2 The Greatest Faker of Antiquity: Still Foolin’ ’Em

[On 2014/8/26 & 12/22, a somewhat restrained & spare version, www.dioi.org/pf.pdf, of the following paper was submitted to the Journal of Astronomical History & Heritage. Its referee report, while admitting our obvious expertise, included personal remarks (fn 1) echoing religious Ptoleman O. Gingerich’s various past slanderous referee reports upon our work, naturally requesting removal of anything embarrassing to his clique, even offering to take another look at the paper (fn 35: “If . . . DR revises . . . I would be happy to look it over.”) to confirm that the censorship he was ordering had been satisfactorily carried out. So our 2015/9/30 resubmission added extensive notes, responding to such typical intrusion by reviewing — at least for JAHH’s info — the long, 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 for the History of Astronomy in permanently severing communication with DIO: suggesting, as later confirmed, www.dioi.org/owo3l.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 ineligible for censorship. DIO instead agreeably refused to fall into that trap; thus, fleeing was JAHH’s only escape-option to effect pre-ordained rejection. The 2015/9/30 version follows, very slightly enhanced.]

ABSTRACT

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

A INCURABLE DENIAL OF THE UNDENIABLE

Claudius Ptolemy’s Almajest is the central document (§1 below) of our valued heritage from classical antiquity’s mathematical astronomy. Though 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 Serapite state religion at Canopus (near Alexandria) where stood in major temple, which specialized in curing illnesses through astrology&dreams. Ptolemy also authored astrology’s bible, the Tetrabiblos. His employers no doubt appreciated his consistent demonstrations that reality was in perfect accord (§2) with divine celestial theories. However, for centuries, historically savvy astronomers have known that this famous 2nd century AD astrolger-geocentrist mathematician accomplished said illusion by indoor-faking or plagiarizing all his allegedly-outdoor “observations” of celestial phenomena, to force precise accord with indoor mathematical models (some of which had already existed for centuries) and tables computed therefrom. Ptolemy’s depredations even included stealing and mis-precessing Hipparchos’ immortal 128 BC 1025-star catalog (R.Newton 1977 pp.239-242), a deed which for over a millennium polluted astronomers’ attempts to gauge precession, until Tycho in 1598 detected (Rawlins 1993D fn 141) and threw out Ptolemy’s fakes & was thus able for the 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 (§8; 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 & (see Neugebauer 1975 pp.69, 119, 205: J.Evans, 1987-11]
date; A.Jones 2010A p.12) that Ptolemy was actually an honest outdoor astronomer (even below, at §[3] 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 unqualifiedlyvertabim-refarminfing (Gingerich 2002 p.70) his original 40*-ago vertabim echo (Gingerich 1976) of the once-unopposably-dominant mantra (§M2) that Ptolemy was "the greatest astronomer of antiquity" (Neugebauer 1975 p.931).

**B CRIME AND SHUNNISHMENT AND HISTORY-OF-science: SMEAR-REVIEW AT JOURNAL NORMALCITY**

**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 minuscule shortage of perfection in its some don’t empathize with the laughter a genuine scientist can hardly stifte, 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 one-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 Ptolemist could keep doubters from being taken seriously just by portraying them as those who had committed the crime of being "less Ptolemy than," and [h] of merely disparately: June H. J. 1997H §F1C & Rawlins 1998C §F7C & F8C). That the controversy has been rife with "unprofessional" avalanche of discoveries consistently (fn 12) soon after by a direct appeal (fn 35) to the head of the American Astronomical Society, urging supervision of its shamelessly shunning Historical Astronomy Division (H.A.D.), but the unprofessionalism of 2002 has only worsened since. History-of-ancient-astronomy’s lengthy communal monolithic and cranioolithic insistence upon defensively maintaining — by character- assassination (fn 5) & the threat of exile (idem fn 1) for dissenters — are dishonestly, defiling, perception-inverting (§N), logic-blighting (§M), holey-corpse honest-Ptolemy-myth as its ultimate herd-sacred tenet, can only weaken the field’s cred. Same for launching fantastic, irrelevant, and transparently projective descriptions of bemused skeptics as "angry" (Pedersen 1974 p.23) and "bitter" (Evans 1998 p.268). For which side is angry, see DIO 11.1 p.2; also the case of an eminent referee and Jesus-devotee (fn 5) who routinely (fn 1) calls today’s numerous (idem) Ptolemy-skeptics a tiny bunch of paranoids for believing what he himself privately knows to be true. Healthy restoration

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

Ptolemy regularly fabricated data from which he perversely claimed he 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-receoally — for either 1974 Pedersen or JHA Editor Evans. Each could always be counted upon to evade the ugly evidence, trying to make the issue of Ptolemy’s integrity untestable thus irresolvable, so never having to admit their original mistake in defending him. And far and far harder: to face the shame of having for decades (mostly behind-the-back: fn 5) gang-condemned as fools and cranks who turned out to be more prescient (§M3 and fn 12) than their denigrators. Pioneer skeptic Robert Russell Newton is deceased. Nothing can now compensate him for the hateful, largely-whispered bile heaped upon him to prevent his case from getting a fair hearing while he lived: sampling at fn 35, plus MacArthur Genius and amateur scientist Noel Swerdlow’s branding this brilliant physicist a Velikovskian “con-man” (see DIO 1.1 §3[12D-3]; 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 shrunk chauvinist

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 dispised 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

avoidance, 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 [j] that an ancient who was off his latitude by 1°/4 and could not find the Sun within a degree, was a regularly observing astronomer, and [ii] that anyone who disagreed was the insane party (§B1). Equally risible: to be shrankanalysed 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 catalogue 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 53) 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 Skyg; Gingerich (1978c/22) 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 Fraazer 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 ontologial 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 sableae — accuse someone of insubstantiality even while suppressing hrosy evidence (e.g., Gingerich’s own evidence evinced at P.A.S.P. at the head of this note). At Gingerich’s insistence, U. Notre Dame’s 1999/7/5 debate on whether Ptolemy observed or stole the Ancient Star Catalog could not be held unless Rawlins was kept from the platform. Evidently inspired, by decades of toadily awesome H.A.D. worship, to standards of equity and consistency innehurable to the less godly (and strengthened by the surety with which he can count on fellow pols’ reverent silence on his secret actions, some even going so far as to supplant the role of the referee). Of course, Rawlins didn’t actually publish the letter he received another paranoid’s letter regarding said zany’s 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 man.” (S. Stow. Eminent Victorians, 1918, Manning chapter, end of part 2.)

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

Rawlins 1982C p.362 proves that Pedersen positively knew of Delambre’s crucial discovery (§K1), but chose not to impart it to his readers. [See §3 fn 121.]
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Notable features of modern Ptolemists are: [a] Consistent preference (fnn 18 & 50; §N15) for the inherently likely10 over the inherently unlikely11 over the inherently likely. [b] Failure to notice that theories, each tailored specifically and entirely for dodging the latest12 individual Ptolemy-undenying bullet (see also §§6 and fn 18) defies probability — as well as Occam’s Razor, which seeks the single coherent13 theory without confessing. (As with the late Frederick A. Cook Society, some dementia’s only cure is death.)

10 Ptolemy-doubting scientists like Sam Goldstein, R.Newton, B.L.van der Waerden, have all freely admitted in print their occasional mistakes; Rawlins even did so by self-lampoon, right on the cover of www.dio.org/vols/wb2.pdf, DIO 11.2 (2000) [and p.30, p.33 items 5&6]. We await the century when Sweden-Dor-Sweden-Gingerich can display in print the same humility as the “puppies” (fn 1) skeptical side.

