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

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


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

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

Effectively libeling scientists’ standard theory-testing criteria (by which one looks for the simplest theory consistent with the most data), Shcheglov calls us “deluded” for Occamly choosing the popular, simple, datatiling hypothesis: that the 1.4-factor error (40% overestimate) in Ptolemy’s Earth-longitudes was from just multiplicatively stretching them to shift from Eratosthenes-Almajest’s 700 stades/degree to Marinos-Ptolemy’s 500 stades/degree. Shcheglov discards the 1.4-stretch theory by claiming that his true explanation for the 1.4-exaggerated longitudes “proves to be much more complex and intricate” than 700/500 simplicity. But nothing approaching the promised “proof” of the need for complexity ever actually appears in the article, where most complications are gratuitously, articially 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 eficiently as plain, raw multiplication.

¹Curious examples of Shcheglov mischarges: [a] The simple-stretch idea is alleged (Shcheglov p.693) to bear logical fallacies; none are produced. [b] The stretch-solution is said (idem) to follow R.Newton’s criminal charges versus Ptolemy. Though I agree Ptolemy faked I’ve never said his stretch was anything but a bad mistake (end of §F below). [c] Ignorance of alternate theories is implied (vs *DIO* 6 fn 47. *DIO* 20 fn 2). [d] I’m mis-said (p.693) to claim accurate land-surveying underlay Ptolemy’s longitudes. (My spare proposal was a simple longitude-multiplication, without any connexion to Shcheglov’s amazing & valuably complete reservoir of centuries of stadelength guesses.)
B Shcheglov (p.705) calls early accurate geography “a quaint illusion” — and 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 [Muffian fantasy] of bumbling Greeks also defies the broad context of their high physical science as revealed by us [e.g., www.dioi.org/jl09.pdf, for decades]. Our ordmag-estimates of Greek accuracy (check out each for yourself):

1 for big cities’ geogr. latitudes L (Strabo 2.5.7; Isis 73.2 p.264; DIO 16 13 3 §C1-C2) for scientists’ L (Isis 73.2 p.263; Centaurus 27 p.280; DIO 4.1 §3 §F; EAH 17 p.326)

0 for star declinations (ditto)

1% for Earth-circumf. precision (ArchiveHistExactSci 26 p.216; DIO 14 1 §5 §A & eq.28)

1% for Earth’s tilt “obliquity” (Klio 27 p.266; DIO 16 13 §3 §A-B eq.2 and Tables 1&2)

1% for lunar mean distance (Almagest 5.13-17; DIO 8 §1 §H4; 59 Earth-radii vs really 60)

10% for lunar eclipse-prediction (DIO 1.16 §eq.32)

1% for time of lunar eclipse (Greenwich 1984 in Vistas in Astronomy 28 pp.258&265)

0.1% for lunar limbus vs Sun prediction (DIO 16 §1 fn 24)

ditto or even 1% for star-vs-eclipsed-Moon gap (DIO 1.3 fn 288; DIO 16 §1 §A fn 22)

1% for solstices (Bull.A.A.S. 17.2 p.583; DIO 20 §2 eqs.21&25#Table 3; P.Foad 267A)

for —145/3 equinox on Alexandria paleara, polestar-set ring (Isis 73.2 p.263 n.17)

10% for sidereal year (DIO 6 §1 fn 38&47; DIO 9.1 §3 Table 2; DIO 11.1 §1 fn 14-15)

1% for century for 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 anomalistic 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-astronomy are, like Shcheglov, unaware of these symptoms of high Greek science, some, e.g., Gingerich&Swerdlow, speculating that scientists kept only theory-accordant data [falsely contradicted by Hipparchos’ record] from 3/8 in f4, thus unwittingly modeling all ancient science on a blundering astrologer, Ptolemy.

QUESTION: how could the above-listed measures have ever progressively evolved into accurate achievement by following a tradition of just keeping on confirming prejudice? [Classic projection from own behavior? See below POSTSCRIPT’s final line.]

