

‡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 Ptolemaist O.Gingerich's various past slanderous referee reports upon our work, naturally requesting removal of anything embarrassing to his clique, *even offering to take another later look at the paper* (fn 35: "If . . . DR revises . . . I would be happy to look it over.") to confirm that the censorship he was ordering had been satisfactorily carried out. So our 2015/9/30 resubmission added extensive notes, responding to such typical intrusion by reviewing — at least for *JAHH's* info — the long, revolting history of such stifling of open discourse, **but giving *JAHH* permission to delete these or anything else it thought inappropriate**, with our encouragement at the prospect of such helpful assistance. In reaction, *JAHH* has followed the *Journal for the History of Astronomy* in permanently severing communication with *DIO*: suggesting, as later confirmed, www.dioi.org/oww31.pdf, that *JAHH's* initial request for cuts was made in vain hopes (encouraged by years of Gingerich-circle slander of DR) that *DIO* would adamantly refuse revision or cuts, thus killing the paper without *JAHH* being indictable for censorship. *DIO* instead agreeably refused to fall into that trap; thus, fleeing was *JAHH's* only escape-option to effect pre-ordained rejection. The 2015/9/30 version follows, very slightly enhanced.]

ABSTRACT

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

A INCURABLE DENIAL OF THE UNDENIABLE

Claudius Ptolemy's *Almajest* is the central document (§J1 below) of our valued heritage from classical antiquity's mathematical astronomy. Though Princetintute's Neugebauer 1957 (p.191) has called it "one of the greatest masterpieces of scientific analysis ever written" the present paper will prove that in truth Ptolemy was not a scientist, but a mathematician who faked science. In an astrology-saturated era, he worked for the occultist Serapic state religion at Canopus (near Alexandria) where stood its major temple, which specialized in curing illness through astrology&dreams. Ptolemy also authored astrology's bible, the *Tetra-biblos*. His employers no doubt appreciated his consistent demonstrations that reality was in *perfect accord* (§M2) with divine celestial theories. However, for centuries, historically savvy astronomers have known that this famous 2nd century AD astrologer-geocentrist-mathematician accomplished said illusion by indoor-faking or plagiarizing all his allegedly-outdoor "observations" of celestial phenomena, to force precise accord with indoor mathematical models (some of which had already existed for centuries) and tables computed therefrom. Ptolemy's depredations even included stealing and mis-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^y in advance (*ibid* Table 23: 100 select stars for 1701.03). But a few invincibly innocent & deeply committed (§B2; fn 11) archonal historians-of-astronomy keep intermittently trying to breathe life back into their longstanding tradition — e.g., Neugebauer 1975 p.284 & Pedersen

1974 p.258 — of imagining¹ (& see Neugebauer 1975 pp.69, 119, 205; J.Evans, 1987-to-

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

date; A.Jones 2010A² p.xiii that Ptolemy was actually an honest outdoor astronomer (even [below, at §N3] rating Ptolemy a better *observer* than Hipparchos!); or at least (Schaefer 2013 p.47) that there is still a serious question about whether he observed outdoors: classic the-controversy-continues resort (latest example: fn 1), ever dear to those fearing faces-loss in a prominent controversy. Even after a 50^y avalanche of discoveries consistently (fn 12) contradicting his position, Earth's most loyal Ptolemist responded to Thurston 2002S p.69's prominent challenge by *unqualifiedly* verbatim-reaffirming (Gingerich 2002 p.70) his original 40^y-ago verbatim echo (Gingerich 1976) of the once-unopposably-dominant mantra (§M2) that Ptolemy was "the greatest astronomer of antiquity" (Neugebauer 1975 p.931).

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

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

some don't empathize with the laughter a genuine scientist can hardly stifle, upon carefully examining the goal-directed imitation-science that is (not merely *charged* here but is) repeatedly *proven in detail*, throughout the paper, to be what has been consistently and exclusively used for decades to do what a once-ruling cult wanted done: make a case — any case (§H2 & ‡3 fn 66) — that Ptolemy was an honest, outdoor-observing astronomer. The referee imagines that the bad-old-days still apply, when Ptolemists could keep doubters from being taken seriously just by portraying them as those who had committed the crimes [a] of being few, and [b] of merely *disagreeing* with formerly-mid-spectrum orthodoxy. He actually dream-contents (fn 5) that the view that Ptolemy faked or plagiarized is still today a way-out *end-of-the-spectrum* theory, a charge which uninformedly classifies Ptolemy-doubters Dennis Duke (fn 23), Sam Goldstein, Gerd Graßhoff, Kimball Hansen, Willy Hartner, Alex Jones (fn 2), Charles Kowal, Robert Newton, Myles Standish, Richard Stephenson, Hugh Thurston, Gerald Toomer (fn 17), Bart van der Waerden, Curtis Wilson, and Don Yeomans (2005/4/28) as fringe! (Also Kristian Moesgaard, who was the 1st centrist to realize the significance of R.Newton's fractional-endings test: §12. Moesgaard 1980C says of the R.Newton fractional-endings analysis: "This renders it probable that the [*Almajest* star catalog] longitudes" were plagiarized from Hipparchos. But the original version of the paper [sent to DR 1978/4/30], as submitted to the R.Newton-hating *JHA*, read instead [emph added] "*Beyond the shade of a doubt* this renders probable that the [*Almajest* star catalog] longitudes" were plagiarized from Hipparchos. The incident is nearly an exact repeat of frightened Astronomer Royal Geo.Airy's equally revealing bowdlerization of "shadow" from his 1846/7/9 letter to Neptune-affair co-conspirator J.Challis: see Rawlins 1992W §B2 & Rawlins 1999N §H2.) And for more unintended quasi-humor, we have the referee's judgement — as a seasoned (fn 5!) authority on *non-insult etiquette* and *fair play* towards "those with different views" — that the paper shows insufficient doses of the *respect he believes is owed* to those wannabee-numerate historians-of-science who have repeatedly (§B2) libelled physicists R.Newton and Rawlins as dishonest crazy incompetents and who have (fn 5) for years ducked debating Rawlins (compare to fn 6, below), though having the courage to serially portray his work in the most negative light to the extent of over 100 pages (1987-2008) in the *Journal for the History of Astronomy*, from which he has been banned (§B1) for the last 1/3 of a century, during which time virtually none of Newton's or *DIO*'s dozens of positive contributions to knowledge (e.g., R.Newton 1977 & R.Newton 1982, www.dioi.org/vin.htm) have been credited there, a blank entirely in accord with shun-necessities&practices all too obvious from fn 34 & ‡3 fn 109.

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

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

refereeing (fn 3) but is alert as can be in not permitting any author to argue in its pages that Ptolemy was a liar, even while allowing the defense clique to contend he was not (Evans 1993 p.145; Rawlins 1999 §§F7-F8). That the controversy has been rife with "unprofessional" acts, such as shunning, has been nationally published (Schaefer 2002 *loc cit*), followed soon after by a direct appeal (fn 35) to the head of the American Astronomical Society, urging supervision of its shamelessly shunning Historical Astronomy Division (H.A.D.), but the unprofessionalism of 2002 has only worsened since. History-of-ancient-astronomy's lengthy communal monolithic and craniolithic insistence upon defensively maintaining — by character-assassination (fn 5) & the threat of exile (*idem* and fn 1) for dissenters — an evidence-defying, perception-inverting (§N), logic-flouting (§M3), holey-corpse honest-Ptolemy-myth as its ultimate herd-sacred tenet, can only weaken the field's cred. Same for launching fantastic, irrelevant, and transparently projective descriptions of bemused skeptics as "angry" (Pedersen 1974 p.23) and "bitter" (Evans 1998 p.268). For which side is *provably* angry, see *DIO* 11.1 p.2; also the case of an eminent referee and Jesus-devotee (fn 5) who routinely (fn 1) calls today's numerous (*idem*) Ptolemy-skeptics a tiny bunch of paranoids⁴ for believing what he himself privately knows⁵ to be true. Healthy restoration