11 For examples of downright embarrassingly improbable apology to defend archons’ pro-Ptolemy commitments, see here at, e.g., §§K&L2, fn 37. (As well as Rawlins 1985G n.12; Rawlins 1991W §E3 & fn 99. Cumulative oscillation-oscillation historical review at Rawlins 1992V §C31.) The ultimate far-fetchedness in service to orthodoxy was devised by Evans and promoted by Sweden-Dor 1992 p.177, attempting evasion of the fractional-endings argument (R.Newton 1977 pp.255). The incredible result (Evans 1987 p.243) is spoofed at Rawlins 1992V fn 46 (emphasis in original): “Let’s see, we start by setting [the armillary astrolabe’s] ring 5 NOT on the chosen fundamental star’s ACTUAL [Catalog] longitude at ring 3 but rather at the nearest whole-degree value LESS than [this longitude], for which Evans prefers a 40° ending; then, after sighting the stellar quarter with ring 2, we read where ring 2 meets ring 3 AND THEN [“mentally”] ADD BACK, ONTO THIS READING, THE AMOUNT [40°] WE JUST AS NEEDLESSLY SUBTRACTED OFF IN THE FIRST PLACE. . . . Got it?” And don’t miss that this bizarre Evans scheme not only causes the unnecessary trouble highlighted here (and expands the amplitude of the absent error waves [§H1] from 1°2 to 3°4/!, but would, Evans, loc cit further commit Ptolemy to the extra bother of having to fudge over a hundred (Rawlins 1994L Table 1) stars’ resulting 25° and 55° endings (into 20°s and 00′s, respectively) in the manner shown at §44. As one encounters no less than 64 pages of such desperate resorts, we recall (§BB2 and H2-B2; Rawlins 1992V §§C31-C32) it is intermittently contended that Ptolemy’s plagiarizing the Catalog doesn’t prove dishonesty. But, if so, then: why did the JHDA waste over 100 pages fighting academe’s acceptance of a Catalog-theft that (whenever JHADists momentarily admit it) doesn’t-really-matter? (Iraq war apologists dodge similarly: DIO 18 §F.) For further imaginative excursions, see (Gingerich 1976 in Science) “On Ptolemy as the Greatest Astronomer of Antiquity” and (§M2 Scientific American, 1979, “The Greatest Astronomer,” and thus since these no-worship-sly-titled Ptolemy-advertisements, neither Science nor Scientific American has printed a word on Ptolemy’s frailties; likewise, Sky and Telescope, which has instead repeatedly defended him: 1976 Feb-to-2002 Feb. The vaunted Free Press at work.) Extra community-embarrassments: massive double-Pb-paper Evans 1987 (below, fn 47); top Newton-exiler (below, fn 35) Sweden-Dor 1989, on whose invincible math-innocence (repeated 1979, 1981, 1989, 2010 while reaching for Ptolemy-exculpations), see above, at fn 8 or Rawlins 1991W §E3. Two more cases of scientially ambushmading chronically misinformed½fudger of Ptolemy’s own Venus tables. (Similarly for the Sun §§D5 and the Moon §§G3.)

This C is by far the funniest (and most astonishingly inept fake in the entire history of the oldest science. (But watch Ptolemy again/kgain try mightily to match it at §§E58.G3.)

C VENUS VERSUS VENUS

AND THE VENUS-MADE-ME-DO-IT DEFENSE

C1 The Greatest Astronomer of Antiquity’s sloppiness with his Venus swindles was so extreme that he inadvertently reports (Alm 10.1.2) having 1st-hand “observed” the same event — the 136 AD greatest evening elongation of Venus — on 2 different dates 37° apart (136/11/18&12/25), at 2 different positions 37° apart, and at 2 different maximum elongations from the mean Sun, differing by 7/5 [nearly the Sun’s semidiameter]! 47°1/3 versus 47°16/30. Not only do these data disagree with the outdoor sky, they also disagree with Ptolemy’s own Venus tables. (Similarly for the Sun §§D5 and the Moon §§G3.)

C2 This is by far the funniest & most astonishingly inept fake in the entire history of the oldest science. (But watch Ptolemy again/kgain try mightily to match it at §§E58.G3.)

D ILLEGALLY BLIND: SKY or SLY? MULTICORROBORATION

D1 The Greatest Astronomer of Antiquity’s four solar “observations” (Alm 3.1 and 7) are on average about 50 times nearer15 to his indoor tables than to the real outdoor sky create Reasonable Doubt, Rawlins 1992V §C20 responded: “Besides a range of specific evidence pugnacious, there is the simplicity of that hypothesis’ fit to the larger evidential situation: if we merely assume that Ptolemy swiped the Catalog, virtually all of [defenders’] central purported ‘Enigmas’ (bid §§C22) of the case immediately evaporate.”

10 [Ptolemites are hereby asked to specify any that don’t.]
(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 ordinate 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 positions10 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 dominant, and planetary contexts throughout R.Newton 1977b) for his 3 equinoxes, Ptolemy is alleging 1°-hand visual sightings of the Sun’s center on the celestial Equator at times agreeing to ordinate 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°.) Understand that, while these errors’ ridiculous grossness alone proves fraud, we additionally know EXACTLY (§D5 below) the method of all four fabrications, though Ptolemy presented each as an outdoor observation of the real sky, not a mere indoor-calculation.

NB: No cultist admits that Ptolemy did anything dishonest here.

D3 Further, the tabular Sun’s −65° mean longitudinal error (§D1) at the epoch of Ptolemy’s tables, 137.547 (Antonius Pius Year 1 Thoth 1 [137/7/20] Alexandria Apparent Noon), isn’t a constant in time: it varies by −2°/0.0c. (A rate consistently bi-miscomputed at CalTech’s Swerdlov 2010 p.152, item 3.) So: when is Ptolemy’s Sun correct? Obviously that epoch must be 137.547 + (100°/cy)65°/7 − 23/(cy) = −145, which (as seen at Rawlins 1991W §M6; similarly below at §D5) is Hipparchos’ era, and is indeed the regnal epoch (Ptolemy VII Phoenicus 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 Hipparchan PH solar model’s big 0°-4-amplitude annual period error. (Again: see error formula for Ptolemy’s Sun at Thurston 1998A §C1.) For Ptolemy’s mimicry of not just systematic but even random Hipparchan error, see the glaring case of Arcturus: fn 37.

D5 This unsubtle echo connects to the irony that Ptolemy didn’t fake the solar data via tables but by even cruder means. (He fabricated similarly elsewhere as well: Venus [§C1; Rawlins 1991W fn 166], the stars [fn 37], and the Arbela eclipse [§G3].) As early as 1639 — the year the Ptolemy controversy should have ended (or been doomed to never begin?) — Christian Severin said Ptolemy had merely computed the alleged solar observations from Hipparchian data, and Delambre 1819 (pp.1xvij-1xix) explained in detail how Ptolemy had fabricated his solstice and equinoxes by merely adding integral numbers of Hipparchian 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 c2, reconstructing independent Hipparchos’ 139 AD Autumn Equinox) to recover Ptolemy’s 132 AD Autumn Equinox (Alm 3.7), just add 278 Hipparchian years (365°1/4 − 1/300 each, 6° longer than has been claimed, e.g., in The Acquittal (§M). And in Hipparchos’ case, a ratio something above unity is to be expected since the tables were, after all, based upon his slightly flawed observations, of which he [contra same delusional Acquittal] reports several discordant with theory & each other: [3 fn 8.

10 Ptolemy didn’t have to look skyward to see theoretical data instead of real: his Optics takes perfectly false-theory-accordant refraction angles that are erroneous by up to 2°1/2 or 150 arcmin. (See, e.g., Neugebauer 1975 pp.895-896.)
Moreover, such an error (see math of 5.12 and 13, or Rawlins 1994L eq.1) would carry exactly into the “Clean Dozen” unfudged and unused Ptolemy-contemporary stellar declinations δ reported at Almajest 7.3 (the only honest Almajest star data from Ptolemy’s era). These, however, show (Rawlins 1994L §F9) that the observer’s error20 in assumed geographical latitude L was +4°2′, about 9 standard deviations distant from Ptolemy’s −14°. (Similar disconnect for the 1025 celestial latitudes β of the Almajest 7.5-8.1 star catalog.) So the δ were plagiarized from a contemporary anonymous observer who knew his L.

E3 Confirmation is achieved via statistical induction (Rawlins 1994L §F8) of the exact latitude L = 31°1/4 assumed by the observer of Almajest 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. (In the Gad Almajest’s leapfrog anachronism: the Pharos, an embankment explicitly named Heptastadion [ἐπτασταίδω] Strabo 17.1.6)?

E4 At GD 4.5.76 Ptolemy lists the Alexandria harbor Pharos lighthouse’s L as 31°05′, bithellly copying this false L from Hipparchos-Strabo (Strabo 2.5.39; Neugebauer 1975 p.1313; Diller 1984 fn 23; Rawlins 2009S fn 16). Question: Since his Almajest had already (§E1) put Alexandria instead at 30°58′ (consistent with GD 4.5.9’s S’-rounded 31° value) why didn’t an Alexandrian25 & 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 (ἐπτασταίδω) Strabo 17.1.6)?

E5 So by carelessly25 copying disparate latitude data from Vitruvius (§E1) and Hipparchos (§E4), “astronomical observer” Ptolemy adopted — simultaneously — two L

during decades, have never noticed either?! — which could have saved themselves a half-century of dript tortuous 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-cowelusion-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 born down and attacked with pseudo-scientific, quasi-historical, quasi-scholarly, quasi-New Testament “proofs”, which was precess-faked so inadequately for stated epoch 137 AD that their coherent 5 stars’ mean is (fn 37) instead correct for over 100 years before 160 AD! There are two unknowns when analysing ancient star-declination lists: the observer’s epoch E and the error x in his assumed latitude. For the four observers whose star declinations are discussed in Almajest 7.3, the curious paper, Brandt et al 2014B (discussed also in fn 37), gets mostly non-outre 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 Almajest 7.3 declinations has ever correctly understood how to find both E and x [& associated standard deviations]. The 2014 paper also errs in dropping near-solstitial stars for being weak indicators of epoch — forgetting that they are superior indicators of x, and thus matter in gauging the accuracy of the 4 men’s adopted latitudes for their observatories.

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

22 Yes, Ptolemy clumsily double-lists L values for sites other than Alexandria (e.g., Heliopolis-vs-On and Syene-vs-Elephantine: details at Rawlins 1985G p.260 and n.6). But none were the world’s cultural center he is supposed to (by his defenders) to have been familiar with and from where he reports 1st-hand astronomical observations (Almajest 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, Synopsis (§3 fn 15), as: Cut&Paste. Not in Liddell-Scott-Jones. Yet.

