr</th>
<th>$E \pm \sigma_E$</th>
<th>Adopted $L$</th>
<th>Its Error $x$</th>
<th>Actual $L \pm \sigma_L$</th>
<th>$\sigma_0$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Timoch</td>
<td>−294±11°</td>
<td>31°12’</td>
<td>−1°8±2’.7</td>
<td>31°13’8±2’.2</td>
<td>±9°0’</td>
</tr>
<tr>
<td>Aristyll</td>
<td>−258±10°</td>
<td>31°15’</td>
<td>+1°±0’.7</td>
<td>31°14’0±0’.2</td>
<td>±6’1’</td>
</tr>
<tr>
<td>Hipp</td>
<td>−131±05°</td>
<td>36°08’</td>
<td>+0’2±1’.2</td>
<td>36°07’8±1’.2</td>
<td>±5’2’</td>
</tr>
<tr>
<td>Anon</td>
<td>+159±09°</td>
<td>31°15’</td>
<td>+4’4±2’.0</td>
<td>31°10’6±2’.0</td>
<td>±6’2’</td>
</tr>
</tbody>
</table>

In 1994, ex-
1982, it was shown that Eratosthenes’ original Earth-circumference C was neither of the long-accepted (variously rounded) stade-values, 25000 or 25200, but instead was 256000. In 2008, 26 later, all 3 were checked against Eusebius’ long-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.)

It was simultaneously found that: Earth-radius \( r \) was the empirically primary datum, consistent with being based on Sostratos’-independent Pharos method \((\S 124)\), which directly finds \( r \) from his Pharos Island lighthouse (in Alexandria harbor), built near Sostratos-Eratosthenes’ time and place. Again, our deliberately silent (\S[2]2&126) JHAD-center — which accepts no discovery if not from its own network — must regard the temporal\&spatial coincidences as completely without significance. (Like unrecognized confluence in another sphere: www.dioi.org/shg.pdf, “Kit Marlowe’s Perfect Crime” \( G.3.\))

Proposed Sostratos ingenious idea\&achievement: mount Pharaoh’s flame precisely \( h = 300 \) feet or half a stade above sealvel, so the apt equation, \( r = \sqrt{2+2h} \) becomes, thus \( r = \sqrt{2} \) stades; can be found by just squaring the flame’s coastal overwater visibility-distance \( v \) in stades. At first the trick seems suspiciously overeas & dimensionally impossible. But it works. Note that squaring 202 stades, and rounding conventionally, yields Eusebius’ \( r = 40800 \) stades (\( \S 122 \)): a 3-to-1-unlikely chance-hit (Rawlins 2008Q \( J.1 \); Rawlins 2018V).

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 Poseidionios’ \( C = 180000 \) stades, the other anciently standard Earth-size, is exactly 5/6 too low. It is an Occam\-DIO dream-come-true to perceive that since horizontal light rays’ curvature = 1/6 Earth’s, the atmospheric refraction would cause observed errors in C of 6/5 and 5/6, respectively, for two simple, clever, low-physical-labor never-leave-home methods \(^{180}\) of measuring the Earth: the Pharos-flame method (6/5) virtually attested by Pliny 2.65;164; and the also physically-easy (& obvious?) double-sunset method (5/6). So DIO’s refraction theory at once satisfies both Sostratos&Eratosthenes&Hipparchos’ \( C \) (6/5 high), as well as

\[ C = 256000 \text{ stades} \]

\[ \text{induced from a Nile map's latitude intervals: Rawlins 1982pp.212, 214, \protect\text{216}-217; Rawlins 1985Gp.229; Thurston 2002p.66. For which } C \text{ fits Eusebius’ } r = 40800 \text{ stades: just multiply } r \text{ by } \sqrt{2} \text{.} \]

\[ \text{Rawlins 2008Q eqs.2&128; eqs.14-15&17-18 for } r \text{ as Sostratos' direct empirical measure.} \]

\( \text{See ibid } \S A \text{ for the Pharos-flame method and Pharaoh’s semi-attestation of it. Double-sunset method: Rawlins 1979 or Scientific American 1979 May. Interval between times of sunsets seen from Pharos’ toptopbehind exceeded a time-minute, unmissably-enormous alert and gauge of } C \text{'s size. (Elementary illustration-by-extremes that different results ensue for flame vs sunsets: Rawlins 1979v} \text{1A5).} \]

\[ \text{To call this needing direct atmospheric photographic* raw data cited here, the foregoing utterly original\&successful atmospheric-refraction theory — tri-nearly solving the INTERMINABLY-continued ancient Earthsize mystery — cannot legitimately be ignored. But it is: } \]

\[ \text{fn 111 below. Unbelievably worse: } \text{fn 109! One recalls not only JHAD shunning of Diller (fn 25), but also the case of L. Boltzmann’s kinetic theory of gases, which E.Mach \& others spurned because (though theory neatly fit evidence) no one had ever seen a molecule. Did this trigger Boltzmann’s 1906 suicide (just one more vindication by Wilson-cloudchamber)? We don’t know. What we know is: certain polls cited hereabouts wouldn’t care. Past perhaps praying for history to repeat. Ever so vainly.} \]

180 Rawlins 1985Gp.265, taking an idea due to (ibid n.22) Gosselin 1790, suggests an ancient, adjusting for the 700 stades\(-10 \)\(-500 \) stades\(-15 \) switchover, switched lengths by 7/5, mis-assuming they were based on land-surveys (\( \S 1 \)). Or by 4/3 via Poseidionios’ \( C = 240000 \) stades\( -180000 \) stades. Proposing Greeks organized to compare eclipse observations (fn 13): Rawlins loci cit, vs fn 90 above & consistency with the Verde Isles, westernmost known land, chosen as Marinos’ \( 2 \) longitude (www.dioi.org/501.pdf, sf A5), to kill longitude sign-muffs like those (fn 97) cursing Shcheglov 2016. [Speculation 2018/7/15. Marinos = pseudonym, like “Philo?”. Or map-title from maritime Tyre?]}

181 Rawlins 1985Gp.229; Thurston 2002p.66. For which \( C \text{ fits Eusebius’ } r = 40800 \text{ stades: just multiply } r \text{ by } \sqrt{2} \text{.} \]

182 Rawlins 2008Q pp.3&211&218; eqs.14-15&17-18 for \( r \) as Sostratos’ direct empirical measure.
Yet the right solution is: [1] directly for radius (fn 107), not circumference; [2] geographical ([J32], not at all astronomical; [3] physical ([J36], not meteorological.

127 But whence arose the lincipthin 185m stade? Before imperial standardization, stades varied ordmag 10% from locale to locale, the smaller among the early ones now naively, selectively, anachronistically used by Eratosthenes’ mod-groupies to rig right-on correctness for his too-big C. In 2014, it was seen for the 1st time that the much-attested (fn 111) early 3rd century BC Greek rule of dividing terrestrial meridians into 60 parts (not 360) — step-one of C’s potential sexagesimalization, conventional Greek fraction-practice — could’ve led naturally to the Ptolemaic empire’s regularization of the “stade” by defining it, parallel to our definitions of meter and nautical mile (fn 111), as C/60/60/60 = 40000000m/216000 = 185m. This is the best — the only — defensible scientific practice explaining modern-consensus-185m’s Greek origin & durable adoption, which survived even eventual 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 Ptoleomy 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 Meroe’s latitude (nowhere interrupted by the Nile or sharp mountains): 1578 km = (in 60°) 23/8 = 1/4 + 8550 stades at 600 stades/degree, or nearly 10000 stades (Strabo 2.5.7, 17.3.1: fn 111 here) at later-standard 700 stades/degree? — accurate to ordmag 1’ or 1 nautical mile. Was the hypothetical survey supervised by contemporary scientist Shimabukuro, demonstrably (fn 99) in 1° accuracy latitude-fix via ringed instruments? 129 For over 100°, at least from H.Berger, scholars have wondered if the early 300000 stades Earth circumference C, cited in Archimedes’ Sandreckoner, was due to Dikaearchus (c. —300). In 1994, DIO showed that if Dikaearchus measured sea-horizon dip from atop conveniently-seaside Mt.Pelion accurately (1°1/100) & computed C from it, then his over-estimate of Pelion’s height as 10 stades would (in ignorance of quantifiable atmospheric refraction) have produced C = 300000 stades within ordmag 1%.

130 DIO produced hitherto-unperceived & thitherto-unouched physical evidence that the Galactic Equator appeared on ancient Greek celestial globes: www.dio.org/th.fl.f.htm#phd.

131 Kallippos’ 329/6/28 Summer Solstice was his famous calendar’s epoch. Modern discovery of the event’s hitherto unknown hour unexpectedly happened in connexion with 1985 realization that Kallippos’ 365¼1/4 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 fn J4), thus —431/6/27 3/4. So, adding 102 Kallippos years, or 37255¼1/2, that to 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 epoch for his lunisolar calendar. Kallippos induced his yearlength from division of 37255¼1/2 by 102, finding (as it happened) exactly 365¼1/4 days. His solar motion was codified into his famous Kallippos 76-year cycle of four 6940 Metonic 19° cycles minus 1, that is, 365¼1/4 per year. Due to Meton’s 17-truncating his own Solstice’s ±3° error (interval’s net error +2°), he accidentally arrived at his 1°1/2 Julian solar, nearly 3 centuries before Caesar’s Sosigeanes.

132 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, y = 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 235/199.