JHA Editor M.Hoskin, as quoted at Rawlins 1991W §B1): he actually had the fantastic gall, in a 1980/7/30 letter to Hoskin, to suggest that the *JHA* was refereeing by "the swiftly-gauged, as against the substantial." Considering the solid scholarship the *JHA* keeps publishing, like its unmatched discovery of the Winter Equinox, among so many other pearls (www.dioi.org/jha.htm#kqlz), this advice was indisputably a "damned lie." (So claimed patient, sedate, judicious [Schaefer 2002 p.40] Hoskin in his 1983/3/3 letter to hothead-horribilis [*idem*] Rawlins, announcing his exile from *JHA*, while suit-threateningly rejecting Rawlins' gentle mathematical criticism of *JHA* refereeing for a 1982 October *JHA* paper — Hoskin's letter mailed, ironically, just before *JHA* received the ethical author's agreement that, after all, the paper in question was just as invalid as Rawlins had told Hoskin: see fundamental correction in the 1984 June *JHA*.) Who better than Hoskin to impart the secret of reliable refereeing? — as in Hoskin's 2003/6/21 letter to H.Thurston, www.dioi.org/pm3.htm, enlightening us on *JHA* email-era refereeing efficiency: "it is quite common for an article received at breakfast to be refereed during the morning . . . and the verdict sent to the author by lunchtime."

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

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

will merely (!) require the opening of discourse⁶ and minds.

B2 Ptolemy regularly fabricated data from which he perversely claimed he derived his models' elements, even while practicing the very opposite. Loyalist Pedersen 1974 p.23 originally disagreed but summed up the esteemed astronomer J.Delambre's view of Ptolemy as "a scientific cheat, swindling with the very method of science and betraying the empirical character of astronomy, setting forth results computed from theory disguised as empirical data in support of this same theory." *Ibid* p.258 also originally rejected the charge that Ptolemy stole from Hipparchos the thousand star-positions in the Ancient Star Catalog — even while suppressing⁷ 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^y led a crusade to convince the world [i] that an ancient who was off his latitude by 1°/4 and could not find the Sun within a degree, was a regularly observing astronomer, and [ii] that anyone who disagreed was the insane party (§B1). Equally risible: to be shrinkoanalysed as paranoid by one who thinks the whole universe is run by an invisible mind and who publicly calls on "our Lord Jesus Christ." A world where sanity is insanity and vice-versa. With respect to its obvious duty to return the Ptolemy controversy to a *scientific basis* — above politics, smearing, shunning, religious devotion — the American Astronomical Society for a half-century hasn't lifted a finger. But (fn 35) has generously given it to the skeptics. Soon after Gingerich had brought *P.A.S.P.* up to snuff on who is and isn't reliable, *Skeptical Inquirer* Editor K.Frazier asked him to debate Rawlins on Ptolemy in *SkInq*; Gingerich (1978/2/2) explained his refusal by calling Rawlins "exceedingly paranoiac" for "suggesting that a cabal has been suppressing the consideration of [R.]Newton's work" on Ptolemy. When finding that Frazier had sent a copy of this helpful character-profile to Rawlins, Gingerich got miffed at Frazier! (1978/6/2 letter) — so exalted is his permanent mental state, assured of ontological immunity from the ethical standards of ordinary mortals, sinless by very definition. Like his image of his inherently unindictable ancient astronomer-hero. E.g., only such an elevated being could — without any sense of hypocrisy, of damage to knowledge, or of harm to the slanderer — accuse someone of insanity for believing in a conspiracy to suppress heresy even WHILE he was so conspiring (Gingerich to *P.A.S.P.* at the head of this note). At Gingerich's insistence, U. Notre Dame's 1999/7/3 debate on whether Ptolemy observed or stole the Ancient Star Catalog could not be held unless Rawlins was kept from the platform. Evidently inspired, by decades of toadily awesome H.A.D. worship, to standards of equity and consistency incomprehensible to the less godly (and strengthened by the surety with which he can count on fellow pols' reverent silence on his secret actions, some even going so far as to supplicate repeatedly for personal permission, before publishing anything unblest), Gingerich in a 2000 referee report to *Isis* on an article (Thurston 2002S) appreciative of Rawlins' inductive successes, typically promoted freespeech-in-theory while inserting an element aimed at ensuring that heretics' Irresponsible abuse of freedom would be properly recognized as what could only issue from a disordered mind: "To say that the paper should not be published will only fuel the paranoiac claims of the small [fn 1!] group fighting Ptolemy, who believe that a cabal of [Neugebauer] 'mufia' [sic] are preventing their viewpoint from being aired in the standard journals." Hmmm. On 1976/9/15, Rawlins received another paranoiac's letter regarding said zany's own recent book-review (emphasis added): "So far the Neugebauer camp has not been heard from. *Perhaps my merely mentioning [R.]Newton in a review of Neugebauer has placed me beyond speaking terms.*" (See www.dioi.org/pm1.htm.) And who was this fellow-paranoiac, who imagined a closed Neugebauer "cabal"? No other than O.Gingerich! In the midst of decades of documented — ah — "inconsistent" character-assassination fantasy, Gingerich adds one more fantasy, soberly describing himself as "a practicing Christian": 1978/2/2 to Frazier. Obviously, being religious doesn't mend vendettas. We recall gullible or cynical saint-mythologizer Cardinal John Henry Newman's politically deft holy war on straightforwardly ethical independent idealist Chas. Kingsley; at Newman's 1890 death, after the standard public eulogy, Cardinal Henry Edward Manning privately mourned his colleague thusly: "Poor Newman. Poor Newman. He was a great hater." (Lytton Strachey, *Eminent Victorians*, 1918, Manning chapter, end of part 9.)

⁶ *DIO* has long had a standing offer to publish debates, with quite novel rules, www.dioi.org/deb.htm, scrupulously designed to ensure fairness. [Except for inspiring a contemptuous joke from A.Jones, these rules — and *DIO*'s generosity in offering debate-space — have proven of no interest to Muffiosi.] Such openness to airing the findings of what the referee calls "those with different views" (fn 1) contrasts revealingly with the degree of dissent-toleration exhibited by the referee's own circle.

⁷ Rawlins 1982C p.362 proves that Pedersen positively knew of Delambre's crucial discovery (§K1), but chose not to impart it to his readers. [See ¶3 fn 121.]

in fact had stolen it. Pedersen 1974 p.258 added (emph added): "Our general impression of [Ptolemy's] moral and intellectual integrity *would be damaged beyond repair* if we had to believe that he simply derived his catalogue from a previous work by Hipparchus without the slightest acknowledgement of the fact." Yet when it later became obvious that the theft had indeed occurred, Pedersen flexibly decided that stealing stars didn't really prove Ptolemy was dishonest, after all (Pedersen 1993 p.559). Agreeably reverting to the 1974 Pedersen, Evans 1998 p.262 says: "At stake is Ptolemy's reputation as an astronomer; at issue are his honesty and reliability as an observer." At stake? Well, not-reeeally — for either 1974 Pedersen or *JHA* Editor Evans. Each could always be counted upon to explain-away (fn 11) any negative evidence, trying to make the issue of Ptolemy's integrity untestable thus irresolvable, so never having to admit their original mistake in defending it. And far, far harder: to face the shame of having for decades (mostly behind-the-back: fn 5) gang-condemned as fools and cranks those who turned out to be more prescient (§M3 and fn 12) than their denigrators. Pioneer skeptic Robert Russell Newton is deceased. Nothing can now compensate him for the hateful, largely-whispered bile heaped upon him to prevent his case from getting a fair hearing while he lived: sampling at fn 35, plus MacArthur Genius and amateur⁸ scientist Noel Swerdlow's branding this brilliant physicist a Velikovskian "con-man" (see *DIO* 1.1 ¶3 §§D2-D3; and for who's really Velikovskian, see below at §N19, and *Worlds in Collision* p.330). Given such a heavy longterm investment in their position, it's an easy prediction that not even the nine ultra-obvious evidential items set out below will cause Swerdlow or Evans — or anyone else in their shrunken⁹ chauvinist