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

E6 Also revealing of Ptolemy’s degree of empiricism is his astonishing listing of the Pharos (§E4) at exactly the same L (E1) — 31° 05′ — as for his home Serapic temple at Canopus from where he had only to look down the Mediterranean coast after dark to see that the 12 nautical mile (nmi) distant Pharos flame was slightly over thirty degrees south of due west, so the two sites’ L could not possibly be the same. (Real L difference: 12 sin 30° = 6°.) Further evidence that Ptolemy “doesn’t seem to have allowed his eyeballs out at night” (Rawlins 1985G p.266).

F IMPERVIOUS MERCURY

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

F2 But the difficulty for Ptolemy’s loyalists is this: his Canobic Inscription, written some years before the Almajest (as proven in the brilliant paper, Hamilton, Swerdlow, & Toomer 1987), listed precisely the same Mercury mean motion, but most of the other elements differed. So: how could the mean motion have been empirically and mathematically based for both works, if the respective derivations involved discrepant elements? E.g., deriving the 139 AD position for the Canobic Inscription elements versus doing so using the Almajest elements, produces results disagreeing by over 5°. Yet The Greatest Astronomer of Antiquity gives the identical mean motion in both works, to six sexagesimal places, and this route has been exclusively rewarded (fn 28) by the JHAD, throughout The Controversy, and which has been challenged, with pseudo-scientific, quasi-historical, quasi-scholarly, quasi-New Testament “proofs”, which were precess-faked so inadequately for stated epoch 137 AD that their coherent 5 stars’ mean is (fn 37) instead correct for over 100 years before 140 AD! There are two unknowns when analysing ancient star-declination lists: the observer’s epoch E and the error x in his assumed latitude. For the 4 observers whose star declinations are discussed in Almajest 7.3, the curious paper, Brandt et al 2014B (discussed also in fn 37), gets mostly non-outre 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 Almajest 7.3 declinations has ever correctly understood how to find both E and x [& associated standard deviations]. The 2014 paper also errs in dropping near-solstitial stars for being weak indicators of epoch — forgetting that they are superior indicators of x, and thus matter in gauging the accuracy of the 4 men’s adopted latitudes for their observatories.

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

22 Yes, Ptolemy clumsily double-lists L values for sites other than Alexandria (e.g., Heliopolis-vs-On and Syene-vs-Elephantine: details at Rawlins 1985G p.260 and n.6). But none were the world’s cultural center he is supposed to (by his defenders) to have been familiar with and from where he reports 1st-hand astronomical observations (Almajest 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, Synopsis (§3 fn 15), as: Cut&Paste. Not in Liddell-Scott-Jones. Yet.

G THE ARBELA ECLIPSE: FUMBLED PLAGIARISM:

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

G2 Unless isolated from scientists of his world (a serious probability [fn 26 & §1 F], with serious implications), Ptolemy had dozens of contemporary eclipse-comparison reports at his disposal. (Almajest 4.6 and 9 use several eclipses of the 120s-130s.) But corresponding
foreign eclipse times couldn’t have supported the longitudinally-stretched geography (§G4) he borrowed (with credit)25 from Marinos of Tyre. Instead, Ptolemy’s vast opus provides (GD 1.4.2) but one26 example: two longitudinally much-separately reported — 500’ old! — of the famous Arbela — 330/9/20 lunar eclipse’s start, saying it was seen there at 23° and in Carthage at 20°, thus proving that the 2 places are 3° or 45’ apart in longitude.

**G3** However, Pliny earlier reported the same data very differently: 20° (8 PM) for Arbela (modern embattled oil-city Irbil) and 18° (6 PM) for Sicily, whose west end — major city Lilybaeum — was part of the Carthaginian empire, and of longitude similar to Carthage. Modern calculations27 show that non-astronomer Pliny was quite accurate, while The Greatest Astronomer of Antiquity was amazingly wrong, over 2°5 off for Carthage, 3° off for Arbela. The former error nearly equals the entire actual 2°1/4 longitude gap between the sites, and the latter error far exceeds said quarry. But the weirdest part is yet to come: Ptolemy’s own lunar tables put the eclipse just about as much in disagreement with his reported times as modern tables do: 2° Carthage and 3° Arbela. How explain such an entertainingly disastrous fabrication? Start by consulting Pliny 2.72.180 on the −330/9/20 lunar eclipse. Reading the passage carefully, one sees that no numerical hour is given explicitly for the western apparition in Sicily, merely: moonrise ("exorions"). By contrast, the Arbela time is given as the “22nd hour” after sunset, or about 20°, which is the very time Ptolemy gives for the Carthage report. Why? Well, look carefully at the Pliny passage cited: by a fluke of grammar, “secunda hora” appears nearer in the sentence to “Sicilia” than to “Arbelam”. This obviously suggests that Ptolemy used Pliny or his source but (evidently unable to read Latin well) took Pliny’s 20° time to be Carthage’s.

**G4** But how did Ptolemy arrive at 23° for Arbela? Since Gossellin 1790, it has been obvious that multiplication by an expansion factor (Diller 1984 SC5) had been applied by Marinos or Ptolemy or their source to a prior map’s accurate longitudes, creating the oversize longitude intervals of the GD. Rawlins 1985G eq.15 showed that the expansion factors were either 7/5 or 4/3 in the region under consideration. Assuming that the earlier accurate map correctly put Arbela 2°1/4 east of Carthage, then expansion by 4/3 would produce 3°, the very gap — the very wrong gap — Ptolemy reports. I.e., typically for him (and his defenders) the conclusion was established ere the evidence was engaged.

**G5** He simply added this 3° to 20°, thus arriving at his fantastic 23° time for Arbela.

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

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

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

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

26 Due to modern communal non-recognition of occult Ptolemy’s 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#cknh) there then existed no empirical scientific community to be isolated from!

27 The Battle of Arbela was fought at nearby Gaugamela and 11°4 after the eclipse. We find actual Local Apparent Times of the −330/9/20 eclipse’s umbral start: Carthage 17:43, Lilybaeum 17:52, Gaugamela 19:56, Arbela 19:54. So the Gaugamela-Lilybaeum difference in geographical longitude E is ΔE = 2°04’; Gaugamela-Carthage, 2°13’.

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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°72 in the Catalog’s latitudes β (cosine waves: tibid eq.4) and northern longitudes λ (sine waves: tibid eq.3). We don’t. (Amusing details at Rawlins 1992V §§C13-C15 & fn 31). Also see the inspired findings of Graßhoff 1990 — which instantly converted dedicated and scholarly Ptolemist G.Toomer — as well as the perceptions of Duke 2002C, all of which combine to show that, e.g., errors in Hipparchos’ stars are statistically quite discernable in the Alm’s, including a few ultra-giveaway cases where a star with an error of several degrees is found to have the same sized error, with the same sign, for both Hipparchos and Ptolemy.

**H2** It thus became obvious c.1990, even to the most religious, that many Ptolemy stars were Hipparchos fakes. However, no archon was ever going to admit in print the plain truth: the establishment had been blinded by proof that its challengers had been right all along — that Graßhoff’s test had now unexpectedly surprise-vindicated the long-loathed Tycho-Newton-Rawlins position that the Catalog was stolen. (The post-disaster spin of some was the judicious archons atop the American Astronomical Society’s Historical Astronomy Division [H.A.D.] — surely a truth-determination criterion to live by — while Graßhoff had. Which translates as: [a] ashen refusal to acknowledge that — until the truth hit them in all their faces — believers had been too limited and predisposed to see anything significant in the same evidence from which skeptics had drawn the right conclusion years ahead of final proof; [b] denial of credit to unapproved first perceivers, according to a principle handed down to us from on-high, the JHA itself [quoted at Rawlins 1991W fn 127, emph added], “the first speculative occurrence of an idea is generally far less significant than its later emergence, possibly in other hands, supported by persuasive arguments.”) The post-Graßhoff era has been especially fertile for indiscriminate fallacy backpals by Ptolemy’s (selectively) malleable modern choir, as the politically ambitious realized that the JHA would ever so gratefully publish anything that muddied the clear evidential situation, in order to save archons from facing apt appreciation for decades of falsely denigrating now-vindicated scholars: simply pretend vindication either never happened or isn’t 100.000000000%. Question: is anyone empathy with the cornered defenders’ needless pain here, caused by artificial extension of the Controversy? (Puts one in mind of equally needless ongoing misery from other kinds of poverty than intellectual — mass-agony likewise of insufficient concern to those who subsidize its perpetuity, to ensure their own perpetuity in ofce.) This is so literally pathetic — Chaun’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 all29 stars were copied from Hipparchos. [2] Schaefer (2002) says the Yale Bright Star Catalog also grabs previous catalogs’ stars, so what’s the concern? However, both these defenses of Ptolemy’s integrity plainly founder upon his claim of 1st-hand observation of all 1025 stars. And argument [2] is on the logical and ethical level of defending a bank-robber by pleading

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28 Rewards handed out to those who attacked the R.Newton satan include JHA boardship (R.Newton 1991 fn 2) and a MacArthur for miss-swordlow. (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 barrage 1998 and 2001-2002 attacks on Rawlins (on Rawlins’s non-occultist isolation from actual astronomers). The plausity here may actually be deliberate.) Selecting boardmembers by such criteria will damage mean-IQ atop JHA for decades to come.