C That ancient geographers’ longitudes were based on eclipses is doubted by Shcheglov p.690 as “too impractical”. I’ve outdoor-eye-ball-timed enough lunar eclipses to know their accuracy is ordmag 1° [anciently somewhat vitiated by sundial graduation limitations, ordmagly agreeing (at 4°¹/1’) with the well-under-1° accuracy of pre-stretch Geography longitudes, D.Rawlins 1985, “Ancient Geodesy: Achievemen and Corruption”, Vistas in Astronomy 28:255-268; p.265 (1984 Greenwich paper). Though eclipses are common (Ptolemy experienced 3 in 3 years: 133-136 AD, Almagest 4.6), Shcheglov’s n.8 accepts that this central method, is half a millennium old, the 3309/20 “Arbela eclipse”, Ptolemy’s reported time (longitude) gap is 4/3 too big, so Shcheglov’s n.8 tries 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 Roman vs Alexandria; Pliny 3 Campania vs Armenia (4th pair: Pliny’s correct 2 Sicily vs Arabla).

D But Heron didn’t even try to gauge longitude-gap by eclipse (Neugebauer, History of Ancient Mathematical Astronomy, 1975, p.848). Kleomedes’ 4° gap is virtually correct since Cadiz at 25°W longitude and Persepolis at 38°32E is 33°57’ apart. Shcheglov just mis-signed Cadiz and found 30071° (comfortably consistent with Ptolemy’s false 4/3 factor for his Carthage-Arbela gaffe-gap). As for Pliny 2.72.180, Shcheglov knows Campania & Armenia are c.2° apart. [Longitude gaps between Naples & the Geography’s Armenian cities (Diller DIO 5 Table 17; 1984) Dioskourias, Artaxata, Gaggara are 1°47’ , 2°11’ , 2°23’ , respectively, all indeed about 2°]. So Shcheglov concluded that Pliny’s 3° is too high. Yet Pliny even the Campania-Armenia longitude gap is 3° but that the eclipse was seen 3° of local time differently. [Ancients recorded the time of an eclipse’s start: Neugebauer op cit p.844 n.12.] For a solar eclipse, one can’t just equate time-difference and longitude-difference. Local Apparent Time for the eclipse differed in Naples from that at the 3 Armenian cities, by 2°29’ , 2°48’ , 3°14’ , respectively, mean 2°50’. So Pliny’s 3° was not “badly overestimated”. (Neugebauer, op cit p.668, had verified Pliny, evidently adding, “Solar eclipses are, of course, without value for longitudinal determinations.”) So, ironically, both of Shcheglov’s eclipse-examples for ancient inaccuracy have backfired.2

E Shcheglov’s other Pliny record is the same Arabla lunar eclipse Ptolemy mis-reports as 8 P.M. at Carthage, 11 P.M. at Arbela. But Pliny has the same event 6 P.M. at Sicily (west Sicily was under Carthage then), 8 P.M. at Arbela, resp, both times correct within minutes. Shcheglov n.8 doesn’t connect the two Arabla-eclipse-reports; & neither he nor any other historian-of-science has noted that “authoritative” (G) scientist Ptolemy has accidentally misassigned Arabla’s 8 P.M. to Cartaghe! 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 Cartague time. Since his 4/3-stretched map already had Arbela 3° east of Cartaghe (real gap 28 ½/4), he faked Arbela thusly: 8 P.M. + 3° for time of lunar eclipse (Greenwich 1984 in

1.4.2), to illustrate this

omnipresent parallax. But Heron didn’t even try to gauge longitude-gap by eclipse (Neugebauer, History of Ancient Mathematical Astronomy, 1975, p.848). Kleomedes’ 4° gap is virtually correct since Cadiz at 25°W longitude and Persepolis at 38°32E is 33°57’ apart. Shcheglov just mis-signed Cadiz and found 30071° (comfortably consistent with Ptolemy’s false 4/3 factor for his Carthage-Arbela gaffe-gap). As for Pliny 2.72.180, Shcheglov knows Campania & Armenia are c.2° apart. [Longitude gaps between Naples & the Geography’s Armenian cities (Diller DIO 5 Table 17; 1984) Dioskourias, Artaxata, Gaggara are 1°47’ , 2°11’ , 2°23’ , respectively, all indeed about 2°]. So Shcheglov concluded that Pliny’s 3° is too high. Yet Pliny even the Campania-Armenia longitude gap is 3° but that the eclipse was seen 3° of local time differently. [Ancients recorded the time of an eclipse’s start: Neugebauer op cit p.844 n.12.] For a solar eclipse, one can’t just equate time-difference and longitude-difference. Local Apparent Time for the eclipse differed in Naples from that at the 3 Armenian cities, by 2°29’ , 2°48’ , 3°14’ , respectively, mean 2°50’. So Pliny’s 3° was not “badly overestimated”. (Neugebauer, op cit p.668, had verified Pliny, evidently adding, “Solar eclipses are, of course, without value for longitudinal determinations.”) So, ironically, both of Shcheglov’s eclipse-examples for ancient inaccuracy have backfired.