⁸ Demonstrating how one earns a MacArthur in certain sub-fields, the principled Dr.Swerdlow has not only called R.Newton dishonest and kook (§B2), he has additionally published a Joe-McCarthyesque incompetency-accusation against him (quoted verbatim at R.Newton 1991 §E22), claiming lots of thesis-gutting mathematical errors by Newton here-in-his-hand *without giving a single valid example*. (Check the revealing cavil-count context data carefully detailed at *ibid* fn 6.) And the *Journal for the History of Astronomy* — applying its proud smear-review policy — naturally approved this much-rewarded (fn 28) libel's publication *in that state*, no questions-asked. (Swerdlow's ugly response to R.Newton aimed less at proving his thesis wrong than at portraying him as astronomically incompetent. Given the two sides' relative technical talent, such Ptolemaic antics reliably kept on giving Newton and Rawlins their top entertainment of the controversy.) Perhaps the least-believable incident was when *JHA* Book Review Editor Gingerich chose as reviewer, for Newton's 1976 book, miss-man Swerdlow — whose predictably maw-foaming throat-lunge (samples included at fn 35, direct quote at R.Newton 1991 §D13) inadvertently revealed that Swerdlow didn't understand even so much as the TITLE of the book he thought he knew enough to denigrate: see it happen at R.Newton 1991 §A2. We respond with the sort of list Swerdlow should have laid out against Newton but could not (admire in-passing Swerdlow's personal reinvention of the mathematics underlying the most elementary issues, including both of his attempts to answer central heresy on Ptolemy): [1] Ineducability on the high-school maximum problem cited at §N7, being unfamiliar with the long-standard (see any Bowditch *Navigator*) Equal-Altitudes technique, for years repeatedly (fn 11 below) insisting that a quadratic's maximum can't be found accurately, *because near maximum, the function isn't varying much* — e.g., Swerdlow 1989 p.36, when trying to alibi Ptolemy's Venus fakes. [2] Obliviousness to the fact that ALL observing ancients used solstices not equinoxes to gauge yearlength (their awareness is itemized at fn 11 [2]). [3] Innocence of need (similarly at Rawlins & Pickering 2001) to convert longitude residuals to great-circle by factor $\cos \beta$ (during his attempt to answer the absent-error-waves test, Swerdlow 1992 p.176; misunderstanding clarified for him by Rawlins 1992V fn 31). [4] Following (Swerdlow 1989) Ptolemy's elaborate fraudulent mathematics to complicate the finding of Venus' orbit from greatest elongations — though simply bisecting the angles between their sight-lines sets-up solution of the orbit (Rawlins 2002V; Thurston 2002V) with 10th-grade ease. (The Venus synodic mean motion deduced from the *Alm*'s proofs is the worst of all the planets. But, if adjusted according to the theory that its reporter had confused tropical and sidereal cycles, it strikingly becomes among the best, as accurate as Mars', good to ordmag 1'/century — see Rawlins 2002V §C3.) [5] Obvious mis-math: here at §D3. [6] And some Swerdlowian fudgery (R.Newton 1991 fn 7) that Ptolemy would've been proud of.

⁹ Ptolemaists' bunker is here described as not shrinking but shrunken, since for years now it's been composed of only the hardest cases, by now auto-rejecting all evidence showing they were extensively, viciously wrong. (Like those who'll be annually fantasizing forever in Ferguson.) And all will die

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

B3 Notable features of modern Ptolemisses are: [a] Consistent preference (fn 18&50; §N15) for the inherently unlikely¹¹ over the inherently likely. [b] Failure to notice that serial-proposal of a disjointed collection of *odd-hoc* theories, each tailored specifically and entirely for dodging the latest¹² individual Ptolemy-indicting bullet (see also §D6 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.dioi.org/vols/wb2.pdf, *DIO 11.2* (2003) [and p.30, p.33 items 5&6]. We await the century when Swerdlow or Gingerich can display in print the same humility as the “pompous” (fn 1) skeptical side.

¹¹ For examples of downright embarrassingly improbable apologia to defend archons’ pro-Ptolemy commitments, see here at, e.g., §§K&L2, fn 37. (As well as Rawlins 1985G n.12; Rawlins 1991W §E3 & fn 99. Cumulative osculation-oscillation historical review at Rawlins 1992V §C31.) The ultimate far-fetchedness in service to orthodoxy was devised by Evans and promoted by Swerdlow 1992 p.177, attempting evasion of the fractional-endings argument (R.Newton 1977 pp.245f). The incredible result (Evans 1987 p.243) is spoofed at Rawlins 1992V fn 46 (emphasis in original): “Let’s see, we start by setting [the armillary astrolabe’s] ring 5 NOT on the chosen fundamental star’s ACTUAL [Catalog] longitude at ring 3 but rather at the nearest whole-degree value LESS than [this longitude, for which Evans prefers a 40’ ending]; then, after sighting the stellar quarry with ring 2, we read where ring 2 meets ring 3 AND THEN [“mentally”] ADD BACK, ONTO THIS READING, THE AMOUNT [40’] WE JUST AS NEEDLESSLY SUBTRACTED OFF IN THE FIRST PLACE. . . . Got it?” And don’t miss that this bizarre Evans scheme not only causes the unnecessary trouble highlighted here (and expands the amplitude of the absent error waves [§H1] from 1°/2 to 3°/4!), but would (Evans, *loc cit*) further commit Ptolemy to the extra bother of having to fudge over a hundred (Rawlins 1994L Table 1) stars’ resulting 25’ and 55’ endings (into 20’s and 00’s, respectively) in the manner shown at §14. As one encounters no less than 64 pages of such desperate resorts, we recall (§§B2 and H2; Rawlins 1992V §§C31-C32) it is intermittently contended that Ptolemy’s plagiarizing the Catalog doesn’t prove dishonesty. But, if so, then: why did the *JHA* waste over 100 pages fighting academe’s acceptance of a Catalog-theft that (whenever JHADists momentarily admit it) doesn’t-really-matter? (Iraq war apologists dodge similarly: *DIO 18* §F.) For further imaginative excursions, see (Gingerich 1976 in *Science*) “On Ptolemy as the Greatest Astronomer of Antiquity” and (§M2) *Scientific American*, 1979, “The Acquittal of Ptolemy.” (In the 4 decades since these two worshipfully and optimistically titled Ptolemy-advertisements, neither *Science* nor *Scientific American* has printed a word on Ptolemy’s frailties; likewise, *Sky and Telescope*, which has instead repeatedly defended him: 1976 Feb-to-2002 Feb. The vaunted Free Press at work.) Extra community-embarrassments: massive double-Pb-paper Evans 1987 (below, fn 47); top Newton-exiler (below, fn 35) Swerdlow 1989, on whose invincible math-innocence (repeated 1979, 1981, 1989, 2010 while reaching for Ptolemy-exculpations), see above, at fn 8 or Rawlins 2018U §§B2-B3. Two more cases of shamefully shoddy science seem descended from solstice-denier Swerdlow’s delusional argument (§N7 below): [1] The instrumental (*and historical*) misconception of Jones 2002E p.16, that ancients found geographical latitude *L* from observations at equinox. (Contra chapter-one Ptolemy, *Alm* 1.12; see Rawlins 2009S §§F2-F3 & eq.8.) [2] Evans 1998 p.206 explicitly echoes Swerdlow’s highschool incomprehension, claiming a transit instrument “could not determine the time of solstice very precisely. owing to the [gradual] nature of the solstice itself. A precise measurement of the length of the year could not be based on the solstices. More reliable for this purpose were the times of the equinoxes.” Aside from his clique’s instrumental blind-spot (§N7): is Evans *even aware* (as is, e.g., Toomer) that all outdoor astronomers’ extant ancient yearlengths were solstice-based? — Meton (*Alm* 3.1), Euktemon (*idem*), Kallippos (Toomer 1984 pp.12&214, fn 72; Rawlins 1991W §K4), Aristarchos (§N18 below), Hipparchos (§N19 below). (Recoverable Greek solstices’ rms error 2^h [§N7] vs Greek equinoxes’ rms error 7^h [§M3].) The risibility recreates *Funny . . . Forum*’s general Miles Gloriosus as philosopher: *Stand Aside. I Take Laaarge Logical Steps*.