29 D.Duke’s statistical studies indicate that very nearly all stars were appropriated. If Evans and Schaefer were right that Ptolemy observed a substantial section of the Catalog, then the error-correlation dot-diagrams of Graßhoff 1990 would exhibit an obviously disjunct mix of superposed shapes: circular (stars observed afresh) and elliptical (stars copied from Hipparchos). But the diagrams are instead just elliptical. (Even if otherwise, this would prove only that someone other than Hipparchos — not necessarily Ptolemy — observed the stars whose dots mapped circularly.)
that, well: doesn’t everybody withdraw money from banks? The BSC does not claim 1st hand observation, while The Greatest Astronomer of Antiquity explicitly 30 does 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” JH-A.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 assuring the world are crazy dishonest paranoid incompetent cranks. And we’re not supposed to giggle? (You begin to see why the seething losers can never admit it?)

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

H4 Several Sagittarius (Sgr) non-dim stars were missed by Tycho, so Evans 1987 p.168 (like Evans 1998 p.272) tries to create antarctic-circle ambiguity by stressing that these stars — well over 4° high, in what JHA Editor Evans calls “Sag” — are not in Tycho’s catalog, an argument put forth in innocence of the fact that Summer Solstitial non-darkness at the Dare’s northerly latitude L = 55°.9, impeded these difficult Sgr stars’ availability. (By recording Fomalhaut, Tycho actually went down to within 2°.6 of the horizon: Rawlins 1993D Table 17.)

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

I1 The Ancient Star Catalog (Alm 7.5-8.1) has an obvious excess of 0° endings and 30° endings in the latitudes β, due to ancient Egyptian and Greek proclivity for expressing non-integers by using inverse integers: “unit fractions”. (Cause of both excesses detailed at Rawlins 1994L §4B.) But the most common ending for the longitudes λ is 40°.

I2 R.Newton 1977 (pp.245-254) showed statistically that this odd circumstance was simple to explain, once he discovered the key and unlocked the longstanding mystery: when Ptolemy stole Hipparchos’ stars, he naturally left the Catalog latitudes β unchanged, while updating all Catalog longitudes λ: 1°/century-precessing them by adding 2°2/3, the false figure which Alm 7.2-3 claims stars precessed during the 2 2/3 centuries between the Catalog epochs of Hipparchos and Ptolemy, −126.278 (Rawlins 1994L fn 45) and +137.547 (§D3), respectively. From slyding each longitude λ by 2°40’, 0° endings became 40’s; 10’s became 50’s; 15’s became 55’s and were rounded to 00’s; 20’s became 00’s; 30’s became 10’s; 40’s became 20’s; 45’s became 25’s and were rounded to 20’s; 50’s became 30’s. (Note how the odd endings 25’ and 55’ got eliminated.) The frequencies of endings in λ and β are displayed by Rawlins 194L Tables 1 and 2, and the whole slyde&hyde process is verified via χ² test (ibid §§B-C).

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

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 0° endings) in the Catalog longitudes λ, but (thanks to the 00° ballot-box being [deliberately?] stuffed with the entire sample of rounded 55°’s) the most shocking frequency-contrast is elsewhere (Rawlins 1992V §C22 item [e]; Rawlins 1994L fn 5): the spectacularly greater number of 10° endings than 30° endings. (Before Ptolemy added 2°40’ to Hipparchos’ λ, these were 30’ and 50’ endings, respectively.) Looking naively at the tabular distribution, the fact that 30’s are the least frequent endings is bizarre, since 30’ is nearly the most frequent latitude ending, as it should be. Indeed, for the latitudes β, 30’s are roughly twice as common as 10’s, but this is reversed for the longitudes λ, by far the strongest confirmation of R.Newton’s hypothesis for explaining the longitudes’ odd fractional-endings distribution.

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

I5 So R.Newton (§J2) explained why longitudes λ are near-benefit of 15’s and 45’s (only 32 5 in all: five 15’s, no 45’s), though appearing with roughly expected frequency for latitudes β: twenty-eight times more often than for λ.

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

I8 Some Ptolomeists 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 PedaDodgical 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 (conflating their occasional over-optimism with Ptolemy’s flagrantly consistent M.O.). But, again, among these, only our Greatest Astronomer of Antiquity ever stole a thousand stars — the factor that (as in §J8) separates the “merc” Fudger 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 1998C fn 108) were three lunar eclipses (Babylon, Hipparchos, and perhaps Menelaos) and twice Venus.
J CONCLUSIONS

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

2. The most educational observation we may end with, regarding the timorous state of the modern history of ancient astronomy community, is this: its fiscal rulership can read all that you have just read, though finding not a digit out of place in the ancient catalogues or charts (in the modern sense). If [A] Ptolemy has done absolutely nothing (except for not having put the data to such use as might have been made of them), then [B] Dr should continue to be non-cited34 for [1] his witchcraft (fn 35) in co-hypnotizing scholars (fn 1) into realizing Ptolemy cheated, & [2] exposing the vile tactics of archaeologists who'll never admit they were wrong to slander35 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 number of useful facts of earlier 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 [1] 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 *identem* (data that Ptolemy reports from others are faked as 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 shunned is forever worthless — oblivious to the possibility that his output may prove valid (or later to start), at which point, how did the bully-invested, no-turning-back shunner then justify continued non-citation? For saving faces (and what else matters to archaeologists?), he has no choice but to continue to shun — not knowing (as he gets progressively deeper-in) when if ever the deceiver can stop. As we get to the point where evidences CENTRAL TO THE FIELD (e.g., §§111-N17 and N18; fnn 1, 7, & 47 item [1]) cannot be openly discussed without fear of archaeologists — or editorial submission to such (fn 1) — we've descended into knowledge-destructive sociopathy.

15 Among *JHA*-circle-herd-talk companions toward R.Newton and his solid mathematical analyses have been George intersection-point ("incompetent") (A. Almajest [7.3]), and 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L APPENDIX 2: THE MAGNITUDE SPLIT
    (AND WHY WASN’T PLUTO KNOWN TO ARCHIMEDES?)

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

L2 A passing allusion by Evans 1987 p.166 even imaginatively hints that perhaps there were, say, rocks just-south of Ptolemy’s putative observatory that just-so-happened to block just-enough southern stars as to make his putative observations’ declination range deceptively look29 as if the observed were at Hipparchus’ latitude L instead of where Ptolemy’s

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various of the non-cataloged Alexandria-visible stars were conveniently attachable to nearby constellations. And we know that Ptolemy was (or copied) a star-attacher: Alm 7.5-8.1 lists dozens of “informatae” stars which are in the vicinity of traditional constellations though still outside them, but which he nonetheless appends to them. This includes even Arcturus. (Which we designate as PK110 — meaning star #110 in Peters & Knobel 1915.) Further, the vast constellation Argo (today broken into pieces: Car, Vel, Pup, etc) had already been recognized for centuries, and the Catalog includes 45 of its stars (PK849-893); yet several Argo stars aren’t in the Catalog (but unCataloged ε Car [m = 1.9] is less than 6º from Cataloged δ Vel: PK886), despite being easily visible from Ptolemy-era Alexandria (per-argument null c.50º after the LMO, should just-happen-to-also-be the lone Hipparchos star whose δ-error also goes null c.50º later than Hipparchos. (To be exact, 52º after Hipparchos; 56º after LMO.)

Hipparchos’ μ0 was a bit dimmer than 5: §3. For his deep south sky, virtually every identifiable star brighter than μ ≈ 5 appears in the Catalog. Note: this is about the μ (Rawlins 1993D §44) at which Tycho’s normally high star-cataloging accuracy begins to fade. (Tycho had 14% less sky visible to him than Hipparchos, so he had to go a bit deeper in μ to approach his goal of netting 1000 stars.)

Evans 1984 had argued for Ptolemy’s originality with the equant, but only 33º later is adding his μ0 invisibility to squirm out of a religious paradox. See Swerdlow similarly at Rawlins 1992V fn 43.

40 Not just the date argues for Ptolemy’s Seraphic temple being the source of Antinous’ intermittent celestial immortality: additionally, there is a copy of that very temple in the Canopic Vale of Hadrian’s Villa, outside Rome. Go to www.dioi.org/cot.htm#rh2v for further analysis, plus Rawlins photos of Hadrian’s Canopic temple replica (with poolside stone crocodile), as well as of a classical-era Antinous statue (肿th from Adrian’s grave).