111 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 [J10]). The ad hoc nature of the durably mythic “Eratosthenes” stade is obvious to most specialists, e.g., P.Gosselin, E.Bunbury, D.Dicks, O.Neugebauer, J.Berggren, A.Jones (more at [J2]). The 185m is more: i) indubitably the true short from a poor solution, but waited for a valid one to come along. So far so good. But now that Dio’s airbred theory is here, no historian-of-science is claiming the math doesn’t work. Or that anything works better. Has the-catatonia got the JHAD-tongue? [The dwarf-stade myth is efficiently, consistently, bluntly, and utterly evaporated by Engels 1985 p.309. Sexagesimally-defined stade: Rawlins 2012T fn 2, self-contradicting the titular contention of Rawlins 2008Q & note ibid /A4[a] that early-Ptolemic survey-based Earth-circumference determination was just legend. This can be seen as showing DR’s poor judgement. Or desire to learn. Or both.]

112 Dutka 1993 p.64 cites Rawlins 1982N — whose App.A explicitly links 6/5 to lighthouse and 5/6 to sunsets — without ([J26] citing the paper’s atmospheric refraction theory that explains these felicitous fits to the 2 respective ancient C-values at issue, & with no sign whatever of understanding the paper’s physics. Strabo’s arcs (where Earth-curvature is up to a meridian circle of circumference 39870000m): 5000 stades Alexandria-Aswan & Aswan-Meroe, each good to ordmag 1’ for 700 stades/degree: 7°+1/8 + 7°1/8 = 14°1/4. (Rawlins 2009S C3 notes Philo’s solar work at Meroe, presumably for an imperial survey.) Testimony for early-Ptolemaic meridians in 60th: Strabo 2.5.7 (Eratosthenes): also Geminos, etc.: Neugebauer 1975 pp.590 (n.2), 733, & 1364 (Fig.43). Is a Ptolemy I survey’s memory embedded in Kleomedes 1.10’s famous legend? (Rawlins 2008Q /A4[a].) Dissenho 1950 pp.230-251, cites 5 ordmag-10%-dispersing Greek stadion’s stade-long race-courses. (Shcheglov 2016 proves tacs more indubitably.) The only post-Ptolemy-I course is also the 185m one. The ancient stade was 1/8 of a Roman mile (1480m): Engels 1985 p.308. Updated compendium of ancient Earth C-values, in stades: Aristote 400000, Dikarchoi (??) 300000, Timochior (??) 216000, Sostratos-Eratosthenes 256000, and Poseidonios-GD 180000. Correct circumference C = 216000. (Meter = C/410/10/10/10/10/1010). Nautical mile = 1852m = C/725600 = C/216000 = 5/4 of the Roman mile. Further speculation on the pharaonic stade’s history is found at www.dio.org/th.fl.f.htm#ch. Relating attested meridian-60th to 185m is another JHAD-uncited completely original Dio revelation.

I33 Sourcing Ptolemy’s final lunisolar ratio.¹¹⁴ 105416⁰ = 8523⁰, occurred 2 decades ago (all 10¹ digits exactly elicited) by test-exploring Greek awareness of the 80⁰ sidereal eclipse-cycle (1/5 of 80⁰ cycle attested: Geminos 8.40-41): solution, awareness, & nest not suspected ere Rawlins 1996C eq.31. (Sidereal year: ibid fn 110.) Royal Mufia Cavilliers have produced no math error or alternate solution since. Predictable result (see Rawlins 1996C’s title and boxed 2013 statement atop its p.2): permanent silence.

I34 More muteness greeted DIO’s 2002-2003 discovery that all 3 previously unsolved, adoptedly 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 ratios. To emphasize the precision and the breadth of this achievement, we display the just the way Ptolemy at Almajest 4.2&6.9 explains determining lines synodic, anomalistic, & draconitic. Notably, no matter where, over a 40⁰ span (3⁰ century BC to 2⁰ century AD), the pairs’ latter eclipses are located in time, all the prior ratio-solving eclipses turn out to be from the very same century, the thirteenth BC (§136). One might suppose the center’s largely old-guard pan-Babylonianists, would welcome the prospect that such remarkable Greek triple-accuracy could have a fundamental & irreplaceable debt to Babylon and would delight in the potential new vistas opened by these astonishingly exactly matches. Instead, the entire history-of-ancient-astronomy shunniness, frustrated by inability (like §133) to find math error or alternate eclipses to show non-uniciness, 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-cult¹¹⁶ scholarship by Johannes Koh who had already (earlier) estimated Babylonian observations’ massence as about = 1350? Surprise realized that Hipparchos’ famous 600¹ lunisolar tables effectively went back just that far only occurred in 1715.

I35 So we should gauge the proposed eclipse-ratio method by comparing it to what may be verified loosely referred to as “the competition” (e.g., fn 119). Facts: No other method is attested. (Twice: idem.) No other method is so simple & immediately-direct-to-the-result. No other method could ensure such high accuracy, 1-part-in-1⁰⁶, 3 times out of 3, eliminating false near period-ratios (§137). No other method than eclipse-period integral ratios so naturally accounts for why all said motions were expressed as integral ratios. No other method explains the 4-digit size of each ratio’s 2 components: as in Almajest 4.2&6.9.¹²⁰ No other credible (fn 119) method, attested (or unattested) has math-generated ANY of the numbers sought, while DIO’s proposal has done so for ALL 24 digits precisely — that is, all six 4-digit components — on-the-nose in each case: see www.dioi.org/thr.htm#cqtp.

¹¹³ PionHyp 1.16 (Heiberg 1907 pp.78-79 or Neugebauer 1975 p.901 eq.3); 105416⁰ = 8523⁰. Solved: Rawlins 1996C eqs.20-31. Thanks to K.Moesta for a persuasive correction.

¹¹⁴ JHA. The admirable exception to Hist.sci ignoring ancient monthlength accuracy: Pedersen 1974 pp.164-424. But he does not realize how such accuracy was achieved, nor does he go on to challenge the anti-empirical orthodoxy we saw at fn 8.

¹¹⁵ Moesta 1992 p.474. Initial Mufia tactic vs R.Newton & DIO was non-citation. But Isis Editor Margaret Rossier’s publishing DIO-respecting Thurston 2002S defied the 30⁰ shun, inspiring (what else from patological unregenerate?) DOUBLESHUN: [a] Thurston’s swift ever-exile from JHA (www.dioi.org/pm3.htm); [b] DIO-citations’ end in AAS-HAD’s Newsletter & [c] Isis’ Cumulative Bibliography; [d] during Thurston 2002S’s refereeing (2000), the usual indiscriminate (fn 66) unfettered anti-books for launch. Schaefler 2001 (Pb), Schaefler 2002, Jones 2002E (2nd to 3rd) to Ph, Duke 2005T; Duke 2005W (Ph), Jones 2010B (2nd to 3rd), as well as outdid each other to squash #1 blackballei no matter how: §§3R-B, all now on JHA’s certified-Premier (fn 42) board. Re JHA’s prior villain, we quote from fn 28: “Rewards handed out to those who attacked the R.Newton satan include JHA boardship (R.Newton 1991 fn 2) & a MacArthur for miss-man [fn 96 here] Swerdlov. (It’s hard to find good help anymore.) . . . maid-men Evans & Schaefler were elevated at JHA not long after their massive bungled 1990/2001-2002 attacks on Rawlins. (The unsubtle here may actually be deliberate.) Selecting boardmembers [thlusly] will damage mean-IQ at JHA for decades to come.”

¹¹⁶ For this recent shock, see www.dioi.org/thr.htm#brkv. Re Hipparchos’ 600¹ tables, see Pliny 2.9,53.

I36 To emphasize the precision and the breadth of this achievement, we display the three anciently-adopted lunar speeds DIO has mathematically traced to hugely-separated eclipse-pairs, all starting around the 13⁰ century BC (details www.dioi.org/thr.htm#cqtp):

- 1291/13/23 vs -280/11/16 & -1273/12/05 vs -262/11/26 → 624⁰⁷ = 669⁰⁵ (System A),
- 1244/11/13 vs -140/1/27 → 545⁰⁸ = 592⁰³ (Hipparchos),
- 1200/07/11 vs -1189/06/12 vs 136/3/06 → 327⁰⁷ = 351⁰² (PlanHyp).


I37 Again: mere integral division is DIO’s twice-ATTTESTED eclipse-cycle “method” (too fancy a term?) of exactly reproducing all 24 digits. In the 1 1/2 decades since these super-simple DIO solutions 2002-2003 debut: no historian-of-science has publicly engaged a single one’s science. Nothing beyond a rigid clique’s continued traditional insistence on its vaporous theory that UNATTTESTED laborious Babylonian analysis of poor lunar horizon¹¹⁸ data couldamusta produced such accuracy — if only enough¹¹⁹ data were averaged! (This bizarre notion came inevitably out of the Neugebauer-Babylonianist cult, ever-clinging baselessly¹²⁰ to its sacred tenet that Babylon gave rise to high Greek astronomy.) Naturally, no numbers are provided¹²¹ to show how such a fantastic reconstruction could: produce HYPER-accurate results, or [b] find the draconitic month at all,¹²² or [c] distinguish....
among almost-as-accurate proximate ratios (www.dioi.org/thr.htm#epc), or [d] explain why each solution emerges as a ratio, a glaring feature of ALL pre-Ptolemy adopted lunar motions, which by contrast to orthodoxy is accounted-for perfectly inevitably by the eclipse-period-ratio solution. Jones’ blinded private rejection (by subsequently-undercut reasoning) of the DIO consonant solution, flies all of the overkill-numerous, solid, unabiguos 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 – 140/27 eclipse, the very same perigee eclipse which he uniquely had also previously (Almajest 6.9) paired with a less early apogee eclipse (– 719/38) for exactly the same draconitic purpose, with inferior result — inevitably, due to shorter timeframe.