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

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

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

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

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

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

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

C2 This is by far the funniest¹⁴ & most astonishingly inept fake in the entire history of the oldest science. (But watch Ptolemy again&again try mightily to match it at §§E5&G3.)

D ILLEGALLY BLIND: SKY or SLY? MULTICORROBORATION

D1 The Greatest Astronomer of Antiquity’s four solar “observations” (*Alm* 3.1 and 7) are on average about 50 times nearer¹⁵ to his indoor tables than to the real outdoor sky

create Reasonable Doubt, Rawlins 1992V §C20 responded: “Besides a range of specific evidences of plagiarism, there is the simplicity of that hypothesis’ fit to the larger evidential situation: if we merely assume that Ptolemy swiped the Catalog, virtually all of [defenders’] central purported ‘Enigmas’ (*ibid* [§C22]) of the case immediately evaporate.”

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

¹⁴ However, O.Gingerich (2000 Summer referee report on Thurston 2002S) regards the Venus double-dating as a testament to Ptolemy’s “great ingenuity in tackling an otherwise essentially insoluble problem;” admittedly fabrication but innocent since (Gingerich 2002 p.72) there was no other way to solve for the orbit: Venus-made-me-do-it. At D.Duke’s instigation, three non-fraudulent solutions (by himself, D.Rawlins, and H.Thurston) to “insoluble” Venus swiftly appeared in *DIO 11.3* (Duke 2002B, Rawlins 2002V, Thurston 2002V) with the comment (Rawlins, *op cit* §17) that Swerdlow’s and Gingerich’s straightfaced “alibiing of Ptolemy’s Venus fumblefarces is akin to a defense-lawyer going into court to get-off a counterfeiter who was so stupidly careless that he accidentally printed Ben Franklin on both sides of his attempts at faking hundred-dollar bills. [But would *even a lawyer*] . . . try to excuse such inept criminality by claiming that the bungled bucks showed *immortal, greatest-technician-of-the-era BRILLIANCE?*” Swerdlow 1989 p.36 fallaciously explains-away (to his own, the MacArthur Foundation’s, and the referee-challenged [fn 3] *JHA*’s satisfaction) Ptolemy’s huge Venus inaccuracies by stolidly repeating his innocence (§N7) of how to find a quadratic problem’s maximum, this time for Venus’ elongations. (Awful details at Rawlins 2002V fn 20.)

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

(errors displayed at Thurston 1998A Table 1): the mean of the error-sinusoid that best fits the tables, and thus very closely fits the “observations,” exceeds a full degree: $-65'$. (See formula at *ibid* ⊙ 1.) This, though naked-eye solar measures can be made to ordmag 1' (fn 47; Rawlins 2018U §B4). Either Ptolemy's genius was so preternaturally refined that he could look up into the sky and (fn 18) see objects' theoretical positions instead of their actual positions¹⁶ or (§B2) he has broken the law of empirical science by pretending to base theory on outdoor celestial “observations” actually computed, indoors on the sly, from or to fit the very same theory (Thurston 1994P §D & Rawlins 2002V §§B3-B5), such fakes then used to “prove” said theory, a (literally) preposterous sham-process as Delambre saw: §B2. **D2** To appreciate the grossness of the illegality here, consider its sheer enormity (as emphasized in solar, lunar, and planetary contexts throughout R.Newton 1977): for his 3 equinoxes, Ptolemy is alleging 1st-hand visual sightings of the Sun's *center* on the celestial Equator at times agreeing to ordmag 1' with indoor theory — when in truth **NO PART** of the *real solar disk* was on the outdoor-sky Equator at any of these three times. For his two Autumn “equinox” times, the real Equator was 34' north of the solar center, i.e., over twice as far from it as was the Sun's own limb! (The angular radius of the Sun is 16'.) Understand that, while these errors' ridiculous grossness alone proves fraud, we additionally know EXACTLY (§D5 below) the method of all four fabrications, though Ptolemy presented each as an outdoor observation of the real sky, not a mere indoor-calculation.

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

D3 Further, the tabular Sun's $-65'$ mean longitudinal error (§D1) at the epoch of Ptolemy's tables, 137.547 (Antoninus Pius Year 1 Thoth 1 [1377/20] Alexandria Apparent Noon), isn't a constant in time: it varies by $-23'.0/cy$. (A rate consistently bi-miscomputed at CalTech's Swerdlow 2010 p.152, item 3.) So: when is Ptolemy's Sun correct? Obviously that epoch must be $137.547 + (100/cy)65'/(-23'/cy) = -145$, which (as seen at Rawlins 1991W §M6; similarly below at §D5) is Hipparchos' era, and is indeed the **regnal** epoch (Ptolemy VII Physkon Year 1) of — and the time of creation (Rawlins 2018U §O) and launch of — his Prime solar orbit (dubbed “PH” at *idem*). How can Ptolemy's defenders expect to credibly deny that he plagiarized from Hipparchos, when Ptolemy's Sun and thus his entire longitudinally contingent celestial system (not just the Sun but the Moon, planets, and stars) is correct only for the time of Hipparchos?

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

D5 This unsubtle echo connects to the irony that Ptolemy didn't fake the solar data via tables but by even cruder means. (He fabricated similarly elsewhere as well: Venus [§C1; Rawlins 1991W fn 166], the stars [fn 37], and the Arbelia eclipse [§G3].) As early as 1639 — **the year the Ptolemy controversy should have ended** (or been doomed to never begin?) — Christian Severin said Ptolemy had merely computed the alleged solar observations from Hipparchan data, and Delambre 1819 (pp.lxvii-lxix) explained in detail how Ptolemy had fabricated his solstice and equinoxes by merely adding integral numbers of Hipparchan years to Hipparchos' observations of solstice and equinoxes. (Discussion: Thurston 1994P p.58; and Thurston 1998A §§A&S.) For example (by method of *ibid* ⊙2, reconstructing indoor calculation of the 139 AD Autumn Equinox): to recover Ptolemy's 132 AD Autumn Equinox (*Alm* 3.7), just add 278 Hipparchan years ($365^d/4 - 1/300$ each, 6th 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.

¹⁶ Ptolemy didn't have to look skyward to see theoretical data instead of real: his *Optics* fakes perfectly false-theory-accordant refraction angles that are erroneous by up to $2^\circ/12$ or 150 arcmin! (See, e.g., Neugebauer 1975 pp.895-896.)