41 Has it previously been noticed that not one of Antinous’ six stars appears in the Hipparchos Commentary’s αλ, or indeed, anywhere else in that work? The stars were PK295-300: or from Bayer: η, θ, δ, ε, κ, λ αλ. (Three of the six endings are 40º or 10º, vs two randomly expected, a slight hint that Hipparchos observed them after writing the Commentary. ) The group was named for bereaved Emperor Hadrian’s recently Nile-drowned teen boytoy (Rawlins 1992V fn 44) and presumably to thank the emperor for his visit and for favoring (perhaps initiating) imperial sponsorship of the Seraphic astrological-medical superstition the Canopic temple specialized in.

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29 Likewise, Schaefer 2001 proposes that atmospheric aerosols instead of rocks blocked Ptolemy just enough to fool us into mis-concluding that most of the catalog was observed at about Hipparchos’ Rhodes Island L = 36ºN. 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 antique circles of the stars collected by Hipparchos and by Ptolemy) his argument inevitably requires that Alexandria’s aerosols were much greater than Rhodes’ — again, conveniently, by enough for pseudo-indicating a Hipparchan latitude for the Catalog’s main observer. Dense turbidity proponents Schaefer and (less incautiously) Evans propose opacities that are obviously over-high for antiquity. Schaefer (op cit) choosing an opacity of 0.15 mags/atm seems to put the star γ Ara at a distance where it cannot be distinctly verified by Rawlins: probability of Hipparchan capture P < 1/1000. The only way to make it remotely possible (for 0.23 mags/atm) that γ Ara could have been recordable by Hipparchos would be to adopt (instead of Rhodes City’s L = 36º4”) the Rawlins 1994L §46 Hipparcan position (§16) for observing southern stars at Cape Prasonessi (L = 35º53”, vertical distance above sea level z = c.200m), making μ < 5.2 α (Schaefer has never acknowledged that Prasonessi could’ve been Hipparchos’ south-Rhodes observation post. (See discussion at Pickering 2002A §§B2 etc, regarding Schaefer’s intelligent [if only slightly mitigating, in this case] argument that γ Ara’s low P should be seen in the context of several other similarly situated stars [too few of which are of identifiably bright magnitude], even while he himself remains impervious to the larger context of Pickering op cit’s numerous other strong, mutually-verifying clear-weather proofs.) Still at Prasonessi: Evans’ preferred 0.2 mags/atm makes μ = 5.8, P = 6%. (But he is credibly willing to admit the possibility of opacity as low as 0.17 mags/atm, which would leave μ = 5.4, P = 1/3.) Schaefer’s impressively-published case for an opacity which inadvertently worked against Hipparchos’ recording γ Ara makes an even more impressive impact upon us when we learn that Hipparchos actually did record γ Ara: it’s found at Hipparchos’ Commentary 3.2.6 (which Schaefer was unaware of at this time, since he didn’t consult that central work until Pickering told him about γ Ara face-to-face at the 2001 H.A.D. meeting). By contrast, γ Ara’s visibility to Hipparchos is reasonable by Rawlins’ 0.15 mags/atm opacity, which has the star’s μ at 5.2, very near Hipparchos’ capture-limit μ0, with capture probability P = 1/2. (See Hipparchos-capture-probability-function by Rawlins 1982C p.363 — and its later independent 2011 confirmation at www.dioi.org/cot.htm#ppbb, explaining why Hipparchos counted precisely seven Pleiades.) Similarly, we check opacity from α Car’s attested (Strabo 2.5.14) visibility to Eudoxos at Knidos (Rawlins 1992V μ = 3.2, while Wandke’s 2003 3.3 mags/atm makes the entire thick-weather line-of-alibiing was squashed by Pickering 2002A (the best paper, ever, on this topic). The altitude of this (ibid §D9) is the 1º3漫步 (for this case) of Eudoxos’ sighting of α Car (h < 1º), as also of Hevelius’ recording of υ1 Er (ibid §14) — both far too dim for visibility by Schaefer’s opacity picking. (Pickering op cit §F) further demonstrates that bright stars were visible on the horizon in antiquity — most spectacularly by revelation of the hitherto-unconsidered fact that Hippocrates’ and Ptolemy’s achrony limitation/setting data for Arcturus and Saturn has been to refer to these objects’ ancient visibility ON the horizon since achrony effects cannot even be defined at
defenders argue he really, really might’ve been.
L3 Hmm. Why do partisans allow their enthusiasm to proffer already-vulnerable-enough arguments without even testing them? Here, one need only, both for Hipparchos’ and for Ptolemy’s epoch and latitude, list the sky’s stars (bright enough to be clearly identifiable in the Catalog) in order of (a) post-extinction magnitude \( m \) and (b) apparent altitude \( h \) above the horizon. If, in Ptolemy’s list [b], all the stars above \( h = 6^\circ \) are in the Catalog while all below are not, then the rocks aren’t in the apologist’s head but actually existed. Yet, test [b] fails. (For both ancients.) By contrast, adopting an atmospheric opacity appropriate to the best nights (when else would one search for dim stars?) near Rhodos’ southern tip, Cape Prassonesi (see fn 42 for geographical latitude \( L \) and height \( z \) above sea level), Hipparchos’ list [a] exhibits a startlingly clear\(^{22}\) split at a post-extinction magnitude \( m_\alpha \) slightly less bright than 5 (obviously his effective limit for capture): the stars dimmer than \( m_\alpha \) are not in the Catalog, while those brighter than \( m_\alpha \) are.\(^{44}\) Comparing these sensible results, to those gotten from applying the same Magnitude Split Test (DIO 9.1 1999 p.2) to The Greatest Astronomer of Antiquity’s Alexandria, will (fn 43) give any scientist a hearty upchuckle.

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

M1 How did too much of the academic establishment get sucked into promoting astronomical history’s presumed pretender as the “Greatest Astronomer of Antiquity”? [a] Were public attacks on a famous scientist resented by science’s politicians as endangering science funding?\(^{25}\) — but astroleger-mathematician Ptolemy was not a scientist. Not empirical.

\(^{22}\) any other altitude than \( h = 0^\circ \): see the lucid and irrefutable discussion at ibid [FN11]. Further, thanks to my astroleger-mathematecary by B. Goldstein, we now have the fact (Rawlins 1993D §8) that Ptolemy was said in so many words that 1st magnitude stars (pre-extinction \( m = 1 \)) were visible on the horizon in antiquity. (In the exchanging-frauds tradition noted at §K1, some loyalists disebelieve 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 Ptolemy’s value similars to that of an ancient catalog along with the “greatest of antiquity”\(^{14}\) magnitude \( (\mu = 14) \). So: why didn’t Archimedes beat Clyde Tombaugh to the discovery of Pluto?

\(^{23}\) Define split-Vagueness V in §L3’s list [a] as: dimmest Cataloged star’s \( m \) minus brightest non-Cataloged star’s \( \mu \). Testing Hipparchos’ \( (\gamma \text{ Ara vs } \epsilon \text{ Cru}) \) at Cape Prassonesi (height \( z = c.200\text{ m above sealevel} \)) for five assessed measurable opacities: 0.14 mags/atm (negligible aerosols), 0.15 (Rawlins 1982C), 0.17 (Evans 1987, #2), 0.20 (Evans 1987, #1), 0.23 (Schaefer 2001): \( V = 1/5, 1/4, 1/3, 1/2, 3/5 \), respectively.

\(^{24}\) Among those tested by Rawlins 1982C Table II, the only major star (\( m < 3 \)) that seems unambiguously to be missing from the Catalog is \( \mu = 2 \text{ Vel} \) (\( m = 2.7; \mu = 3.1/4 \) for Hipparchos, 3 1/5 for Ptolemy). A speculation at DIO 4.3 §14 showed how star PK964 could be a mangled version of a position originally based upon an hypothetical observation of \( \mu \) Vel, high by \( 1/34 \) in R.A. (3° great-circle), but in declination fully accurate to ancient precision.

\(^{25}\) The half-century Ptolemy Controversy should have been over in half an hour — had all participants amicably coordinated at the outset and sat down together to compare data and enlighten each other. Had defenders realized early on, before positions hardened in ignorance of, e.g., the significance of Ptolemy’s long abandonment of Alexandria’s geographical latitude \( L \) (§E1) and real scientists’ high-accuracy achievements (§M3), participants would (Panglossianly assuming minds all around) have soon shaken hands, parted in peace — and moved on to more challenging historical mysteries. (As DIO long since has, most of our research on ancients being reconstruction of lost astronomy.) Instead, Ptolemy’s promoters from day-one followed his example by knowing all the answers before consulting either empirical evidence (as against texts) or actual able scientists (as against cult gooroos).