Comments: In all history, no astronomer before Hipparchos ever used an apogee-perigee eclipse-pair. Scoffins at the theory’s outrageousness-vs-orthodox-preconception inadvertantly compliment it by reflecting its potential advance’s enormity. I38 §136 presents three perfectly fitting 2002-2003 eclipse-solutions to Greek adopted monthlengths. Why shummers’ 15°-impointence in finding DR-errors? Or alternate solutions? Answer (§134): there IS no other umbral lunar eclipse-pair whose integral-months ratio precisely, proportionally, directly yields (by Almajest 4.2&6.9 method) any of §136’s three attested, never-before-solved Greek integer-ratio motions, with: both eclipses visible in Greek-Babylonian region, latter eclipse within 50° of discovery-date (c. – 262, – 140, + 136, resp), earlier eclipse not ere Almajest 4.6’s – 720/19 (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: $10000 for 3277°/3512°; $20000 for 6247°/6695°; $30000 for 5458°/5923°; $40000 extra for all 3 relations. [To certify the bet, DIO will deposit $50000 with the National Academy of Sciences, if it will hold same for winners until time’s up.]

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

J1 The foregoing suggests shortcomings in the modern ancient astronomy subfield re: [a] Scrupulous & neutral refereeing. [b] Turfless generosity & citational integrity. [c] Open-minded curiosity and tolerance (Hoskin, Evans, Jones, and Toomer have fled contact with Rawlins for years, e.g., Thorston 1998D fn 2) without a professional-survival priority — nay, necessity — of treating archons with an attitude of nondissent, even supplication, to allay (non-imaginary) fear of being unpublished or outright blackballed, as if such etiquette-issues outrank (§145) academic skills & integrity. (And o-yes accurate history.) [d] Scientific skills (or regular consultation with able scientists), & especially the purest scientists’ attitude of humble subservience to evidence (acquired by careers of frequent empirical contradiction). [e] Celerity of incorporation of knowledge-advancements (vs fn 20 here: 84 years?!) that will minimize historians-of-science’s capacity to grasp the empiricism & brilliance of Hellenistic science. [f] Essential, genuine neutrality and curiosity (hardly compatible with a cult’s insistence on aggressively protecting sacred viewpoints for decades on end), enhanced by willingness to hypothesis-expose — ever subject to evidence-congruity (e.g., fn 16&83 vs fn 20&25&40). [g] Philosophical & science-common-sense Occamite

weighing of competing theories by such criteria as simplicity (§34 above), probability’s relation to confirmation (§2 [N15], minimal hypotheses (§D & fn 122), fruitfulness (§C3[b], E, & F), predictivity (§F & G). Instead of by herd-grantmanship.

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

125 Jihad-shunning (longstanding: above, fn 109, & Rawlins 1991W fn 171&173 of Diller, Newton, & DIO is known to all in the JHAD-circle, resorted-to from careerists’ fear that honest critics are simply bad-for-business.Organizing such disgusting cultist behavior (which works by influence [e.g., fn 116], not ESP) is a disgrace to academia. Like marriage, shunning is easy to commit, hard to end (§2 fn 34); usually in scientific intellectual rivalries and fields so dependent on intellectual rejigging, it is (among other ethical lapses: fn 10) he continues decades of ducking explanation and exposing pretensions are worse crimes than the counter-crimes of smearing, shunning, deceiving, stealing, doctoral censoring, en route to effecting decades of knowledge-subtraction. The exiling entity commits to the banned’s worthlessness without anticipation of concomitant risk: what if the shunnee then produces valuable knowledge? The shunner can never admit banishing valid offenses against ethical scholarship. So either [a] he loses faces by unshunningly owning to a mistake or [b] omerta-fakes (e.g., fn 66) the existe’s vindicated work as being just as worthless as Infallibly decreed at the outset & bars non-denigrating citation. How many image-protective archons ever chose option [a]? [e] Celerity of incorporation of knowledge-advancements (vs fn 20 here: 84 years?!) that will minimize historians-of-science’s capacity to grasp the empiricism & brilliance of Hellenistic science. [f] Essential, genuine neutrality and curiosity (hardly compatible with a cult’s insistence on aggressively protecting sacred viewpoints for decades on end), enhanced by willingness to hypothesis-expose — ever subject to evidence-congruity (e.g., fn 16&83 vs fn 20&25&40). [g] Philosophical & science-common-sense Occamite

theory of fabrication explains both and to the Almajest’s 1st precision. Therefore, in each case (lunar or solar): which approach would Occam prefer?
stand indefinitely-uncorrected his own miscalculated-backfired evidence, not retracting the slander of Greek science it was adduced for, in everlasting 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-inadequacy’s untested-but-alleged consistency with allegedly-normal science (fn 8; also Evans 1998 p.209) that allegedly-retained only theory-fitting data: i.e., forcing all Greek astronomy to fit a faker-astrologer; then, pre-post-erously, with Ptolemaic evidential circularity, using that very model to prove his normalcy, a truth-inversion warp that’s ruled the field for most of a century.
#4 Ptolemy Enormity

Ptolemy-Defense Cult Lays Yet ANOTHER Egg On Own Already-Unwipesably-Eggregious Faces

ArchonBishop of TruthBury's Trowel & Slander

We now analyse the latest installment, this time from the Journal of Astronomical History & Heritage 2014, in a half-century serial display of unfailingly invalid archonally apology-defenses of Claudius Ptolemy, sacrosant aristocrat-astrologer of the American Astronomical Society and its HAD: author of astrology's bible, the Tetrabiblos; science-fabricator; & Almajest-perpetrator. The spectacular result has been one of history's grandest compilations of establishment pseudoscience literature, all accomplished in the service of attempting to save the reputation of a 'scientist' icon who was not a scientist at all, but (§A below) just a mathematician who faked science. Badly. Among Ptolemy's numerous clumsy Almajestis (the silliest on scintillating display here in [2] were, e.g., [a] a 2 different dates (37 days apart!) for the same Venus maximum-elongation, [b] plagiarizing Hipparchos' star catalog, [c] impossible-for-regular-observer ignorance of his own city's latitude, [d] 4 alleged solar ‘observations’ which were (as no historian-of-science denies; or admits) many times nearer Hipparchos’ old indoor tables than to the outdoor sky’s actual Sun. The JAHH paper in question, J.Brandt, P.Zimmer, & P.Jones (below known as Brandt et al 2014B), attempted mathematical analyses of the Almajest's stellar declinations, observed by four successive ancient Greek astronomers over nearly half a millennium. The journal and authors contend that the dozen Almajest-contemporary declinations could be Ptolemy’s observations, never warning the reader that zero evidence is provided to establish that claim, while simple, definitive, long-published, referee-urged proof to the contrary is below shown (§C5) to have been deliberately omitted. The paper’s “bivariate least-squares” statistical analyses were not bivariate and thus didn’t exactly find any least-squares residual-squares, as is also demonstrated below (§C23). While observers’ epochs E are nearly right (but not new), attempts to find their geographical latitude-errors x are revealed as grossly misguided, at a primitive level (§§C9&C12), though referee DR provided, ahead of publication, accurate x (and standard deviations) for all four of the ancient astronomers being analysed, solutions which could’ve been (but weren’t) crudely verified by elementary arithmetic, as will be shown here (fn 34 or §C23). Our discussion's bluntness derives from the fact that, though Brandt et al 2014B is politely written, its knowing evidential omissions cooperate in trying to grant eternal life to an establishment myth — Ptolemy as Great Out-Door Astronomer — that rolls on, decade after decade, persisting only because the American Astronomical Society doesn’t care that its Historical Astronomy Division is deeply invested in a pathetically obvious historical lie, viciously (fn 4) defended by those JHAD archons who long ago mistakenly decreed Ptolemy “The Greatest Astronomer of Antiquity” and thus have faces so at risk of megga-eggitudinal disgrace that they must forever encourage pseudo-science-for-The-Cause of forever-pseudocontroversy, cult-obediently incapable of admitting that any skeptic has ever made an indubitable contribution to knowledge. Below, at §B, the most recent misfire (Brandt et al 2014B) is put into the context of decades of like uniformly baseless mobaganda (though those interested only in 2014’s mistash 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.

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

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, fn 52) risking rational debate with DR, written & signed by Gingerich & Swerdlow (fnn 4&18, C23). Our discussion’s bluntness derives from the extent of such ultra-elite Level-3 suppression of dissent. Nobody disputes the 50-to-1 indictment. But no Reputable Forum (including even popmags, newsrags, & toobs) dares broadcast such heresy-supportive truth, either. The rigid decade-after-decade smearing of dissent 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.  