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

D6 We have now encountered the following telltale Ptolemy solar curiosities: [1] mean positional error exceeding a degree (§D1) for epoch 137 AD; [2] and null only (§D3) for Hipparchos' epoch; [3] Ptolemy “observations” tightly (§D4) and overprecisely (§D5) theoretical not empirical. No matter how obvious the implications of items [1]-[3], each has inspired nevereverever-say-die¹⁷ efforts at explaining them away. However, as in §B3 [b], we find no substantial connexion between chauvinists' several desperate and disparate¹⁸ alibis (other than the common aim of rescuing Ptolemy), while by contrast all three oddities are mutually-corroborative of each other through the single simple theory that simultaneously, coherently, and fruitfully explains them: Ptolemy faked.

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

E1 At *Alm* 5.12 and 13, The Greatest Astronomer of Antiquity provides *and computes celestial positions* using his assumed geographical latitude L for Alexandria: $30^\circ58'$ — an erroneous value swiped from Vitruvius 9.7.1 (probably based on observation by asymmetric gnomon, not transit circle). For c.8000 sites, Ptolemy's *Geographical Directory (GD)* lists, in Books 2-7, geographical latitudes L and geographical longitudes E east of the Blessed Isles (discovered at Rawlins 2008S §F, to be obviously the Cape Verde Islands), uniformly rounded to the nearest twelfth of a degree. At *GD* 4.5.9 he gives $31^\circ05'$ for his religious home, the Serapic temple at Canopus. The Alexandria and Canopus values are each too low by $14'$. *No regular celestial observer* — Ptolemy's pretense (at, e.g., *Alm* 7.4) — *can be this far off and not know it*.¹⁹

¹⁷ Go to *New York Times Science's* 2009/9/8 exam of a century of establishment promotion of another scientifically unverified myth, a study in cemental immunity to oncoming evidence, analysed in the context of other *DIO*-shunning bad-loser cults. Previous day's online edition:

<http://tierneylab.blogs.nytimes.com/2009/09/07/who-was-first-at-the-north-pole>. (Ptolemy's all-time record-success at hoax-longevity is noted only in the 9/8 version.) The honest exception to the pattern described is Gerald Toomer who, though previously much-committed to belief that Ptolemy didn't take the Catalog from Hipparchos, immediately changed his mind on seeing Graßhoff's analyses.

¹⁸ From chats with Ptolemyists over many years, the following alibis are recalled: [1] Ptolemy's $-65'$ solar error has been speculated as due to his having constructed his system before most or all of his solar “observations” (pretty dumb, since the system depended on the Sun: §D3), so he was stuck with the error and decided [see Ragep at †3 fn 9] not to re-do his whole scheme. (How is this a defense against a charge of knowing pretense? And plagiarism, since [§D3] the system's error is just that of Hipparchos' solar tables, 2 2/3 centuries later.) [2] The adherence of “observations” to theory (Sun, Venus, etc) is explained as due not to fraud but to Mere fudging or “adjusting” of real presumed data, though whatever positions the “observations” were being fudged to agree with were **SECRET** indoor calculations, so either way it's fraud. [3] The embarrassment that Ptolemy's tabular mean Sun was correct only for Hipparchos' time has been explained by presuming that Ptolemy thought Hipparchos' observations were better than his own putative outdoor observations, so (§D1) he instead reported Hipparchos-accordant data. (Was it honest to commit this appropriation? — and without saying so, which makes it a theft.) [4] See also rocks and asymmetrically unclear air at §L1 and fn 42, respectively. Would that the energy and ingenuity expended upon these 4 joke-fantasies had instead been applied to open-minded, undirected, exploratory research.

¹⁹ Rawlins noticed this unsubtle point (that no regular celestial observer could be $1^\circ/4$ off in his adopted geographical latitude L and not realize so) *immediately* upon entering the controversy. Thurston 1994P noticed something just as glaring back in the 1940s. How could Ptolemy-specialists,

E2 Moreover, such an error (see math of *Alm* 5.12 and 13, or Rawlins 1994L eq.1) would carry exactly into the “Clean Dozen” unfudged and unused Ptolemy-contemporary stellar declinations δ reported at *Alm* 7.3 (the only honest *Almajest* star data from Ptolemy’s era). These, however, show (Rawlins 1994L §F9) that the observer’s error²⁰ in assumed geographical latitude L was $+4'\pm 2'$, about 9 standard deviations distant from Ptolemy’s $-14'$. (Similar disconnect for the 1025 celestial latitudes β of the *Alm* 7.5-8.1 star catalog.) So the δ were plagiarized from a contemporary²¹ anonymous observer who knew his L .

E3 Confirmation is achieved via statistical induction (Rawlins 1994L §F8) of the exact latitude $L = 31^\circ 1/4$ assumed by the observer of *Alm* 7.3’s Clean Dozen stars — that is, $17'$ higher than Ptolemy’s stated latitude of $30^\circ 58'$ (§E1): a hard conflict which alone shows that he had nothing to do with the Clean Dozen stars. Besides stealing them.

E4 At *GD* 4.5.76 Ptolemy lists the Alexandria harbor Pharos lighthouse’s L as $31^\circ 05'$, blithely copying this false L from Hipparchos-Strabo (Strabo 2.5.39; Neugebauer 1975 p.1313; Diller 1984 fn 23; Rawlins 2009S fn 16). Question: Since his *Alm* had already (§E1) put Alexandria instead at $30^\circ 58'$ (consistent with *GD* 4.5.9’s $5'$ -rounded 31° value) why didn’t an Alexandrian²² & allegedly-outdoor observer notice he’d thus inadvertently stretched by ordmag TEN-fold the 7-stade-long embankment connecting Alexandria to its Pharos, an embankment explicitly named Heptastadion (επτασταδῖω: Strabo 17.1.6)?

E5 So by carelessly²³ copying disparate Alexandria L data from Vitruvius (§E1) and Hipparchos (§E4), “astronomical observer” Ptolemy adopted — simultaneously — two L

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

²⁰ Excluding no stars when bivariate-least-squaresing the Clean Dozen produces the L -error cited, leading ultimately (carefully trace Rawlins 1994L §§F3-F9) to the observer’s $L = 31^\circ 11'\pm 2'$ (*ibid* Table 3) — consistent with Alexandria’s $L = 31^\circ 12'$. Same process produces the Clean Dozen’s epoch, 159 ± 8^y , in fine accord (*ibid* fn 45) with the 10th century *Suda*’s dating of Ptolemy to epoch Marcus Aurelius 1 (160/7/14). Previously unnoted vastness of *Alm* 7.3’s leapfrog anachronism: the Clean Dozen δ were observed c.160 AD (as just shown) and then merged with the SackSix δ which were precess-faked so inadequately for stated epoch 137 AD that their coherent 5 stars’ mean is (fn 37) instead correct for over 100^y before 160 AD! There are two unknowns when analysing ancient star-declination lists: the observer’s epoch E and the error x in his assumed latitude. For the four observers whose star declinations are discussed in *Alm* 7.3, the curious paper, Brandt *et al* 2014B (discussed also in fn 37), gets mostly non-outré results for epochs E yet for all 4 cases messes up the other unknown, x , the error in the observer’s assumed latitude. For the 4 observers, the figures given (*op cit* p.331) for the latitudes’ “accuracy” (which the authors compute instead of x) are: Timocharis $0'.72$, Aristyllos $0'.18$, Hipparchos $0'.24$, Ptolemy (?) $0'.3$ — values which are oversmall by an ordmag. Likely-wasted-at-present wakeup to the history-of-science community: outside of *DIO* (¶3 Table 2, or Rawlins 1994L §F9 & Table 3) no paper on the *Alm* 7.3 declinations has ever correctly understood how to find both E and x [& associated standard deviations]. The 2014 paper also errs in dropping near-solstitial stars for being weak indicators of epoch — forgetting that they are superior indicators of x , and thus matter in gauging the accuracy of the 4 men’s adopted latitudes for their observatories.