[\( b \)] 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 \( \text{Alm} \)’s spherical trigonometry proofs were original — until 1901, when it was found that they were taken from Menelaos (c.100 AD): Pedersen 1974 p.73 n.9.

\[^{26}\] Inevitably-feebly 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-great-cow value trigger the shunning ([E2] for the Reformation-germinating mistake of trying to defend by reason that which cannot be defended by reason) of Ptolemy’s fudges, by deliberately (fn 46), falsely claiming that everybody-did-it ([E2] for the Reformation-germinating mistake of trying to defend by reason that which cannot be defended by reason) by suppression, banishment, indiscriminate argumentation, and circulation of way-overdone baseless or irrelevant personal denigrations against opponents. So when we see such phenomena we should sense said weakness. As a general rule that can save plenty of time and bother:

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

M2 Admittedly-non-peer-reviewed Scientific American’s Swerdlow-Gingerich-inspired premature “Acquittal of Ptolemy” (ScAm 1979), published in anti-Thoughtcrime horror at R.Newton’s scientific 1977 exposure of Ptolemy’s career of fabrication, could hardly have been more ill-timed (fn 12) or more extreme in fundamental-premis misunderstandings of Ptolemy in particular and ancient science in general (most of which survive immutably to this day among his remaining band of believers). It verbatim-echoed the already-echoed ([E2] for the Reformation-germinating mistake of trying to defend by reason that which cannot be defended by reason) playing to an amazing ms-recovery by B.Goldstein, we now have the fact (Rawlins 1993D §8) to The Greatest Astronomer of Antiquity’s Alexandria, will (fn 43) give any scientist a hearty upchuckle. How did too much of the academic establishment get sucked into promoting astro-scientific history’s presumed 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 astroleger-mathematician Ptolemy was not a scientist. Not empirical.

\[^{27}\] So-called “observations” repeatedly in error by ordmag a DEGREE (see, e.g., §D [“Illegally Blind”]), and especially at fn 47) obviously never happened in the 1st place, so there were never any Ptolemy observations to select among. [b] The durable Neugebaurian mantra that effectively-dishonest data-selecting was standard behavior for The-Greatest ancient scientists: [a] cannot survive 5 seconds of critical examination ([M3]), and [b] is based on circularly taking astrologer

\[^{28}\] the leader of the losing army begins also to lose track of priorities and will not give up until the enemy is knocking at the bunker door. During this final phase of real wars, millions die. For nothing. But the leader’s vanity. In a hypothetical academic war, the whole sub-field could be made for decades to look foolish. For nothing. But, luckily, academe has no vain leaders. So it never happens.

\[^{29}\] the leader of the losing army begins also to lose track of priorities and will not give up until the enemy is knocking at the bunker door. During this final phase of real wars, millions die. For nothing. But the leader’s vanity. In a hypothetical academic war, the whole sub-field could be made for decades to look foolish. For nothing. But, luckily, academe has no vain leaders. So it never happens.
Ptolemy as the quintessential or ultimate ancient scientist — knowingly46 rejecting the inconvenient fact that his genuinely empirical predecessor Hipparchos, though also motivated by astrology (at least in the period — 157 to — 145), published theory-discordant data, solar (fn 15), lunar (Alm 4.11), and stellar (fn 47). The attempt to 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 praise or to condemn the ancients for the accuracy or for the errors in their numerical results. What is really admirable in ancient astronomy is its theoretical structure...” See also Neugebauer on Aristarchos’ data as non-empirical (Neugebauer, op cit pp.642-643; Rawlins 2008R §A1). How then did Aristarchos and Archimedes (idem & Rawlins 2012T §E1) find the solar diameter to ordmag 1? How did ancients find the mean distance within c.2% (59 Earth-radii: Alm 5.13)? Or find their observatories’ geographical latitudes to ordmag 1? (See [§4 Table 1] or Rawlins

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

47 To back The Acquittal’s fantastic crock that ancient scientists weren’t accurately empirical, Evans 1987 reports that on 1981/7/16 he observed from Seattle the longitude of star Λ Sgr by measuring via cross-staff its angular distance from a lunar eclipse and found that his result was off by 2°/3 of a degree, nearly triple the angle from lunar center to limb! This error is about equal to that of indoor-Ptolemy’s inaccurate 135/100 lunar radius (by repeat reading fraud by R.Newton 1977 p.190), but (marginally more relevantly) also similar to outdoor-Hipparchos’ huge errors twice (Alm 3.1) when also comparing a star (Spica) to the Moon (during eclipses of — 145 & — 134). Pointing to all 3 large misses in the eclipse-star observations (by himself&Hipparchos), Evans triumphantly concluded: “No better demonstration could be wished” of 1° uncertainty in naked-eye observations, preaching that such Real Science should work. “Ptolemy-skeptics’ judgements regarding the precision achievable by ancients, not-veryimplicitly asking: So, Msrs. Newton & Rawlins — NOW what? so criminal about Ptolemy’s one-degree-erroneous observations? Evans’ and Hipparchos’ three large outdoor errors clearly vindicated the Acquitters!”

And all the many JHA experts who had for months vetted and refereed Evans 1987! Until Rawlins 1991W fn 288 and Rawlins 2009E §A & fn 7 applied the theory that Evans and Hipparchus had simply committed a sign-error in parallax when reducing each observation — a theory that decades later also proved just as neatly fruitful for Regulus’ hitherto-inexplicably huge — 35° Hipparchan error (§§B3&B6). Recomputation showed (ibid) that all four ordmag-1⁴⁶ observational errors of Evans (Seattle 1981) and Hipparchos (Spica twice and Regulus) shrink to ordmag 1. (I.e., all four errors were primarily those of reduction, not observation.) Warned of his sign error by Rawlins 1991W and in 1997 by Thurston&DR, while Evans 1998’s text was still unfinalized, its pp.257-258 ever-so-subtly Memory-Holed his 1981 no-better-demonstration data and switched to a different eclipse (no observed data recorded: from 1977 Spokane, an eclipse never mentioned [Rawlins 2009E fn 7] during Evans 1987), continuing, while sticking to only Hipparchos’ slips, the same Sermon-on-the-Muff (§§B4), just reprinting his 1987 argument (Evans 1998 pp.256-259) header “HIPPACHUS [sic] AND PTOLEMY ON PRECESSION”) & kept pretending Hipparchos’ Spica data showed Greek observational unreliability. [Irony. Evans has debased himself FOR NOTHING: these random goofs can’t alibi Ptolemy’s systematic fit-to-theory errors.] All while non-citing Hipparchus’ usual accuracy or Rawlins 1991W fn 288! No-better-demonstration-could-be-wished [1] of the JHA Assoc Ed [now Editor!]’s citation integrity (DIO 8 1998 p.2) & [2] of cultist disconnect between evidence & conclusion. My former colleagues, the latter stands entirely. Their methodology also durably evident throughout the revealingly flip-flop-history (Rawlins 1992V §§C31-C33) of the Ancient Star Catalog controversy. Today, Evans continues (Rawlins 2009E §A2; DIO 9.1 1999 p.2) decades of evading Thurston’s & Rawlins’ questions on the matter. (In 1997 June, DR asked Evans face-to-face. Evansation. DR then asked Evans for his office phone number so the two could confer. Evans refused. And JHA’s determination to shun permits Evans to face no consequences for such stealth. Other than 2013 appointment to JHA Editorial Board. Like Gingerich (§A fn 5), Evans cannot ever be shown wrong by non-cult outlanders. [On the of-course-Disappeared “notes from that [eclipse] evening”; §4 §B6.]

1994L Table 3). How could Hipparchos measure all of his 3 eclipse-based-star-longitudes (Rawlins 2009E to similar precision? (Note, too, the 3 neatly-interconnected 1% hits hypothesized at §N10.) How were solstices fixed ([N7] to ordmag 1⁴³? Whence arose a Greek stade-length consistent with knowing the Earth’s size to 1% (fn 49 below)? Was it just a series of miracles that all 4 surviving ancient collections of star declinations are (Rawlins 1994L §§F5-F9 & Table 3) consistent with each of the 4 observers knowing his latitude to ordmag 1? From where (Martian visitors?) did the ancients obtain all 3 lunar months to (Rawlins 2018U §D) one part in ordmag a million or better? For the synodic& draconitic months: nearer ordmag 10 million! (Most of these accuracies were unknown before DIO.) It would not have been possible for these measures to progress to such admirable accuracy, if researchers had merely tailored data to previous values. Back in 1979, did 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 ([3 fn 34 & §4 fn 2) prominent Johns Hopkins physicist R.Newton, whose analyses of Ptolemy employed math which historians-of-science couldn’t even understand much less perform. Before such fateful investment, did they even know (fn 12) of the inescapable (§F2) Mercury inconsistency, or (§H1) the absent-error-waves-test? Can Mussfios they show they ever even looked for an explanation of those remarkable million-month-precision lunar-period accuracies (§H10), 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 mestassembling — initially from preconception and careless science, then in heedlessly hot outrage at heretical challenge (Swerdlow at §B2, longtime JHA Editor M.Hoskin at fn 3), eventually ever more stubbornly and far-fetcheded as contrary evidence mounted (fn 50) — ultimately got itself regrettably mired down ever deeper into a spectacularly ludicrous position, in which his has still not even started to recover.