3 Prime smear of 2014B is politely written, its knowing evidential omissions cooperate in trying to grant eternal life to an establishment myth — Ptolemy as Great Out-Door Astronomer — that rolls on, decade after decade, persisting only because the American Astronomical Society doesn’t care that its Historical Astronomy Division is deeply invested in a pathetically obvious historical lie, viciously (fn 4) defended by those JHAD archons who long ago mistakenly decreed Ptolemy “The Greatest Astronomer of Antiquity” and thus have faces so at risk of megga-eggitudinal disgrace that they must forever encourage pseudo-science-for-The-Cause of forever-pseudocontroversy, cult-obediently incapable of admitting that any skeptic has ever made an indubitable contribution to knowledge. Below, at §B, the most recent misfire (Brandt et al 2014B) is put into the context of decades of like uniformly baseless mobaganda (though those interested only in 2014’s mistash 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.

Text for the Day:

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

You are protected by the enormity of your stupidity.

The point might be kept in mind by observers of the decades-ongoing spectacle of the history-of-ancient-astronomy field, whose most eminent journal and most prominent society — in tandem with a MacArthur Fellow and a Harvard prof serving as untruthful proponents (fn 18) of the slander that no-one but a CRAZY2 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 unendably but captive-journal-unprintably 50 times closer2 to Hipparchos’ 280°-old indoor tables than to the real Sun. The prime forums perpetrating this fantastical 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 knowingly3 unsupervised Historical Astronomy Division (HAD) — which we shall refer to as the “JHAD” combine. Sieemingly incredible fact of the last 4 decades of the Ptolemy Controversy: not a single published defense of Ptolemy has ever been valid and most have not been particularly smart or honest,4 as we are about to see again & again below. But as with oft-crafty Rains, the papers are aimed at making it possible for the very unimportant literature to be seen as real even that such ultra-elite forums and scholars could seem so stupid. (Also invisibly back-stabbing, slanderous, and 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.

4 DR has asked AAS to monitor HAD’s “unprofessional” (Schaefer 2002 p.40) — before, since his death, honoring DR by elevating him onto the same pedestal.
The Shy Archon Triggering the Present Paper: Politics vs Science

A1 In 2011, DR belatedly2 responded to much-decorated astronomer Jack Brandt’s welcome request to consult a 1982 unpublished DR ms on the 54 star declinations observed by ancient astronomers Timocharis, Aristyllos, Hipparchos, and (allegedly) Ptolemy — reported and analysed at Almagest 7.3. In 2014, much-too-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 far so along towards publication that serious changes appear in retrospect not to have been feasible at the late date of JAHH’s request. (Not the 1st time [e.g., Rawlins 2008 fn 42] Ptolemites have asked skeptics to help them avoid blunders, even while undeterredly determined to promote more cultism.) And, indeed, no changes were made, in response to central points challenged by DR’s scientifically detailed 2014/8/26 referee report, www.dioi.org/jauqg.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 provide as much evidence and competent articles possible. (Not exactly an infectious ideal at brother history-of-astonomy 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 culthiness and non-bravery) less from iniquity than from JIAHish inability (increasingly typical of the whole ever-less-scientifically-skilled history-of-astonomy field) even to begin to tell balanced, competent technical research from cultist apologia.

A2 Brandt et al 2014B p.332 claim that the 2nd century AD star-data of Almagest 7.3 “could have been taken by Ptolemy himself.” The evidence for this politically-convenient falsehood? Ptolemy was alive when they were recorded! — a fact which did not require a new article for bringing, since it’s been published for decades (at least) and has never been in dispute. No other evidence is brought forth favoring the claim, because there isn’t any supportive data whatever — all relevant evidences on the point are to the contrary (§C5 below). These were imparted to JAHH but never entered into its paper, which instead took seriously Ptolemy as observer, and promoted a fact-immune3 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 freshly submitted a paper, “Ptolemy’s Fraudulence” (§2 above), to the JAHH, whose chief, W.Orchiston (formerly established in Oztrollya, like JAHH, but lately transplanted to Thailand) turned it over not to a specialist in the relevant science, to DDS’s email address. Which is

2 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/bjz3g.pdf, accompanied by the requested 1982 ms. Brandt certainly deserves credit for updating star-data (fn 40) and for stimulating DR’s 2011 discovery (§C21), which everyone including DR had missed right along (even though Rawlins 1994L had already concluded that +159 was the Clean Dozen’s epoch): for epoch +159 (unlike for +137) the split between Clean Dozen & SickSix stars was overalpless clean: §C17. (But Brandt et al 2014B didn’t cite any of this.)

3 As we mourn the passing of technically able contributors to scientific history such as B.L.van der Waerden, C.Gillispie, W.Hartner, O.Neugebauer, C.Wilson, H.Thurston, A.Aaboe, R.Newton, & S.Goldstein, we realize that they are being replaced (as JHU’s Harry Woolf warned DR 50’s ago) largely by non-scientists. The new breed has proven admirably industrious, but too-often inadequately trained in science’s skills, criteria, standards, principles, and especially approach to evidence.

4 See below at, e.g., §B4.

but to a fellow politician, who despite “careful” reading could come up with no errors of science or history — or anything else — and thus (in stark contrast to DR’s ref report), offered no scientific guidance at all (unless one delusionally regards shrinkoanalysis4 as science), instead — even while acknowledging that DR is “clearly quite knowledgeable in the astronomical history involved” — insisting on removal of anything embarrassing to his clique, adding gratuitous psychological evaluations5 including accusing DR of an “apparent need to disparage those with different views”. This from a cult which has for a half-century repeatedly (and reliably-always behind-the-back) smeared, as insane, anyone differing from its own reality-detached view of Ptolemy — a genuine, mentally-dissabling insanity which The Leader is now at the last almost alone-in-the-bunker with, outside of (publicly) loyal J.Evans, J.Brandt, & possibly B.Schaefer. (Ptolemy’s many well-known doubters — their consensus not at all well-known — are extensively listed here at §2 fn 1, though JAHH’s guardian (“referee”) is still stuck dreaming-on of a 1/2 century ago, in calling skepticism an extreme position: “worth hearing” he pseudo-tolerantly offers, even while continuing its suppression for a 4th straight decade at his JHA.) The ref added a death sentence to the paper, telling an editor who obviously wishes to stay on the good side of History-of-science’s Archbishop of TruthBury: “If this were my journal, I would not like to see this paper in it.” When JAHH supinely granted him full veto power over the paper, it had been determined that it was not going to appear in any form in JAHH. But censoring editors (& refs) are ever pretending not to be, so JAHH’s initial tentative approach to exploring for an excuse for nonpublication was to find out if the durable myth, that DR would not accept6 editorial revisions, would suffice to dodge publishing archon-allowed heresy.

A3 DR’s reply, www.dioi.org/owoqg.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/owo2w, 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 DJO 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

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, O.Gingerich actually claims [A]: DR wants to be shunned.) DR’s atheism heaps extra aggravation upon this self-described “practicing Christian” of the Mennonite cult. Students of the psychology of hate might profitably investigate this 40’s obsession: bizarre details (& Cardinal Manning’s perceptive) at §2 fn 5.

1 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.

2 One could add Sneider to the list of dihard, but not from admiration of Gingerich: fn 22.

3 In retrospect, it was predictable that Journal of Astronomical History & Heritage wouldn’t publish a paper showing its recently 2014 BZJ article was false in claiming Ptolemy’s star-observer’s. Editor Orchiston’s demand for revision looked like the start of an endless, wasteful game of never nding DR’s requested self-censorship sufciently adequate. (The theory that the paper was doomed from the outset is verified in the next-last paragraph of www.dioi.org/owo3l.pdf, unsurprisingly.) Since a durable cult lie (Hoskin to Thurston 1986/95) is that DR is “impossible to deal with” (contra DR cooperation with, e.g., Polar Record [Univ Cambridge] & Griffith Observer), it was presumably believed that this approach could kill the paper while never appearing to censor anything. Note that if the editor objected to parts or words (he, not DR knows his tastes in this regard: §3 fn 100), he need only have taken up DR’s 2015/9/30 suggestion, at www.dioi.org/jav9u.pdf, to strike such (isn’t this what competent editors do?) and for & are? — the job would’ve taken only an hour & sent the revision back for DR’s OK; but, then, what if DR had replied “Done”? Fixers keep their plans flexible.
tantrum of the other non-US journal in this strange field: the *Journal for the History of Astronomy*, whose Editor Michael Hoskin, upon receipt of DR’s constructive criticism of *JHA* refereeing of one of its papers, responded with threat and correspondence-cutoff. DR’s criticism was supplemented by a polite, *admittedly valid* DR referee report: see the paper’s recomputation at *JHA* 1984 June, which happened only because the *scientist* author preferred honest accurate results, and cast seed upon stone by (1983/4/27) recommending DR’s refereeing skills to *JHA*. Likewise, when Curtis Wilson and Hugh Thurston insisted (e.g., Thurston 1995) on correcting a flock of *JHA*-unrefered errors in Jones 1991H. In these and dozens of other instances (www.dioi.org/jha.htm/#hsbk) of serious DR-apprehended *JHA*-errors, that journal has never taken the initiative in correcting the situation, to produce accurate information — as if it really didn’t care at all about such trifling considerations.

A strict rule at *JHA* (at least vis-à-vis DR): if the erring author is no more honest than the *JHA*, then — no correction is made. (Prototype for *JAHH* 2015 and [11] Isis 2017.)

Embodying a third of a century of the field’s proud progress:

[a] *JAHH* in 2015 exiles a referee who expended extensive time and labor to respond scientifically to *JAHH*’s S.O.S. for assistance in cleaning-up a paper which was beyond that journal’s technical capabilities to evaluate, as thoroughly demonstrated below. Also: some among the authors may have found themselves rushed or only partially consulted, and out-of-available space for their paper, as a deadline approached — and out of time, having waited too long to call in expert advice. (But this doesn’t excuse the central omission: Ptolemy’s - 14' error in latitude L, emphasized in www.dioi.org/jar3g.pdf. DR’s 2011 letter to Brandt, for its fatal contrast with the trivial L- error shown by the data.)