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

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

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

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) — $31^\circ 05'$ — as for his home Serapic temple at Canopus from where he had only to look down the Mediterranean coast after dark to see²⁴ that the 12 nautical mile (nmi) distant Pharos flame was slightly *over thirty degrees south of due west*, so the two sites’ L could not possibly be the same. (Real L difference: $12 \cdot \sin 30^\circ = 6'$.) Further evidence that Ptolemy “doesn’t seem to have allowed his eyeballs out at night” (Rawlins 1985G p.266).

F IMPERVIOUS MERCURY

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

F2 But the difficulty for Ptolemy’s loyalists is this: his *Canobic Inscription*, written some years before the *Alm* (as proven in the brilliant paper, Hamilton, Swerdlow, & Toomer 1987), listed precisely the same Mercury mean motion, but most of the other elements differed. So: how could the same mean motion have been empirically and mathematically based for both works, if the respective derivations involved discrepant elements? E.g., deriving the 139 AD position for the *Canobic Inscription* elements versus doing so using the *Alm* elements, produces results disagreeing by over 5° . Yet The Greatest Astronomer of Antiquity gives the identical mean motion in both works, to six sexagesimal places, and this speed is anyway precisely computed not from the alleged observational base (angular-arc-traversed/time-interval) cited at *Alm* 9.10 but from the period-relation $52200^\circ/16802^d 24'$ found at *Alm* 9.3. [Full calculation of 5° discrepancy: Rawlins 1987 p.236-237.]

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

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

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

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

²⁴ Check Rawlins 2008Q (§C1 & eq.23) for the Pharos’ sea-level visibility-distance, about 20 nmi, or $1^\circ/3$ of great-circle measure on the Earth’s surface. (*ibid* eqs.23 and 24 reveal [using www.dioi.org/cot.htm#kchg] that Sosstratos measured the distance as 20.2 nmi.) The remark on Ptolemy’s-eyeballs of course assumes that our Greatest Astronomer even knew (or cared) which way was north. (Canopus today is Abu Qir, site of 1798’s Chapter 1 in Horatio Nelson’s serial destruction of Napoleon’s fleet; also near the 1894 birthplace of mystic [and #3 Nazi] R.W.R.Hess.)

foreign eclipse times couldn't have supported the longitudinally-stretched geography (§G4) he borrowed (with credit)²⁵ from Marinus of Tyre. Instead, Ptolemy's vast opus provides (*GD* 1.4.2) but *one*²⁶ example: two longitudinally much-separated reports — 500^y old! — of the famous Arbela —330/9/20 lunar eclipse's start, saying it was seen there at 23^h and in Carthage at 20^h, thus proving that the 2 places are 3^h or 45° apart in longitude.

G3 However, Pliny earlier reported the same data very differently: 20^h (8 PM) for Arbela (modern embattled oil-city Irbil) and 18^h (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^h off for Carthage, 3^h off for Arbela. The former error nearly equals the entire actual 2^h1/4 longitude gap between the sites, and the latter error far exceeds said quarry. But the weirdest part is yet to come: Ptolemy's *own lunar tables* put the eclipse just about as much in disagreement with his reported times as modern tables do: 2^h Carthage and 3^h Arbela. How explain such an entertainingly disastrous fabrication? Start by consulting Pliny 2.72.180 on the —330/9/20 lunar eclipse. Reading the passage carefully, one sees that no numerical hour is given explicitly for the western apparition in Sicily, merely: moonrise (“exoriens”). By contrast, the Arbela time is given as the “2nd hour” after sunset, or about 20^h, which is the very time Ptolemy gives for the *Carthage* report. Why? Well, look carefully at the Pliny passage cited: by a fluke of grammar, “secunda hora” appears nearer in the sentence to “Sicilia” than to “Arbelam”. This obviously suggests that Ptolemy used Pliny or his source but (evidently unable to read Latin well) took Pliny's 20^h time to be Carthage's.

G4 But how did Ptolemy arrive at 23^h for Arbela? Since Gossellin 1790, it has been obvious that multiplication by an expansion factor (Diller 1984 §C5) had been applied by Marinus or Ptolemy or their source to a prior map's accurate longitudes, creating the oversize longitude intervals of the *GD*. Rawlins 1985G eq.15 showed that the expansion factors were either 7/5 or 4/3 in the region under consideration. Assuming that the earlier accurate map correctly put Arbela 2^h1/4 east of Carthage, then expansion by 4/3 would produce 3^h, the very gap — the very wrong gap — Ptolemy reports. I.e., typically for him (and his defenders) the conclusion was established ere the evidence was engaged.

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

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

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

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

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

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

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

H2 It thus became obvious c.1990, even to the most religious, that many Ptolemy stars were Hipparchan. However, no archon was ever going to admit in print the plain truth: the establishment had been blindsided by proof that its challengers had been right all along — that Graßhoff's test had now unexpectedly surprise-vindicated the long-loathed Tycho-Newton-Rawlins position that the Catalog was stolen. (The post-disaster spin of some was that the three pioneer skeptics had just regrettably not proven their case *clearly enough to be underdood* by the judicious archons atop the American Astronomical Society's Historical Astronomy Division [H.A.D.] — surely a truth-determination criterion to live by — while Graßhoff had. Which translates as: [a] ashamed refusal to acknowledge that — until the truth hit them in all their faces — believers had been too limited and predisposed to see anything significant in the same evidence from which skeptics had drawn the right conclusion years ahead of final proof; [b] denial of credit to unapproved first perceivers, according to a principle handed down to us from on-high, the *JHA* itself [quoted at Rawlins 1991W fn 127, emph added], “the first speculative occurrence of an idea is generally *far less significant* than its later emergence, possibly in other hands, supported by persuasive arguments.”) The post-Graßhoff era has been especially fertile for indiscriminate fallback apologies by Ptolemy's (selectively) malleable modern choir, as the politically ambitious realized that the *JHA* would ever so gratefully²⁸ publish *anything* that muddied the clear evidential situation, in order to save archons from facing apt appreciation for decades of falsely denigrating now-vindicated scholars: simply pretend vindication either never happened or isn't 100.00000000%. Question: is anyone empathizing with the cornered defenders' needless pain here, caused by artificial extension of the Controversy? (Puts one in mind of equally needless ongoing misery from other kinds of poverty than intellectual — mass-agonies likewise of insufficient concern to those who subsidize its perpetuity, to ensure their own perpetuity in office.) This is so literally *pathetic* — Chauvin's shade shuts his orbs in shame at what his legacy has come to. [1] Memory-hole-unapologetic for his original 1987 and 1998 arguments that Ptolemy probably outdoor-observed the whole catalog, Evans now just hopes that Graßhoff 1990 hasn't proven that *all*²⁹ stars were copied from Hipparchos. [2] Schaefer (2002) says the Yale Bright Star Catalog also grabs previous catalogs' stars, so what's the concern? However, both these defenses of Ptolemy's *integrity* plainly founder upon his claim of 1st-hand observation of *all* 1025 stars. And argument [2] is on the logical and ethical level of defending a bank-robber by pleading

²⁸ Rewards handed out to those who attacked the R.Newton satan include *JHA* boardship (R.Newton 1991 fn 2) and a MacArthur for miss-man Swerdlow. (It's hard to find good help anymore.) Among other examples: maid-men Evans and Schaefer were elevated at *JHA* not long after their massive bungled 1998 and 2001-2002 attacks on Rawlins. (The subtlety 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 explicitly³⁰ does so claim, at *Alm* 7.4, in lengthy detail, falsely saying he observed every visible star (§K1). Bottom lines: [i] The *JHA* committed itself repeatedly to the proposition that the Catalog was all or mostly Ptolemy's. [ii] It isn't. [iii] But, simply from shame-factors detailed elsewhere here, our "premier" *JHA*-H.A.D. (JHAD) solipsistically hallucinates — like Dr.Frederick Cook or Alger Hiss — that if we just never confess, then no one will ever know the truth: that we Experts lost what has correctly been advertised nationally by Schaefer 2002 as the hottest controversy in the field. [iv] But neutral observers increasingly and snickeringly do know — which is marking certain JHADists as losers to scholars they themselves have long been assuring the world are crazy dishonest paranoid incompetent cranks. And we're not supposed to giggle? (You begin to see why the seething losers can never admit it?)