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

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 — what that infallible judiciousness so amply sampled hereabouts — and exclusively publish, those who are equally reliable, trustworthy experts, as distinguished from those crazies who should be blackballed, exiled, unpublished, uncited. However, our question to deities isn’t: have they ever had the faintest idea of how an ancient astronomer actually worked, observed, reasoned, and achieved? (Or: have they ever sufficiently acquired a scientist’s attitude, for appropriate empathy with the scientists of yore?) No, the germane question is: have how 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 retardants 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 unreal field:
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N1 Asserting that Ptolemy got his astronomical elements from his “observations” — the central JHAD-inversion of this controversy. Contra: [1] A particularly penetrating yet simple demonstration of the truth is due to Thurston 1994p (§D), who noticed back in the 1940s that Ptolemy’s iterative proofs of planetary orbital eccentricities start with highly precise estimates, but by the final iteration they’re round as can be. Real iterations proceed in the reverse direction. [2] Another instance of JHAD inverse-perception of ancestry-direction is shown below at §N15 item [F]. [3] Mars’ Alm mean synodic motion is accurate to ordmag 1°/century, yet the “observations” on which Ptolemy’s proof of it is purportedly based are off by ordmag 1° (Rawlins 1987 p.237). [4] The Alm proof of Mercury’s mean synodic motion was already shown above (§F2) to be pretense, by one who 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 fabricator was the immortal empiricist Aristarchos whose universe was rightly at least trillions of times larger than Ptolemy’s geometric 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 originally 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 Aaboec conversation, 1976/39). (Note: Oxford U. Press was warned by 19977/117 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’ Cyclicties: Rawlins 2002B §H.

N7 When a ball is tossed upward at 0° and caught downward at the same height 4° later, most of us know it maxed at 2°. Yet, from his own astonishing failure (R.Newton 1977 fn 20) to understand this junior-high maximum-height problem, MacArthur-Genius Swerdlow keeps asserting (with Evans’ and Jones’ evident assent: fn 11) that solstices could not be determined accurately, and likewise (fn 8) that outdoor maximum Venus elongations must’ve been so crude that Ptolemy was forced to compute them indoors. His argument (perhaps unique in all history-of-science scholarship): the time of maximum cannot be well determined because, near maximum, the object is hardly moving.

This sort of spectacular embarrassment is what happens when a history-of-astronomy crusade depends on those with inadequate gifts in positional astronomy, spatial relations, and common sense. (Assuming Swerdlow is not knowingly ladling nonsense to vulnerable archons too predisposed and subconsciously predisposed to recognize the frank.) Due to just such JHA-published even-scientific, Ptolemy’s even perversely teach — complete with now-ironically Pompous sneers (quoted, R.Newton 1977 loc cit) at sub-JHA-untouchables — that ancient equinoxes were more accurate (fn 11) than solstices, from their own unfamiliarity (e.g., JHA Editor-to-be Evans at fn 11), with [a] the instrumental and astronomical problems involved (R.Newton 1977 pp.81-82; and Rawlins 2018U §F1, whose eq.10 quantifies for the 1st time the ordmag 1° effect of deviation from quadracity: §N19 below, not to mention [b] ancients’ historically uniform choice of solstices not equinoxes for yearlength-determination. Have those who’ve been disbelieving Greeks’ ability to measure solstices accurately (Swerdlow, Evans, Duke) noticed that the newly available papyrus PFoufou 267A (§N19) has tried to enlighten them by directly surprise-testifying to an ancient solstice which was accurate to ordmag 1°? 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 its error 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&8) 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: fn 24] 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 yearlength — they chose solstices.

N8 It will be convenient to here list in one place prominent instances, of the pile-on passion of the most elite historians-of-science, for trashing ancient empirical science:


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

Irony: despite four reliable solstices by Kallippos, Aristarchos, & Hipparchos (Rawlins 2018U Table 3), ancients never got close to an accurate yearlength, as far as we know. In 1977, Brigham Young Univ astronomer H.Kimball Hansen conceived a simple method ancients could have used for accurately finely findings the year’s length. [1] Find a stable stone point on a hill which near an equinox casts a North-South shadow at apparent noon on a stable stone surface below, both stone locations being more secularly immobile than human equipment. [2] On some date around an equinox (no need to be just at one, merely when solar declination-motion is near-maximal), mark where the noon shadow is. [3] Note when it returns there 20 years later, and divide the interval by 20. The result, in just 20°, 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 unexceptionally ignore the perfectly Occamite for 3 values...
astronomers did their high-precision celestial work (\textit{Alm} 7.3) by recording angles in the old pedagogical tradition of clumsy fractions of right angles, etc., a position recently undercut by the high-school-level discovery of the previously-unnoticed fact that Archimedes' solar diameter was measured and bracketed in degree-fractions (Rawlins 2018U fn 4)

\textbf{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 \textit{op. cit} §C4; DIO 13.1.2 [a]). 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 \((\text{f3 fn 120 below})\), though not necessarily proving, mere derivative degeneration from its conquerors’ superior technology. (See also Rawlins 2018U §34.) An obvious & devastating point, never previously emphasized for its implications: \textit{not a single trig table survives in any form from Seleukid-era Babylon}. Babylon made no solstice or equinox observations (Neugebauer 1975 p.566), 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 attempted figure for it, \(35^\circ\) \text{N}, is entirely unreliable in Greek records, not a word on \(L\) where such unequivocal material, \textit{another crushing blow to Babylonianist pretensions}, and \(B\) is too far north by \(2^\circ28'\) — 148 nmi. Finally, while Greek planetary order was physical — Mer-Ven-Mar-Jup-Sat — Babylon’s was astrological, Beneficent — Maleficient: Jup-Ven-Mer-Sat-Mar.

\textbf{N14} Failure to learn anything from the first of \textit{DIO’s} eclipse-cycle solutions (\$333), in which equating 9660 synodic months with 781 sidereal years (the interval between two \textit{attested} local-midnight lunar eclipse records, Babylon \(719/3/8\) & Alexandria [Heron] 623/13/3–14) neatly recovers all 10 digits of Ptolemy’s previously mysterious final-solar-epoch 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 \textit{DIO}. Similarly at Rawlins 2009E fn 7.) This is also the 1st irrefutable evidence for ancient use of sidereal—tropical transformation of period-relations (Rawlins 1996C eqs.26-27), a process later extensively employed in \textit{DIO 11.2}, which allows (here, in fn 8 [a]) reconstruction of Venus' accurate pre-blunder synodic motion.

\textbf{N15} Among the most egregious of all inversions of ancient astronomical procedure: our uniformly on-the-nose high-cyclesolutions of no less than 4 long-mysterious periodic lunar mysteries (\$N14 and \$N16-\$N17) fall upon locked-up minds, disbelieved without a glance by jeering JHADists who (frustrated by inability to find error in heresy’s math but determined \($\text{f3 B}$\) to reject the obviously probable in favor of the preconception-accordant improbable) resort to whatever dodge will serve. Besides jeerleader Jones’ private carelessly unchecked & glaringly one-sided misfire (\$N16), there’s a long-popular theory (item \(E\)): put into writing privately by P.Huber, likely inspired by Neugebauer [also primarily a mathematician, like Ptolemy] that is so inverse-contrary to sensible scientific practice as to gain special popularity among gaping cult-minds, thirsty for any refutation of undeserving outsiders’ proposals, thus not just suggesting but insisting upon JHADists’ pure speculation that very long period relations should have origins all 6 much shorter relations, & no other hypotheses need apply. The truth was 180°-opposite from this rigid position, of course: ancient’s more-convenient short relations descended from less-handy ultra-long empirical ones [\textit{DIO 11.2}], for reasons about to be explored, below.) Which demonstrates yet again our JHAD’s unerring attraction to the erring. Considerations:

\[A\] Without even being told, all positional astronomers instinctively know that the secret of ensuring high accuracy for a 2 event-based celestial period \(P\) is just to wait for a large enough number \(N\) of returns, ensuring a huge time-interval, \(t_1\) to \(t_2\), so that the error in deduced \(P = (t_2 - t_1)/N\) caused by the errors in \(t_1\) & \(t_2\) is trivialized by the enormity of \(N\). (How else could the ancients determine [\textit{Am 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 \textit{Alm} 9.3 are obviously not directly measured since they are [i] \textit{not integral} (a remainder of a few dozen periods, from the cycle’s imperfection), & (item \[D\]) [ii] tropical, not sidereal. \[D\] Genuine, huge, observed integral period-relations were presumably recorded in Ptolemy’s \textit{Planetary Hypotheses} (Neugebauer 1975 p.906 Table 15), mostly on the order of 1000\(^2\), verifying to all but splice-dreaming JHADists that 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 \textit{op. cit} §C4; DIO 11.2 [a]), Clue: virtually all useful astronomical texts from Babylon post-date its conquest by Greece, and its periodic functions are not trigonometric ones (Greek astronomers had trigonometry from no later than c.130 BC) but approximations thereto, by zigzag or even step functions, suggesting (\$3 fn 120 below), though not necessarily proving, mere derivative degeneration from its conquerors’ superior technology. (See also Rawlins 2018U §34.) An obvious & devastating point, never previously emphasized for its implications: \textit{not a single trig table survives in any form from Seleukid-era Babylon}. Babylon made no solstice or equinox observations (Neugebauer 1975 p.566), 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 attempted figure for it, \(35^\circ\) \text{N}, is entirely unreliable in Greek records, not a word on \(L\) where such unequivocal material, \textit{another crushing blow to Babylonianist pretensions}, and \(B\) is too far north by \(2^\circ28'\) — 148 nmi. Finally, while Greek planetary order was physical — Mer-Ven-Mar-Jup-Sat — Babylon’s was astrological, Beneficent — Maleficient: Jup-Ven-Mer-Sat-Mar.