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

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

A4 During the silent months that followed submission to *JAHH* of the paper that ultimately became ¶2 above, ever-cheerily-optimistic DR had entertained the possibility that WO was seeking a 2nd, more neutral referee.

Finally, DR emailed a friendly 2016/2/29 letter, www.dioi.org/oww2t.pdf, to WO and learned of a forthcoming announcement which at last nakedly unveiled a proud new addition to JHAD covering-archendum (¶5A). This confirmed that the usual heresy-containment info-control (standard for the last half-century: ¶2 fn 35) was being governed by the priorities of, in this instance, two colluding politicians. A final 2016/3/21 DIO letter (successfully sent to WO by alternate email address), www.dioi.org/oww3l.pdf, again tried collegially to allay hypothetical shyness (about editing a DR paper) by pointing out that DR could hardly complain of any WO deletions if he’d asked for them! As DR had, in his 2015/9/30 letter. The new letter ended with an easy test (In 13 of the theory that the paper had been dead from the outset.

No reply. Which is perfectly consistent with the theory.

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

A6 We will shortly move on to putting *JAHH’s* 2014&2015 sellouts into the context of the disgrace of worshippers’ ever-more bullet-hole-ventilated half-century pretense that Ptolemy observed, playacting which is intended to prevent the public from catching on to the field’s long-running thespian obfuscation in failing to admit publicly the ultra-obvious. The present paper started by stepping on an Ozzie egg — which reminded us of that old-time Easter tradition of planting hidden Easter-eggs all over a garden before unleashing the kids. Well, this kid is ever entertained by an Easter-hunt’s worth of prior Ptolemist article-eggs all about us (&B), every one a squashable plant, indeed, regularly planted during a half-century of establishment insistence on transforming a clumsy data-faker into not only an honest scientist but a genius,10 no less — a proposition as believable as a rabbit-laid egg.

So we will next turn to enjoying the decades-long history of this desiccated field’s transparent pretend-conviction (fn 50) that promotion of planted apologia for Ptolemy’s observership is any more credible than adding planted eggs to prove the Easter Bunny is real.

Ptolemy archon: “you mean she isn’t?”

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

B1 Does it tell us something about the state of soft academe that the *Journal for the History of Astronomy*, the “premier”15 journal of its field, has for decades used pseudoscience and deception in the shall- we-just-say extrameeemeenly 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 astronomicology. *JHA*’s long-time #2 official, Harvard’s Astronomy 101 teacher & dept Disraeliesque trowel-wielder Owen Gingerich, has repeatedly billed16 this ancient faker and superstition-peddler17 as “The Greatest Astronomer of Antiquity”, claiming that all who question this evidently-sensible-to-him proposition are the insane18 parties to the simmering-if-generally-suppressed dispute inevitably triggered by such superlative saleshype. We will first briefly examine a sample of the succession of careerist soldiers who disgraced themselves by eager enlistment in the *JHA*’s decades-long War-for-Ptolemy, who knew that publishing their genuflections boosted prospects for favor with the field’s dissent-burying archbishops, thereby improving odds for future conference-invites, publication, posts, grants — all with confidence that no matter how outrageously unlikely their arguments, they had nothing to fear from contradiction, in any of their captive journals.

B2 Ptolemy claimed to have outdoor-observed a 140 AD solstice, though his report (Almagest 3.1) typically disagreed with the real Sun by 1½’ ! — or a degree and a half, which is over FIVETIMES the angular distance from the Sun’s center to its limb (edge) — this, while his report agreed with Hipparchos’ indoor tables to within 1 or a fraction of an


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

17 Ptolemy authored astrology’s bible, the Tetrabiblos, the tripe in which has got to be (but usually isn’t) read if one wants a measure of how seriously to take Ptolemy as a scientist. See Rawlins 1977 pp.70-71 &97 for analysis of the book’s escape-hatchery, plus a fundamental Ptolemy gender-miscalculation (ibid p.69) which has escaped detection & thus survived for millennia, persisting even in today’s horoscopes, e.g., those on sale in our grocery-store-checkout-counter literature-departments.

18 Psychoanalyst Gingerich’s intended-to-be-scary but non-existent 2000 referee report to Isis (outed in Rawlins 2003X) called Ptolemy-skeptics just a tiny bunch of paranoids — thereby inadvertently and delusionally smearng most of the scholars in the field, even WHILE he is echoed in the 2015 JAHH referee report’s complaint that DR doesn’t respect those who disagree with him. Can it get any weirder? Well, actually, yes. As we see from www.dioi.org/pm2.htm, a secret Gingerich referee-report slander of DR’s character (so relevant to the subject paper’s mathematical development!) on a basis which Gingerich had forgotten he’d already revealed, at www.dioi.org/pm1.htm. applied to himself. (On the reality of cohesive shunning of Ptolemy-skepticism: see 3 fn 6.)
hour. Similar giveaway factors for his three other solar “observations”, all of which agree just as closely with indoor calculation. (Interim question: given this stark & unquestioned circumstance about Ptolemy, think carefully about what kind of scholar would dedicate himself to defending him, even to the extent of calling all spectral 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 Seward, rejected the all-too-obvious explanation for Ptolemy’s rigged 140 AD solstice with two imaginative excuses:

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

[1] Near a solstice, S. alleges it’s impossible to measure accurately the time of maximum height of the noon Sun, since from day-to-day it’s virtually not changing then. So referring by Phy Beta Kappa (fn 20) and by Reverend Gingerich, as usual (one might almost say: as-always, given the reliable brand of sheeple who man or oldboy Hist.sci’s most prominent forums)21 has approved an argument implying that if we toss a ball upward and catch it 4 seconds later, a 9th-grader (or younger) can’t tell that it peaked at 2 seconds?

[2] S.’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 1st-off-the-mark Ptolemy “observations” that could never have been made outdoors in the 1st place! Especially again & again & again. (The human eye can see to about 2 ordinates better; roughly 1°, and the solar semi-diameter is 16°, so his equinox-solstice errors average about 4 times the distance from the Sun’s center to its edge: §B2.) [b] Further, we know that 2nd century BC Greek scientist Hipparchos reported real observations which disagreed with his theories and with each other (§J fn 8); thus, faking or selecting data was not genuine ancient astronomers’ normal procedure. [c] So many accurate Greek astronomical achievements (e.g., lunar distance

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21The description has become deliberate because [a] the plain Hipparchan counter-evidence (item[2]) at §B3) was sent to the journal before publication, and [b] has been known to the perps for all the decades since, causing not the slightest retraction.

20 The incredible reasoning of Seward (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 §J 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: §J N7.

[B] More expert at the relevant science than certain modern wannaees, all ancient scientists used solstices not equinoxes for gauging yearlength. (Enumeration of these at idem; sources: ibid fn 11.)

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

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

For almost 40 years, 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, J.HA, Isis, Nature, JAHH, AJA, etc. Most, to their credit, later ignored his slander as irrelevant to the content, sought other advice, & published. The most grovelingly slavish — and the least concerned about veracity — were naturally also the least technically qualified (adamantly spurning politically-unacceptable expertise, by forever-cutoff of correspondence): JHA and JAHH & Isis.

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

24 Don’t miss www.doi.org/gm3.htm, longtime (1970-2013) “premier” Journal for the History of Astronomy-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 confirmation into lapdog ever nearer the ultimate intimacy it aspires to: lapsing a needy establishment.
to $\Delta = -29^\circ \sin \lambda \tan \beta$, 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-opennoned Ptolemites robo-countered this new shocker with their usual standard-weapon: scientific inability so truly embarrassing as to raise the question of whether impenetrable Ptolemitism has become a medical problem. Exhibiting the science-grasp of Ptolemy’s fellow-crank-liar F.Cook, MacArthur-Genius N.Swerkow attacked 29° tan $\beta$ as indefinable near the celestial North Pole, where 29° tan $\beta = \infty$; this, merely from his own innocence (2 fn 8) of undergrad math: celestial ($DIO$ 3 §A2 [d]) or terrestrial ($DIO$ 21 §C11) longitudes’ conversion to great-circle measure entails multiplication by $\cos \beta$, thus $\cos (\Delta \lambda)$ can’t exceed 29°. Swerkow’s response: he hides ($DIO$ 3 §C3). Meanwhile, Evans’ attack on the (§B5) absent-error-waves argument confused sine waves with cosine waves ($2 [H1]$) blowing off a 63° degree phase-difference with: “the phase is not exactly right” (Rawlins 1991W fn 152). All this to obscure that he can’t find a cosine wave in the Catalog’s zodiac stars, with amplitude even 1/3 as large as the 1°/2 he sought.