2 Shcheglov’s solar-eclipse misadventures [are similar to NGS-NavFoul’s at DIO 21 §5 §§B3-B4, www.dioi.org/j05.pdf, also reincarnating a backfired attempt to empirically justify demeing Greek accuracy: Evans’ 1987 analysis, that his 1981 measurement of a star’s distance from the eclipsed Moon was ordmag 1° off, like Hipparchos’ two bad measures of Spica. But undoing mis-signed parallax lowers all three 0.6 errors to 0.1 or less; same for Hipparchos’ —35 Regulus error, so odds against all four errors being outdoor are astronomical. Evans won’t reveal his 1981 data; but his 1998 book repeated his 1987 analysis, a non-observed 1977 eclipse quietly subbed for the 1981 event! See details at DIO 16 §1 §A & fn 7. Shcheglov, NavFoul, & Evans have in common that all 3 are self-torpeded by innocence re parallax, as was early Hipparchos (vs his consistently accurate later work: §3 [B8p]).]

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

I. Shcheglov admires Geography latitude-accuracy (p.689, emphasis added): “Methods for determining latitude, being rather simple, had [long] been known in Greece. . . . By Ptolemy’s time, latitudes of a number of the most important cities had been determined (e.g., Alexandria, Rhodes, Athens, Rome, Massalia) . . . . Ptolemy calls such cities . . . ‘foundations’ that should be used as reference points for developing the rest of his map.” No mention that all five “foundations” cities’ Geography lidades 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 (JAH 1984) show that all 4 real, non- amateur Greek scientists cited above at §3 [3] knew their latitude to ordmag 1°. This twice-confirmed Rawlins discovery undoes Shcheglov’s entire inaccurate-geography thesis.

He doesn’t mention it. Nor does he mention the contradiction it obviously creates versus the Geography’s mean latitude error of ordmag 1°. In response to the disjunct, one JHA Editorial Boredperson has offered that geographers must have ignored astronomers! (So, did astronomer-geographer Hipparchos ignore himself?) DR mathematically contends (“Achievement” pp.260-264) these hitherto-unexplained errors were instead from forced latitude-uniformization-herdings, for astrologers’ convenient access to tables at each key latitude or “klima” (for horoscopes’ Ascendant and other “house” boundaries: “Secrets”, eqs.2–3), corruption inconsistent with the astronomer-scientist Shcheglov sees Ptolemy as.

J. “Achievement” p.262 lists 17 cities where, in the Geography, latitude matches klima. Dropping noteworthy-flawed Bithynia (DIO 20 23.4) & way-south Meroë leaves 14 cities.

K. Selling or owning klimata tables for every latitude-degree was impractically voluminous. [Thus, if Almajest’s 1°/4 klimata-interval was adopted, then each city whose longest-day was closer was 1°/8 to a klima was grouped under it, its latitude made equal to exactly that klima’s latitude.] Such groupings of cities under ONE latitude is explicitly attested at Geography 1.4.2 (even while justly criticized at ibid 8.1.1; different authors, in all likelihood). Effects of such data-tampering are obvious from errors found [in §3’s sample], while rms errors.² [as expected if due to real astronomers, like those of [§3]: 64°, –43°, –251°, –30°, –26°, 148°, –40°, –59°, –84°, –108°, 38°, 204°, 10°, 124°. Dropping –251° (confused Carthage mis-latitude: DIO 16 §3 fn 43; 2009), rms error is 93°; but the (more reliable) median is 59°, hinting both are skewed high by a few goofs.