\textbf{N16} Out of typically excessive and (www.dioi.org/thr.htm#bsvx) uninformined certainty that 13th century BC Babylonian observations couldn’t have occurred, privately scoffing at & non-citing as utterly, a \textit{priori}-ridiculous the only solution (\$3 \textit{III}-\textit{I37}) YET discovered for (any, much less) ALL of the last 3 hitherto-unsolved anciently-adopted lunar motions (\[A\] System A; \[B\] draconic; \[C\] Ptolemy’s last lunisolar equation), namely: periodrelation \textbf{ratios} from eclipse cycles exceeding \textit{1000} (all 3 stable, due to integral [or half-integral] anomalous returns), with common integral factors removed, as at \textit{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 \textit{half-dozen} next evidences and fits (Rawlins 2002H §C3-C9) backing the theory that the draconitic month was determined by Hipparchos, by using the very same back-end – 140/1/27 eclipse he’d used (\textit{Alm} 6.9) when first applying (almost as accurately) the same eclipse-period method. (Debate-averse Jones privately produced a \textit{single-item} retort [which insta-melted upon examination: Rawlins 2002H \[D\], while ignoring all 6 obviously shocking 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: \[a\] is bi-attested (\textit{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 (§53) significance of the Almajest’s three lunar periods’ high accuracy — but to then let shunning dictate refusal to cite the 1st solution (§16) anyone has yet achieved, for how these periods were obtained? That’s non-citation with an impressively unanimous lockstep. . . . (Is there a prize we don’t know about, for this special brand of perfecution?) To summarize, cultists keep secret from JHA readers and the larger public BOTH: [1] the historically revolutionary empirical significance (§M3) of the inescapable fact that all three ancient lunar-speeds are micro-accurate, and [2] the eclipse-cycle source (§N16) of such accuracy. (Let Animal House’s Dean Wormer top THAT Double Secrecy.) And don’t miss the saddest&sadist-masochist part: all disbeliefing archons are religious Babylonianists, but their coherent disdain for target-heretics trumps even worship of Babylon, as their rabbotic shunning requires every single cringing cultist to forgo reveling in the mathematical recovery of the greatest heritage from Babylonian astronomy, not to mention the earliest major science, observation and preservation for a millennium "of precise eclipse-records, now-lost, but invaluable to classical-era scientists, who, without the huge N (§N15) which Babylon’s data repeatedly made possible, could never have found those above-cited astonishingly accurate lunar speeds, that are among the prize glories and proofs of empirical Greek astronomy. Rejectors of DIO’s 3 big-cycle solutions have the advantage that all 3 proposed front-end eclipse-records are long gone. But: [a] The early front-end eclipse for the parallel 9660° = 781° case does survive (§N14 & §3 [S33]). [b] Is it JHAD-banned to use intelligence to induce Greek astronomers’ possession of the §N16 front-end eclipse-records? — just as DIO induced (§N19) Hipparchos’ − 157 solstice & adoption of Kalippic motion, both of which (after same JHADists’ scoffs) turned out (§N19) to be anciently attested. In this field, what is intelligence for, if not to revive lost antiquities? Were the earlier data extant, there’d be no inductive mysteries here. Who deserves a field with no challenges, no advances beyond texts?

N18 Rigid, total, as-usual-unanimous, high-odds-oblivious rejection of 5 new data-match-based proposals central to the field: [1] Computing from his saros-based 4868° Great Year, Aristarchos originated the “Babylonian” month, 29'14''1''00'50'', decades before Babylon (§3 [G4]; or Rawlins 2002A eqs.4-8). [2] DR’s 2001/6/27 British Museum lecture showed (ibid: eqs.9-11; or §3 [G5]) Aristarchos applied to this the Metonic cycle (235 months ≈ 19°), resulting in his Metonic “tropical” year \(Y_A = 365\frac{5}{4} - 15 - 4868\) (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. 191 fol. 170° (data at Neugebauer op cit p.601, τ ε δ’ k’ β’ or 365 4' 20' 60 2'; if expressed as the continued-fraction 365 1/4 [1 + 1/20 + 2/60]) [Rawlins, op cit eqs.12-13], this is \(Y_A = 365\frac{5}{4} - 15 - 4868\), verifying above item [2]. All without altering any Vatican document number and much aided by Neugebauer 1975 p.602’s perceptive interpretation of \(ε = \text{sixtieths} \). [4] The sidereal-year companion data, Aristarchos, op cit τ δ’ ε δ’’ δ’’ or 365 4' 10' 4' 3' from vat. 381 fol. 163° (Neugebauer 1975 p.601), we write as continued-fraction 365': 1/4 [1 - 1/(10 - 1/4)] (§3 [G2]; Rawlins, op cit fn 14-15), yielding sidereal year \(Y_A = 365\frac{5}{4} + 1/2\) (good within a few time-segments), 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 \(Y_A \) & \(Y_M\) IS PRECESSION, suggesting geonomologist Aristarchos’ apt pre-Hipparchan discovery of it. His value is near 1°/century, a much-too-low false rate, later rounded to exactly 1°/century & adopted by Ptolemy. Having adamantly rejected all 5 of the above ideas, from the floor at the 2001 event, Jones 2010B (pp.21-22) won’t cite any of the 5 even while covering the same documents. Unable to find fault with any of the foregoing propositions’ math, Jones (ibid n.27) vandalizes their data-bases by (unlike Neugebauer, loc cit) arbitrarily stripping off all accents, never letting his readers know of the bare existence of any of the 5 proposed advances that the unsubtracted data encourage.

N19 Return results from newly-translated paper: P.Fouda 267A. [1] The papyrus’ vindication of the previously unknown Rawlins 1991W (§K8-K9&M4) discoveries that Hipparchos sought a − 157 S.Solstice and that he (twice: idem) used Kalippos’ 365'1/4-year solar motion. Both his − 157 solstice (a different day: Rawlins Kallippos’ 365'1/4-year solar 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 §K4-K5 & eq.25) of P.Fouda 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, 365'1/4 − 1/309? The field was informed of these discoveries in 2015. Inert reaction: §3 fn 66.

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

N21 Carrying §N20’s handbooks-as-primary-science transformation to an unsurpassable apogee, as the field’s archipsanship improvidently50 for decades banished anyone who object to selling the clumsiest (§C & G) faker in astronomical history to academe & the public as “THE GREATEST ASTRONOMER OF ANTIQUITY.”

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50 Note advice at Rawlins 2000A 2.2 & 9.21 according [A] caution before plunging fervently into establishment-cultism’s bleak-hole of inescapable devotion (to temporarily dominant cults’ theories) — an unforgiving singularity which lies in wait to swallow the unwary recruit; and [B] treating contrary evidences not as downers but instead welcoming them as possible helpful warnings of more of the same. 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-leading article (Evans 1987 — sixty-four pages of JHA-up-front anti-Newton, anti-Rawlin, anti-Occam try-anything-apologia), using such outré resorts as already highlighted above (§L2 and fn 11), underterrifyingly explaining-away, to his (and mythical JHA referees’) satisfaction, one-after-another oncoming evidences (fn 12) of Ptolemy’s obvious theft of JHA’s notorious Farnese disaster, either: (a) its 4 big-cycle solutions have the advantage that all 3 proposed front-end eclipse-records are long gone. But: [a] The early front-end eclipse for the parallel 9660° = 781° case does survive (§N14 & §3 [S33]). [b] Is it JHAD-banned to use intelligence to induce Greek astronomers’ possession of the §N16 front-end eclipse-records? — just as DIO induced (§N19) Hipparchos’ − 157 solstice & adoption of Kalippic motion, both of which (after same JHADists’ scoffs) turned out (§N19) to be anciently attested. In this field, what is intelligence for, if not to revive lost antiquities? Were the earlier data extant, there’d be no inductive mysteries here. Who deserves a field with no challenges, no advances beyond texts?
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31 Compiled c.160 (in 20; contra Toomer 1984 p.1). The common, more respectful-sounding title, Almajest, is descended from the Arabic almajasti, Toomer 1984 p.2. So Almajest seems less corrupt.

References

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\[\text{""""The Acquittal of Ptolemy." Written by Sverdlov-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 [*fn7 and fn 8] trio cited in the piece he published [Sverdlov, Gingerich, & V.Thoren], reflecting the kind of muttered slander (more at fn 35) created and spread behind backs by parties many of whom today keep believing that if they can just preserve or salvage some sliver of doubt that Ptolemy faked, they are thereby not utterly convicted of the vilest brand of academic misbehavior, in their decades of gang-smearing those who were — the ultimate irony — on the side of truth right along. Which merely adds ethical incomprehension to scientific. And why would a party have ever in the 1st place resorted to slander & incomprehension to scientific. And why would archons tolerate peace?"

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