B6 In the 1987 $JHA$, Evans took his 1° large step towards proving he had the appropriate irreproachable honesty & ideological loyalty to succeed (as he did in 2013) then-Editor M.Hoskin, by taking-up no less than sixty-four handsome $JHA$ pages with two successive Pb papers (Evans 1987), wielding the brand of original math we saw in the previous section, arguing Ptolemy might have been honest. (After the revelations of Johns Hopkins University Press’ The Crime of Claudius Ptolemy [R.Newton 1977: deftly summarized by Thurston 2002S], acceptance of even this weak possibility was about all that Ptolemites could hope to accomplish among informed scholars. Still the case: e.g., Brandt et al 2012B.) Evans’ main arguments (see also 2 fn 47):

[1] In 1981 Evans used a cross-staff to measure the longitudinal distance of a star from the mid-eclipse Moon. “I find on examining my notes from that evening” the longitude’s error was c. -40° (Evans 1987 p.275), proving Ptolemy-skeptics were wrong to deny that ordmag 1° errors were not unusual for ancients. However, Rawlins 1991W fn 288 and Rawlins 2009E later showed that if Evans’ reduction hadn’t bungled his lunar parallax correction’s sign, the error would’ve been merely ordmag 1°. Evans’ response: he’s hidden “my notes from that evening” ever since, hiding also from questions on the incident put by Rawlins and Thurston. (See §3 fn 11. Conman Frederick Cook ducked inquiry identically [Rawlins 2017A §B13]. Again: one can see why Evans is the ideal choice to carry on the $JHA$ tradition of spotless integrity.) When later retelling the same argument (nearly verbatim: Evans 1998 p.259), Evans conveniently forgets to discuss that 1981 eclipse at all (switching instead to adducing a previously unmentioned 1977 eclipse he hadn’t outdoor-measured with) — and continues inexcusably contending 25 for ancient errors of ordmag 1°.

[2] Ptolemy claimed ($Almajest$ 7.4) to have outdoor-observed all 1025 stars of the $Almajest$’s Ancient Star Catalog. R.Newton 1977 proved that Ptolemy had instead stolen the...
Greek observers’ epochs $E$ were eyeballed not computed.

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

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

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

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

$$\delta = L \pm Z$$

(minus-sign for southern transit, plus-sign for northern upper transit, where $Z$ complements altitude $h$: $h + Z = 90^\circ$), so $L$’s error carries directly, additively, fully into the $\delta$ data, the systematic error of whose mean is therefore the error of $L$. This would seem to be obvious, but the 1st researcher ever to perform the test upon star data, to show 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 (ordinarily $1^\circ$) in the observer’s assumed geometrical latitude $L$. So it should not be hidden from the reader (as it is, throughout Brandt et al 2014B) that when Ptolemy reduces transit data (via eq.1), he uses an Alexandria $L = 30^\circ$S (Almajest 5.12-13), which rules him out as the declinations’ observer since this is in error by $-14^\circ$ (Alexandria being at $L = 31.1^\circ$).

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

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

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


28 Were there a case for a +57 observer, the most tempting identification would be Heron, who recorded an Alexandria-midnight +623/13-14 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 he faked them by adding 2 2/3 centuries worth of his false 1st-cy precession, namely, taking 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 misprocessed 1025-star Almajest 7.5-8.1 catalog is correct. There is no sign that BZJ knew of this match prior to DR’s referee report, nor does such vital information appear in Brandt et al 2014B subsequent to it, either. This positively belongs at the paper’s p.333, where we instead hear that besides the star-declinations (emphasis added): “Ptolemy offers additional evidence for his [(false) precession] value elsewhere in the Almajest (e.g., [Toomer 1984 p.133])” — innocent of the A.Jones-witnessed fact that upon viewing Gräff’s 1990’s evidence decades ago, Toomer agreed that the Catalog stars came from Hipparchos — which renders irrelevant the faked “Ptolemy evidence” cited to Toomer 1984 loc cit. The DIO ref-report asked: “So are we also to ascribe the Catalog to the same secret observer, which thus must have created a catalog of over 1000 stars though no one ever mentioned his or its existence?”

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

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

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

33 BZJ were helpfully provided sufficient advice to inspire proper caution: [1] were given all the right answers for $E$, $x$, and both’s standard deviations, [2] were told that their own values for “accuracy” looked remarkably too small, & [3] were repeatedly warned not to do the problem monovariately.
The erroneous figures for "accuracy" rs in Brandt et al 2011 and Brandt et al 2014B were an ordnag 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 means of the minuscule leftover residuals. (Our reconstructions of data via this procedure are in fn 35.) Perhaps we could dub this the "least-non-squares test". The impossibility of BZJ's numbers are easily seen: when Brandt et al 2014B p.331 puts the "accuracy" of Aristyllos & Hipparchos at 0.004, 0.003 instead correlates to 14&11 ARCSSECONDS, resp — obviously a fantasy (and BZJ were warned of this on p.4 of www.dioi.org/jauk.pdf. DR's invited referee reported, considering that the data's rms is admittedly 0°.1 on the same page: Brandt et al 2014B Table 2. (Equally incredible: idem lists rms values for methods of Maeyama, Rawlins, & BZJ — that agree with each other to a 1000th of a degree!)

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

In addition to the reversal-test revealed at §C11 — showing the invalidity of the paper's method — there is this equally obvious consideration: in Brandt et al 2014B p.331 eq.1, the coefficients of the unknowns are 1 and 0.33333333 α. The rms value of the latter in the these investigations is about 1/4, so the standard deviations for x and E should exhibit a ratio of about 1 in 4 to 4 in E. In Rawlins 1994L Table 3 and below in Table 1, this is roughly true. But no such symmetry appears anywhere in Brandt et al 2014A (abstract) or Brandt et al 2014B p.331. (Nor of paper Z R Rawlins et al 2013 admirably takes part in evaluating anyone's x—or anything at all about Ptolemy.)

BZJ's initial abstract, www.dioi.org/bzj1.htm, Brandt et al 2011, gave figures for "accuracies" (where we flip BZJ's unconventional C-O signs): Timocharis E = 295, 11 stars rs = -0.022, Aristyllos E = -258, 6 stars rs = +0.004, Hipparchos E = -128, 18 stars rs = -0.010, Polyclitus E = -115, 18 stars rs = -0.005. Later, Brandt et al 2014A p.6 & Brandt et al 2014B p.331 give different rs: Timocharis 0°.012, Aristyllos 0°.003, Hipparchos 0°.004, Polyclitus 0°.009. Our speculative reconstructions (via §C9's [a]-[d]) alter the experiments but (in a delicate problem) get agreements with some among BZJ's above false rs values. So maybe this or something like it was BZJ's procedure: [Accurate bivariate least-squares result follows each observer's reconstructed BZJ data, in brackets; plus actual minimal residual-square sum S mín, to show that most BZJ solutions do not approximate it.]

Timocharis 12 stars: E = -295, rs = -0°.022, S = 274522. [E = -277±18°, x = -0.076±0.077, Smín = 2441.22].

Aristyllos 6 stars: E = -258, rs = +0°.003, S = 1477. [E = -258±10°, x = +0°.014±0.045, S = 147.3].

Hipparchos 17 stars (A9n0 = 6°3±.3°): E = -128, rs = +0°.004, S = 446.2.

[Timocharis 12 stars: (Eo = 6°.3±.3°): E = -128, rs = +0°.004, S = 446.2.]

Timocharis 12 stars: E = -115±13°, x = -0°.004±0°.052, Smín = 2521.22].

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 trialerror to find S. Nonetheless, the values found for x (though not recognizing it as latitude-error) and E are roughly correct, since Maeyama in-effect was running a double-monovariate test and had the good fortune that the unknowns' correlations were not too serious. And at least (unlike BZJ) he realized that the leftover residuals after the 1st monovariate test were to be fed into the 2nd such, to find the value of x which minimized the sum of the residuals' squares. However, for x's standard deviation σx, Maeyama 1984 Table 1 column d wrongly lists σo, the mean error of a single observation. The resultant errors range as high a factor of nearly 7 (the Hipparchan 44-star sample).

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

Maeyama are clear from Brandt et al 2011, though Brandt et al 2014B p.331's false presumption is that rs will serve instead of x. But what then of x and its standard deviation σx — neither even mentioned by BZJ? While Maeyama's 84's estimates of σx are (fn 36) off by serious factors, Brandt et al 2014B's misconception produces nothing at all! — no σx whatever.

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

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

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

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

C15 If it seems odd that, previous to 1982, no one had found the accuracy of the four ancient observers' location (see Rawlins 1985G §3 conclusion), let's expose something even more revealing: in 36' since 1982, no one else has computed it correctly, either. The two post-1982 papers both waste precious journal-space extensively on graphs & histograms, all to do the analyses inferiorly, e.g., monovariately finding that E where x is minimal, S mín. (And the archons of history-of-ancient astronomy imagine they have the capacity to judge, shun, condemn, & censor the scrupulous, competent researchers of scientists in such matters?) Note the parallel to the Journal of Astronomical History &
The p.332 grouping is E (Early 6 stars) and L (Late 9 stars), which does not follow the traditional split, namely: the “SickSix” stellar declinations (which Ptolemy “deduced” his false precession from) versus the “Clean Dozen” real declinations (which his precessional math ignored: “our groupings have no simple connection to Ptolemy’s selected six stars” (Brandt et al 2014B p.334). Why? Well, R.Newton 1977 pp.220-225 rightly argues that Ptolemy typically fabricated the SickSix from 1°/cy precession in order to equally—typically then “prove” said precession from the fabrications: §2 §B2. The new grouping would undercut this view by fracturing the SickSix. Problems with the new split (as noted in DR’s 2011/3/15 letter to Brandt): the traditional split is simple, is Ptolemy’s own. Further, DR’s 1994-adopted +159 epoch (Rawlins 1994L fn 45: all 12 Clean Dozen stars with no deletions) was understood in 2011 for the 1st time to establish no residuals—overlap (fn 7) in the Clean-Dozen-vs-SickSix split, a finding nowhere cited in Brandt et al 2014B, which keeps mis-rendering DR’s also-uncited +159 date, though it was repeatedly, www.dioi.org/bjr1g.pdf, www.dioi.org/jau8q.pdf, put to BJZ. As is obvious from Brandt et al 2014B’s Figs.7&8 (C—O), star-residua’s proximity to each other is time-dependent.