L. Given this mess, one might ask: who says there ever were accurate ancient maps? We reply by turning to the same 1°/4 latitude-awful cities’ longitudes, and receive a shock. (Sample originally compiled in 1984 for another purpose so not prebiased for longitudes.)

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

N. Dividing 4/3 into §3’s 14 Geography degree-longitudes vs Alexandria, to unstretch them: those 6 cities within 30° of Alexandria show rms longitude-error c.2°, or about half a degree. The other 8 cities, several of them ordmag 1000 miles from Alexandria, likewise show rms 2°. Some 2° appears from 8 cities’ Geography: Book 8 hour-longitudes (some overlap with above sample), already published at ibid p.265, though neither the informatively small errors nor their implication is remarked by Shcheglov.

O. Errors’ small size is apt (ibid p.258) to longitudes based on accurate eclipse timings. As is their remoteness-independence (§N), since the error in local-time difference for eclipse observers longitudinally 1° apart is no more or less accurate than for 100° apart. Which is why the unstretched 42° from Carthage to Persepolis is correct to ordmag 1°.

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

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

R. Contra Isis, the data are consistent with early currency of astronomically-constructed, accurate pre-Geography maps, which professional astrologer Hipparchos semi-randomly ruined through disturbing longitudes by lumping them into discrete klima-cubbyholes where all cities in a cell are force-assigned the same latitude (§K; [Geography 1.4.2]; “Achievement” p.261; “Secrets” §D) for handy astrologer-access to inevitably-too-widely-spaced klimata tables: Almajest 2.6. (Three centuries later, professional astrologer Ptolemy ruined longitudes systematically, stretching them by factor 4/3 or 7/5. Summary: §3 [11]).

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

T. The history-of-science enterprise is proud of being nonjudgemental, e.g., of superstition. In fact, it’s an important discovery by astronomy that Ptolemy’s selection criteria for klimata–to which he was culturally and historically blinded—were based on astrology. Ironically, this well-intended discipline has long blinded the eld to the obvious: just as his Tetrabiblos was his religion’s handbook for horoscopic interpretation, his Almajest and Geography were also world-astrologer-handbooks. (The 1° fully competent translations of Almajest and Geography called each a “handbook”. Ptolemy’s exact title of what most now call the Geography was actually Geographical Directory, as DIO routinely calls it.)

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

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

W. Both Isis papers cited D.Rawlins 1982, “The Eratosthenes-Strabo Nile Map. Is It the Earliest Surviving Instance of Spherical Cartography? Did It Supply the 5000 Stades Arc for Eratosthenes’ Experiment?”, Archive for History of Exact Sciences 26.2.pp.211-219. But both fail to mention 3 unmissably central and intensely relevant discoveries in that paper and/or “Too-Big” which Isis readers need awareness of:

[i] The Nile Map shows that Eratosthenes’ original circumference C was 256000 stades (later nudged to 252000, perhaps for 700 stades/degree-convenience).

[ii] Eusebius’ Sun-distance, 4080000 stades, is thus 100x (Earth-rad), in the Aristarchos-
Archimedes-Hipparchos-Poseidonios tradition that too-big-for-precision Sun-distance is a power of 10: their 1000r or 10000r, likely origin of the very idea of order-of-magnitude.

[iii] By the correct (now generally-accepted, but still sniped-at) 185 meter stade, 256000 stades is 19% high, near 6/5 of real C; Poseidonios’ & Geography’s C, 180000 stades, is 5/6 low. All the three Rawlins papers which Isis’ authors have profitlessly consulted explicitly stress that air’s bending of horizontal light renders high by 6/5 the C gauged by lighthouse-flame-visibility, while the same air-refraction will make C obtained by timing sunsets (at different terrestrial heights) come out 5/6 low — the double-sunset method. See D.Rawlins, “Doubling your sunsets or how anyone can measure the earth’s size with wristwatch and meterstick,” Am.J.Physics, 1979, 47:2:126-128, p.127. Cited to 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; FrAgep, Archimedes 23 p.124, 2010; Isis 2015 & again in 2016). But, as we’ve seen already, Isis & Shcheglov in 2016 exceed their predecessors, by showing that all the above-noted scientists & forums are Deluded in finding precise ancient geodesy credible.