H3 Pickering 2002A §B1 & Fig.1 points out a history-of-science-ignored ultra-simple disproof of Ptolemy's Catalog authorship (Rawlins 2000A fn 177): the 5° gap which should exist between the antarctic circles³¹ of Hipparchos' stars and Ptolemy's stars (due to their differing latitudes) does not exist. The 2 circles are virtually identical. Controversy over.

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

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

I1 The Ancient Star Catalog (*Alm* 7.5-8.1) has an obvious excess of 00' endings and 30' endings in the latitudes β , due to ancient Egyptian and Greek proclivity for expressing non-integers by using inverse integers: "unit fractions". (Cause of both excesses detailed at Rawlins 1994L §B4.) But the most common ending for the longitudes λ is 40'.

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

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

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

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

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

I5 So R.Newton (§I2) explained why longitudes λ are near-bereft of 15's and 45's (only³² 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 $L = 35^\circ 53'N$), but not with Alexandria's $L = 31^\circ 12'N$. [B] Shevchenko 1990 p.194 discovered for a specified half of the zodiac, stars' λ exhibited no particular excess of 40's. Later, *DIO* 10 (2000) fn 177 tested Gem-to-Sgr (roughly Shevchenko's range) and found that — excepting Sco, whose prime stars' β reach atypically far south for the zodiac — these stars' original Hipparchos 00'-excesses were not in ecliptical longitudes but in *polar* longitudes. The novel and insuperable impediment thus created for Ptolemy's defenders is found in the footnote cited.

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

I8 Some Ptolemists have improbably dodged Ptolemy's other (Sun, Moon, planets) fakes by claiming they're just innocent pedagogical illustrations of his theories (ignoring the inconvenience that he repeatedly calls them real 1st-hand outdoor data — Rawlins 2002V fn 12); but over 90% of the Catalog's stars are never used in Ptolemy's "illustrations," so the threadworn PedaDodgical Ploy cannot excuse his explicit claim (§K1) of 1st-hand observation of all 1025 stars, a theft statistically lock-proven by Grafhoff (above, §H1).

I9 A different defense tactic goes the you're-another route (earlier variant at §H2), citing "other" scientists than Ptolemy who fudged data (conflating their occasional over-optimism with Ptolemy's flagrantly consistent M.O.). But, again, among these, only our Greatest Astronomer of Antiquity ever stole a thousand stars — the factor that (as in §I8) separates the "mere" Fudger from the naked thief.

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

J CONCLUSIONS

J1 The *Alm* is an invaluable resource, our only connexion to much of high ancient mathematics. Given what *DIO* has induced from it (*passim*), we are (as distinguished from R.Newton)³³ especially grateful for its survival. But it must be used with extreme caution.

J2 The most educational observation we may end with, regarding the timorous state of the modern history of ancient astronomy community, is this: its fiscal rulership can read all that you have just read and, though finding not a digit out of place in the analyses, conclude or pretend that: [A] Ptolemy has done *absolutely nothing* dishonest, and [B] DR should continue to be non-cited³⁴ for [1] his witchcraft (fn 35) in co-hypnotizing scholars (fn 1) into realizing Ptolemy cheated, & [2] exposing the vile tactics of archons who'll never admit they were wrong to 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 amount of earlier astronomy that he has preserved. Instead, we owe him our condemnation for the large amount of genuine astronomy that he has caused us to lose." (Note Toomer 1984 p.1's naïve guess: "the work of Ptolemy's scientific [!] predecessors . . . being obsolete, . . . ceased to be copied.") See Neugebauer 1957 p.145 & Rawlins 2008Q §K3. Neither van der Waerden nor Rawlins 2008Q fn 223 agree with RRN's estimate (*idem*) that data Ptolemy reports from others are faked though (as most loyalists don't deny) routinely reduced to produce impossibly consistent desired "confirmations" of his models. Basic confusions of those who see Ptolemy's derivative astronomy as primary: §M1 [b].

³⁴ It is little understood or anticipated that any decision to shun (usually made in anger or fear, initially) needlessly and precipitously puts the instigator's integrity at risk. For, the decision is a bet: gambling that the shunnee is forever worthless — oblivious to the possibility that his output may prove valid (or later start to be), at which point, how does the bully-invested, no-turning-back shunner then justify continued non-citation? For saving faces (and what else matters to archons?), he has no choice but to start faking the output's invalidity — not knowing (as he gets progressively deeper-in) when if ever the deceit can stop. As we get to the point where evidences **CENTRAL TO THE FIELD** (e.g., §§N11-N17 and N18; fnn 1, 7, & 47 item [1]) cannot be openly discussed without fear of archons — or editorial submission to such (fn 1) — we've descended into knowledge-destructive sociopathology.

³⁵ Among *JHA*-circle herd-talk compliments toward R.Newton and his solid mathematical analyses have been the following: "incompetent" (A.Aaboe), "up a pole" (J.Field), "disreputable" (Toomer 1984 p.viii), and (all Swerdlow) "silly," "careless and unreliable," "Velikovskian," "absurd," "crank," "con-man," "insults the intelligence of the most naïve reader" (sources for all but Field [Greenwich, 1984] at *DIO 1.1* ‡1 §C7 & ‡3 §§D2-D3). And see *Scientific American* at fn 52. If taking academe's pretensions at faces-value, one might've thought that the *JHA* or the American Astronomical Society's H.A.D. would insist upon its officers engaging in rational academic discourse. Instead, we've had 4 decades of their dereliction and/or equally inexcusable non-comprehension (despite the author's vain 2002/10/2 request of stonewalling American Astronomical Society chief Milkey that he look into the conduct that has characterized the controversy), while since 1968, JHAD missionary Gingerich — soon after joined by Swerdlow — has (Rawlins 1994S §§B5-B8) lauded jihad against infidels and tried hermetically whackamoling all public doubt on Ptolemy (reincarnating those who burned books and sorcerers in the Dark Ages, to contain another heretical disease), privately contacting any forum or party or person who dissented or was about to: *Nature, Science, Scientific American, Rawlins* (1974), CSICOP, Martin Gardner (1978), Horace Judson (2004), etc — to assure them that the *skeptics* are dishonest crank incompetents, correctly confident that the non-specialist gulliee would believe he need not check alleged supporting evidence of such eminent personages [‡4 p.87]. Apparently, the notion that a MacArthur Genius and a Harvard professor could themselves be either shall-we-say Limited (fn 8) or shall-we-say not-overstrictly-truthful (fn 5) or both, has heretofore seemed just too outré for non-scientists and writers (unfamiliar with the JHAD) to believe, thus popular media (if we naïvely assume power-secretaries' naïvete) continue to be near-100% buffaloed by our dedicated genii. (Rawlins [like even Gardner&Judson] was similarly fooled [1974/11/15, back when he wasn't yet into Greek astronomy], for over a year, by Gingerich saying Newton was just a crank; see Rawlins 1994S §C8.) Question-in-passing: how do scholars of the refined character and competence of Swerdlow and Gingerich keep getting to be eminent in the 1st place? Their JHAD is even now hoping to assert further Dean-Wormeresque Double-Secrecy [§N17] by acquiring Double-Peek fail-safe control over