E.g., the residuals of Alioth and Aldebaran are 18° apart in +128 (Ptolemy group L date of Brandt et al 2014B’s Abstract, Table 2, and pp.332&334) but are within 2° of each other in +159. Indeed, as DR’s 2014/8/26 ref report noted, if we eliminate none of the Clean Dozen and run a bivariate least-squares on them exactly as they stand, the solution is E +159 ±8° & L +14±2° (L = 31°11’±2’: Alexandria) and the extremest residuals are symmetrically within 10° of zero: Betelgeux +10° & Pollux —10°. For contrast, one may examine the results of applying, to the Sick stars, the very same test just done on the actual stars—though that of the non-eliminated stars Altair, Castor, Pollux, & Regulus. So the final published version (p.332) expanded the justification for eliminating the Unhelpfuls to include that their zero error occurred later than 200 AD (a criterion eliminating Alair [p.334 vs p.335&Fig.8], though it was a member of group L) which is to say that the very stars that BZJ was citing as “all zero” (F) which, by subtraction of x, easily produces each observer’s absolute actual latitude L. All four least-squares-fitting E and epochs L (Timoclis 11 stars; Aristyllos, 6; Hipparchos, 19; Anonymous, 12), along with their standard deviations (σE & σL), as well as single-data standard deviation, raw (σ0) and with the effect of rounding39 removed (σi). 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) — preferentially along with a sentence on the obvious novel method42 which DR’s had invented while pioneering this entire line of inquiry. But, probably because DIO’s x values especially & hugely disagreed with JAHF’s “accuracy” values, the published article did none of these things.

Following such odd doings, Brandt et al 2014B performs somersaults of arbitrariness,43 and unorthodox implicit weighting, while splitting the “Ptolemy” 18 stars into two groups (after dropping three stars at p.332, then a reshuffled four at Fig.10) — groupings

<table>
<thead>
<tr>
<th>Obsrv</th>
<th>E ± σE</th>
<th>Adop L</th>
<th>Its Error x</th>
<th>Actual L ± σL</th>
<th>σ0</th>
<th>σi</th>
</tr>
</thead>
<tbody>
<tr>
<td>Timoc</td>
<td>−302±8°</td>
<td>31°12’</td>
<td>+1°.5±1°.9</td>
<td>31°10.5±1°.9</td>
<td>+6°.1</td>
<td>+5°.9</td>
</tr>
<tr>
<td>Aristyll</td>
<td>−258±10’</td>
<td>31°15’</td>
<td>+1°.4±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±8°</td>
<td>36°08’</td>
<td>+0°.2±1°.2</td>
<td>36°07.8±1°.2</td>
<td>+5°.2</td>
<td>+5°.0</td>
</tr>
<tr>
<td>Anon</td>
<td>+159±9°</td>
<td>31°15’</td>
<td>+4°.4±2°.0</td>
<td>31°10.6±2°.0</td>
<td>+6°.0</td>
<td>+5°.6</td>
</tr>
</tbody>
</table>

38 The sole non-fit for the dozens of data in the nulls experiment was Timocli’s Aldebaran. Rawlins 1994L fn 29 suggested that the original North Polar Distance may have been 81°15’, recorded (conventionally for unit-fractions) as 81°1/5’, but later misrecognized (like §3 fn 44) as 81° & 15 arcmin, thus δ = 8°3/4, as at Almajet 7.5. Thus, reconstructed true δ = 8°14/15 or 8°56’, which also sheds a poor residual. And Arcutrus obviously bears a 1° scribal error; restoring the original and eliminating outsized-residual for ZubeneGenesis (loosely-rounded δ = −5°), we have the Timoclis entry in Table 1 here. (For Timocli’s results based on non-reconstructed data, see §3 Table 2.)

39 Timocli & Hipparchos used a precision of p = 12 intervals/degree; for Anonymous, p = 8; Aristyllos, p = 4. The inverse of p = 12/5 is the rms of the effect of average rounding, in degrees.

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

41 E.g., at Brandt et al 2014B p.331, for all 3 observers, our 1982 ms’ epochs E & σE are relayed, conspicuously omitting our x & σx.

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

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

DIO-J.HA 22 ¶4 Ptolemaic Enmity 2018 D.Rawlins

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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 latitude: from nulls38 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 (Timoclis 11 stars; Aristyllos, 6; Hipparchos, 19; Anonymous, 12), along with their standard deviations (σE & σL), as well as single-data standard deviation, raw (σ0) and with the effect of rounding39 removed (σi). 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 obvious novel method42 which DR’s had invented while pioneering this entire line of inquiry. But, probably because DIO’s x values especially & hugely disagreed with JAHF’s “accuracy” values, the published article did none of these things.

C17 Following such odd doings, Brandt et al 2014B performs somersaults of arbitrariness,43 and unorthodox implicit weighting, while splitting the “Ptolemy” 18 stars into two groups (after dropping three stars at p.332, then a reshuffled four at Fig.10) — groupings
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 \textit{E}, Brandt \textit{et al.} 2014B adheres to depending on each star’s “crossing time” (the year when its residual is zero) & “slope” (rate of change of declination/year). Though of some interest and utility as rough checks (on better procedures), these approaches are sub-prime (especially when compared to standard approaches — which are perhaps especially weak since they give results in line 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 \textit{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 \textit{L} (as already noted at fn 42), though the paper indicates no awareness of this as it deletes 3 or 4 stars (not quite the same ones), from one section to another.

C19 During their \textit{E}-search analyses’ odd-option dependence on crossing-times (instead of obviously-preferable measure by residuals; reminiscent of www.dio.org/iff.htm#wsa), Brandt \textit{et al.} 2014B tries including weights by slopes’ absolute magnitudes (p.331 & Fig.6), the kind of Legendrian primitivity that Gauss devised least-squares to obviate. (Again: this requires deletion of stars which Gaussian analysis doesn’t.) But when the paper moves into cluster-analysis, even this precaution vanishes.

C20 The results with a long, illustrated section (slightly altering §C17’s \textit{L}-vs-\textit{E} regroupings that replaced Ptolemy’s simple split) which tests for clusters in stars’ crossing-times. This is a potently poor basis for eliciting anything valuable, for the obvious reason that the crossing-times’ reliabilities are highly disparate (§C19), due to slopes that vary from nearly the full possibility (0'3383/yr) to virtually zero — the latter producing nearly valueless crossing-times, which lead to exclusions and inclusions based on virtually random happenstance. The cluster-analysis deletes (p.335) Castor, Altair, Betelgeux, & Sirius (not consistent with earlier deletions [p.332] of Aldebaran, Betelgeux, & Sirius [§C19]), yet in both cases, stars with slopes weaker than some of these are retained. (See, e.g., fn 42 above.) E.g., why does the clustering section of the paper eject Castor but keep Pollux, whose slope is smaller? — probably because their mutually wan slopes (nearby horizontal) in Fig.7 magnify a tiny difference (just a few arcmin: less than \(\sigma_{\text{L}}\)) 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 \textit{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 \textit{E}: Brandt \textit{et al.} 2014B repeatedly ignores (fn 45 above) the request, by the DIO referee report, www.dio.org/jau8q.pdf, to correct its \textit{repeated} misreading of Rawlins 1994L’s date (for Anonymous’ Clean Dozen stars) as +131 instead of Rawlins 1994L, Table 3’s actual published value, +159. This is a 28' difference, which matters, as we saw at fn 44 & 45. (The record with +131, was merely Rawlins 1994L’s date for Hipparchos — not Ptolemy’s Anonymous — additionally shorn of its minus sign!) Importantly, +159 is an epoch which, for the 1st time, renders stark and unambiguous (§C17) the Clean-Dozen-vs-SickSix split (by contrast to any of the many other Anonymous epochs \textit{E} that were flirted-with in the article or the referee report), and in doing so implicitly jettisons as needless (fn 46) Brandt \textit{et al.} 2014B’s elaborately-derived unorthodox groupings. (See fn 42 above, for the advantages of adopting what is after all Ptolemy’s own split.)

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

C23 As a final quietus to \textit{JAHII’s} monumentally stubborn 2014 adventure, we now show how easily a scrupulous journal could have checked on whether its or our solutions for \textit{x} were correct. All \textit{JAHII} 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 \(\sigma_{\text{L}}\) in Table 1, multiplied by the number of degrees of freedom.) Or: for any of the four ancient astronomer’s star-residuals, [subtract DIO’s tabulated\textsuperscript{47} \(x\) for that astronomer, and then [2] just re-run Brandt \textit{et al.} 2014B’s monovariate test for him. BJZ will then encounter a sorta-pleasant surprise: all four astronomers’ values of \(S\), the sum of residuals-squared, will be found to have declined (comparisons in fn 35), showing that Brandt \textit{et al.} 2014B’s 3 generally (except for Aristyllos) didn’t get very near optimal (extremal) solutions \(S_m\). (Due to low correlations, the differences are not huge; but they show that true bivariate procedures were not applied by BJZ.) Even so, the suggested better solution being found by crude means (fn 36), results in \(S\) usually near but not quite at the lowest \(S\) possible. This goal can, however, be accomplished through a true bivariate least-squares (as in Rawlins 1994L), which efficiently finds the point in \(x\times\text{space where S is a minimum. If BJZ have any doubts that DIO has found THE actual minimum S, they need only conduct the very same test, using our }\text{c}\times\text{values, to find an S lower than their own. Using the slightly improved values (vs the referee report) of Table 1 above, the S cannot be decreased further (more than micro-trivial noise) by varying either }\text{c}\) or \(x\).