[Above paragraph’s conclusion added 2017/5/24&6/22.]

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

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

Z None notes DIO’s refraction solution (6/5, 5/6) their own citations prove they know of. No historian-of-science has ever given grasp of its physics [§1 [ii6]. Its triple-consistency (to 1%) with both 40%-disparate C (Eratosthenes-Almagest 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-maneuver) unifying PHYSICAL theory: refraction by air.

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

POSTSCRIPT [Below items originally “Not for publication” but: why shield shunners?): At least six Experts (p.689) vetted Shcheglov. Besides the foregoing weightier problems, how’d the following mostly-minor but mostly-obvious slips elude 6 putative readers?

n.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 Pliny’s 77 AD Natural History is confused with his deathdate.

p.689: Blest Isles’ location matches Cape Verde Islands, not the 800-MILE-DISTANT Canary Islands (one called “Kerne” at Geographia, 4.6.3). Mistake favored by all for centuries since Ptolemy called one Blessed Isle “Kanaria”. (Language over coordinates?)

Try a modern map vs Geographia 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 that the field that this simple fact wasn’t noted before?

Afterword: The History of science Society Hunkers and Bunkers

Due to cultish historians-of-astronomy, Greeks’ patiently-won accuracy is unknown. (Perverse-ironically: it’s widely believed that semi-literate Mesoamericans were better!) Given Editor H.F.Cohen’s haughty rebuff (§3 p.45), a Letter-to-the-Editor (pp.3-8 here), with cover letter www.dioi.org/isa.pdf, was sent 2017/3/20 to the 30-person Isis Editorial Board; separate emails to ordmag 10 board-members (requesting all 30 be informed of the letter), including Maria Portuondo (astrology), head of Johns Hopkins’ University’s History of science Department, plus a message left on her answering machine 2017/6/11. No response. (Asked later to review these doings, her JHU colleague R.Kargon [history of physics] pled too “rusty”: 2017/9/5.) Having heard from neither Editor nor Board, DR wrote the latter 2017/4/1, www.dioi.org/issb.pdf, hoping (emph in original) to encourage communication while correcting [Isis 107.4’s] unfortunate December misinformation, unwary Isis publication of which might have been avoided, had Cohen possessed the humility to recognize he didn’t understand Shcheglov’s [2016 December Isis] paper except that it enticingly attacked one who was upsetting Cohen by asking Isis to publish too-accurate criticisms of his fellow poles. For Shcheglov, Cohen should’ve sought refereeing from not just the usual suspects but from DIO (re, after all, a huge attack on DIO&DR) during a period when Cohen was actually exchanging emails with DIO, but preferred secrecy. Now, instead of owning to errors, he’s coverupping for not just Ptolemy but for Cohen, taking you all into hiding with him.

The Isis board’s non-reply so far risks being interpreted as . . . doing nothing — about mere plagiarism, and miscomputed demeaning of accurate and scientifically refereed [p.45 below] Greenwich-Centenary scholarship. Less speculatively, we know exactly what Cohen was up to, when he did not tell us he was sending the large paper [www.dioi.org/qjo.doc; now less diplomatically transformed into paper §3 below, here] to a referee until he got a negative report safely in hand [a report again not evaluating any scholarship]. Does he imagine such transparent tactics are not noticed by serious academe? Truthseeking institutions communicate. And will not hide their demonstrated miscalculations. And don’t doubly (2015/3 & 2016/12 n.14), knowingly appropriate credit for (a needlessly) rival journal’s discovery. If Isis does not acknowledge receipt of this letter . . . it will be reasonable for previously unenlightened observers to conclude that your society is unprincipled, and you will not hear directly from DIO again.

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

I received your note and have read the attachments. In my view, the decision of what to publish (or not) in any specific case is the prerogative of the editor.

So I’m afraid I cannot help you out here. Sincerely yours, Lynn Nyhart

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

3Cohen email to DIO 2016/9/27: “Never ever is Isis going to publish a paper which already in its very first sentence . . . contains the phrase ‘smothered by a chaunvist 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.