Our solutions for \(x\) are just 0-4 arcmin. The size may be small, but the issue isn’t: the

\textsuperscript{44}To understand why Ptolemy faked his era’s Arcturus longitude to equal the exact false value he gave at \textit{Almagest} 7.3, see §2 fn 37 — a precise vindication of R.Newton’s solution, which DR is ashamed to admit he did not fully accept until 2011, thanks entirely to Jack Brandt’s inquiry.

\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 \(-8'/2\) (Zubenelengebi) to +15'/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 \(\sigma_{\text{L}}\) (6' vs 5'); an untampered sample (no deletions); and agreement with the Suda’s date for Ptolemy, Marcus Aurelius, +160, not the Ptolemy-claimed epoch: Antoninus, +137. (More exactly, the two epochs are 1607/14 and 1377/20 Alexandria App.Noon.) Note: a monovariate \(c\) solution for the same stars leads to +150, a serious difference (see §C13 on similarity). And, since such automatically assumes \(x = 0\), we have \(L = 31° +6'\) (see §C16 above, & Rawlins 1994L §3H), which is 3 nautical mi north of Alexandria’s \(L = 31°12'\), whereas the +159 bivariate solution \(x = 4'\) closely reflects the +3' error in the observer’s overlarge assumed \(L = 31°14'\), and so is effectively right-on: \(L = 31°11'\pm 2'\) (ibid Table 3). All of these neatnesses render it doubly strange that Brandt \textit{et al.} 2014B persistently refused to recognize DR’s discovery of +159, misprinting it (over warnings) again&again as +131. See §C21 below.

\textsuperscript{46}See at www.dio.org/getp1.pdf, DIO 14 \#1 §2, & www.dio.org/getp3.pdf, ibid §3 fn 13, the parallel case of now-neglected metrological theories that keep getting proposed to weakly explain the already strongly established ancient Earth-size of Sostratos-Eratostenes.

\textsuperscript{47}DR’s latitude-errors \(x\) for Timocharis, Aristyllos, & Hipparchos have for decades been available, for all check, at Rawlins 1994L pp.44-46 & Table 3 — virtually the same as in his 1982 ms, to which Brandt \textit{et al.} 2014B p.331 acknowledges access. The BJZ paper’s sampling differs from Rawlins 1994L’s 19 stars (vs BJZ’s 17) for Hipparchos & 12 stars (vs BJZ’s 18) for Ptolemy, but the \(x\) that’s appropriate for BJZ’s sampling was provided at pp.3-4 of www.dio.org/jau8q.pdf, the \textit{DIO} 2014 ref report. (Due to minuscule differences in adopted star-places, the \(x\) value that will produce minimal \(S_m\) may not be super-precisely identical to DIO’s; but further trials will easily find it, very, very nearby.
central point here is (as 1st revealed in Rawlins 1982G) that ancient scientists found L to ordmag 1º accuracy. For that reason, as well as Brandt et al 2014B’s p.331 advertising 1º accuracy, the most precise solutions for x are appropriate. This becomes important (fn 45) for the Clean Dozen, where x = 4º, closely reflecting the error in the observer’s adoption (independently demonstrated in Rawlins 1994L [F8] of L = 31º1/4 for Alexandria, which is 3º (close to 4º±2º) larger than the reality: L = 31º12º.

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

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 unprincipl[ed]48 — sometimes (e.g., fn 18) even vicious — stubbornness50 of those determined to protect Ptolemy from public exposure by any means (e.g., a recent, obscurely-cited [and they are mostly obscurely-cited] zeal to protect Ptolemy and/or their goooos — resorting to any sloppy argument, any curtailment of free discourse necessary to prevent the larger scholarly community as well as the public from learning that the field’s most powerful archons (controlling the funding and thus the career-security, rewards, & awards of those who volunteer to espouse and do battle for sacred myths) made two huge and related blunders (see §2 [M2]: “to fit him”), when they long ago prematurely announced Ptolemy an honest observer and misperceived Greek astronomy as non-empirical. (See, e.g., ScAm 1979, discussed above at fn 22; and more thoroughly at §2 [M3 & fn 52].)

D3 Being politicians, Ptolemist archons are the sort of people whose idea of intellectual engagement tends (for obvious reasons) not towards weighing scientific arguments but to [1] plundering (§2 fn 5) their opposites as fools, knaves, and nuts (before discussing evidence — if ever doing so at all), while [2] pointing50 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 cynosurae seem so Enormously Stupid — as they must be or act, if skeptics are right?

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

48 But, to be fair, let’s admit that it’s not just the Ptolemy-defender side that uses Dirty Tricks in combat. In those apologists’s eyes, DIO uses Dirty Tricks just as cruelly and frequently — that is, whenever we resort to outrageously extreme outliers like competent scholarship, ethical dealings, and defying Infallible Archons. (To pols, it just doesn’t get any dirtier — or extraterrestrially unfamiliar.) Don’t forget boldly-unntrustworthy DIO’s prime motto (www.dioi.org/mot.htm#bcs): a man who can’t be bribed can’t be trusted.

50 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.

51 Without citing the various powerful evidential proofs that Ptolemy stole the star catalog, Schaefer 2013 p.47 instead revealingly resorts to sociology to aver that we can’t KNOW so because herd-loyal Ptolemists (like BZJ) still exist: “neither side is able to produce decisive evidence to convince the other side.” (But one thing we can be sure of: inserting such archon-comforting & gratuitous irrelevancy is sure to get a paper published at JHA.) One trusts that jollyplo Schaefer is smiling as he watches the JHAD show. And one recalls Thurston’s quote from Bishop Berkeley, “I observed how unaccountable it was, that men so easy to confute should yet be so difficult to convince.” Another DIO motto (DR), which extends also to many other faiths: “Why does anyone continue believing and/or their gurushies — resorting to any sloppy argument, any curtailment of free discourse necessary to prevent the larger scholarly community as well as the public from learning that the field’s most powerful archons (controlling the funding and thus the career-security, rewards, & awards of those who volunteer to espouse and do battle for sacred myths) made two huge and related blunders (see §2 [M2]: “to fit him”), when they long ago prematurely announced Ptolemy an honest observer and misperceived Greek astronomy as non-empirical. (See, e.g., ScAm 1979, discussed above at fn 22; and more thoroughly at §2 [M3 & fn 52].)

D5 Such childishly obvious illusionism, as delineated at §D4 above, utterly confounds the non-specialist part of the academy, as well as the increasingly non-investigative (“science” press, and is the key to the endless pretense of Ptolemist historians-of-science — knowingingly careless of concomitant harm to academic’s accurate perception of ancient history — that archons have not either been proven as Notoriously foolish as Raines: if just a few of puppeteer Gingerich’s claque can forever (§A2, & Rawlins 1992V §C24) keep publishing contrived even-if-laughably-transparent-to-scientists defenses, sapping an ever-befuddled lapdog press-corps, then the prime long-term public perception is secure: Not a single Ptolemy-defense archon was ever wrong on his honesty. (Gerald Toomer the admirable rule-proving exception: §C5.)

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

D6 Beyond Ptolemists’ lack of science’s attitude is the mundane matter of skills. Virtually every member of their clique, whatever his eminence, has no high scientific expertise51 relevant to the Ptolemy controversy. Non-specialists — unable to understand the debate’s technical details (or too busy to take the time) — are oft impressed with networking archons’ too-off-network-geared posts, awards, university connections, etc. And thus are too easily diverted from the seemingly obvious point that just because a Ptolemist is an astronomer doesn’t mean that he knows much about astronomy — that archons have not either been proven as Notoriously foolish as Raines: if just a few of puppeteer Gingerich’s claque can forever (§A2, & Rawlins 1992V §C24) keep publishing contrived even-if-laughably-transparent-to-scientists defenses, sapping an ever-befuddled lapdog press-corps, then the prime long-term public perception is secure: Not a single Ptolemy-defense archon was ever wrong on his honesty. (Gerald Toomer the admirable rule-proving exception: §C5.)

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D7 Concluding: we anticipate that (unless made shy by our 2014 referee report, www.dioi.org/jau8&qpdf) Ptolemy archons are already typically (§D3 above) pointing to Brandt’s many awards&posts, as if relevant, proud that yet another Reputable Figure has, after reviewing the evidence, decided to help the needy establishment by coming out for Ptolemy, hoping no-one will notice the §A1-obviousness of the fact that eventual discordant-evidence-sterilized Brandt et al 2014B’s conclusion was all-along set in cement.

Slippers.

51 In mathematical history-of-astronomy, dimbuls + careerests + thesians + poles now constitute a majority. If able, honest scientists ever rejoin&review the present era, it’ll be remembered, with eyes averted to the fatal Dark Ages, when reason was detrimentally punished in favor of Invincible Innocence. Archons will stoop to ANY tactic, to postpone that day indefinitely. Understandable. For them.
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