K APPENDIX 1: GRUSOME TESTABILITY WARS

K1 Delambre 1817 2:284 was 1st to notice that, in Alexandria's 2nd century AD sky, some stars which transited a few degrees above the southern horizon, and were bright enough to have been recorded by hypothetical-observer Ptolemy, were nonetheless not in "his" Catalog — and, by-an-unfunny-coincidence (explored statistically in Rawlins 1982C), *all these unCataloged stars were invisible to Hipparchos*, who observed 5° north of Alexandria, so that his antarctic-circle (fn 31) of invisibility was radially 5° bigger than Ptolemy's (swallowing about 4/3 more sky). Automatically fighting the probable implication while unable to deny any facts, Evans 1998 p.272 resorts to the improbable (§B3 [a]), in order to set aside such simple antarctic-circle testing, speculating that because no previous mass-star-cataloger known to us had resided so far south as Ptolemy, there were no constellations to which he could attach stars in the 5°-wide strip of sky he could see but Hipparchos could not (and which no hypothetical early southern constellator had filled), so we must excuse Ptolemy — excuse him, that is, for not doing what The Greatest Astronomer of Antiquity himself³⁶ actually says he did, namely, record *all* visible stars (*Alm* 7.4, Toomer 1984 p.339): "we observed as many stars as we could sight down to the sixth magnitude." Evans' dodge typifies modern Ptolemism's death-agonies: *alibing one Ptolemy scientific crime too often requires positing another*. (Check out, for example, fnn 30, 37, & 42.) I.e., even if one accepts Evans' argument, it simply exchanges a charge of plagiarism against Ptolemy for a charge of lying.³⁷ But Evans' theory fails anyway since

this paper's revelations. (Ref-report to *JAHH*: "If DR revises the manuscript, I would be happy to look it over.") Just as Gingerich did with R.Newton *forty-seven years ago* [now 50[!]!] — details at Rawlins 1994S §B13. After all: must protect even 'til-now-undefined Thailand and the antipodean Land-of-Oztrollya [‡4 §A2] from the full truth about the integrity & ability of Ptolemy and his never-too-far-away Muffia public-relations organ.

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

³⁷ Similarly, a recent paper (Brandt et al 2014B; see also fn 20) tries to deny the certainty of Ptolemy's fabrication (from Hipparchan data) of any star declinations δ found in *Alm* 7.3. Various modern scientists have noticed that Ptolemy "proves" his false 1°/cy precession from his 18-star sample using the worst six stars (the "SickSix") and ignores the valid twelve (the "Clean Dozen") which would have accurately yielded 1°.4/cy. From the consistent 6-fold persistence of the SickSix stars' neatness, R.Newton 1977 pp.220-225 realized that Ptolemy had typically (above, §B2) just indoor-computed the SickSix declinations from his 1°/cy theory — and then turned around to "prove" 1°/cy precession from said fakes. The 2014 paper instead conjectures that Ptolemy quietly stole stars from a Lone-Mystery-Observer of 57 AD, though the proposed LMO is uncited by Ptolemy or any other ancient, so *his existence lacks the slightest independent evidential indication*. And the proposal inadvertently trades a charge of fabrication (Newton, *loc cit*) against Ptolemy for a charge of plagiarism: yet another example (above, §K1) of Ptolemists fumble-lawyering to refute one of the hero's crimes while not remarking or even noticing that they're simultaneously stipulating to another! And, again-typically (above, §B3 [a]), choosing improbable theory over probable. Unconsidered question, quite aside from the issue of theft: from where and with what degree of inexcusable **ignorance** would Ptolemy (uniquely, even for him) acquire — steal — a small set of star-declinations obsolete *by a century* (fn 20) while purporting that he observed them himself (*Almajest* 7.3), and insert them into a set of contemporary declinations? **This is proposed as a central new theory in a DEFENSE of Ptolemy's honesty?** Innocently unanticipated by the paper's authors: the 2014 paper's proposed 57 AD date for the former is near the date which Peters & Knobel 1915 p.15 found for when the Star Catalog's mean longitude-error is null: 58 AD. But we know from independent evidences (§I) that the Catalog was stolen by adding false precession 2°40' to all λ (in Hipparchos' -126.278 catalog) causing an 8 decade shortfall from the intended 137 AD epoch, thus the Catalog's naïvely-indicated 58 AD date is just as much an illusion as defenders' proposed proximate 57 AD date for the Sick δ . If not, then are we to suppose that our secret LMO just-so-happened to pop up at the very time that agrees with precession-deficient fabrication from Hipparchos? — an epoch that's nearly the same for Ptolemy's declinations-list AND his Catalog. **NB:** Ptolemy's SickSix fraud-mimicry in stellar declinations is especially obvious since his outlier Arcturus' δ conspicuously is correct for just over 50[!] later than the date consistent with the remaining stars —

various of the non-cataloged Alexandria-visible stars were conveniently attachable to nearby constellations. And we know that Ptolemy was (or copied) a star-attacher: *Alm* 7.5-8.1 lists dozens of “informatae” stars which are in the vicinity of traditional constellations though still outside them, but which he nonetheless appends to them. This includes even Arcturus. (Which we designate as PK110 — meaning star #110 in Peters & Knobel 1915.) Further, the vast constellation Argo (today broken into pieces: Car, Vel, Pup, etc) had already been recognized for centuries, and the Catalog includes 45 of its stars (PK849-893); yet several Argo stars aren’t in the Catalog (bright but unCataloged ϵ Car [$m = 1.9$] is less than 6° from Cataloged δ Vel: PK886), despite being easily visible from Ptolemy-era Alexandria (post-atmospheric-extinction magnitudes μ ranging between $3\frac{1}{2}$ and 5: Rawlins 1982C Table 3), though not from Hipparchos’ Rhodos, since all were (see *idem*) of such dim μ as to be beyond Hipparchos’ in-practice mean magnitude limit³⁸ μ_0 for capture. Even more peculiarly absent from the *Alm* catalog are α and β Gru. Both of pre-extinction magnitude $m \approx 2$ — i.e., of Big Dipperish prominence! — and quite visible (§K2) to Ptolemy at μ about 3 and 4, resp (though at all hours *below Hipparchos’ horizon*), they could have just been set aside as a new³⁹ constellation. After all, [i] There already was a two-star constellation, CMi (PK847-848); [ii] Ptolemy was inventor of the new⁴⁰ asterism Antinoüs, which he formed c.130 AD from six⁴¹ stars “around” Aql (Toomer 1984 p.357).

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

the SickFive, whose weighted mean error vanishes c.59 AD — since he faked it to co-prove false $1^\circ/\text{cy}$ precession from Hipparchos’ atypically *very*-inaccurate Arcturus δ which was accidentally correct for a time later than his actual epoch by (you guessed?): c.50^y. To emphasize this glaring giveaway by summation: if the SickSix sample were actually observed in 59 AD (& its SickFive do indeed have near-null declination-error for that date), it is remarkable that the lone Ptolemy star (Arcturus), whose δ -error goes null c.50^y after the LMO, should just-happen-to-also-be the lone Hipparchos star whose δ -error also goes null c.50^y later than Hipparchos. (To be exact, 52^y after Hipparchos; 56^y after LMO.)

³⁸ Hipparchos’ μ_0 was a bit dimmer than 5: §L3. For his deep south sky, virtually every identifiable star brighter than $\mu = 5$ appears in the Catalog. Note: this is about the μ (Rawlins 1993D §J4) at which Tycho’s normally high star-cataloging accuracy begins to fade. (Tycho had 14% less sky visible to him than Hipparchos, so he had to go a bit deeper in μ to approach his goal of netting 1000 stars.)

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

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

⁴¹ Has it previously been noticed that not one of Antinoüs’ six stars appears in the Hipparchos *Commentary*’s Aql, or, indeed, anywhere else in that work? The stars were PK295-300; or from Bayer: $\eta, \theta, \delta, \iota, \kappa, \lambda$ Aql. (Three of the six λ endings are $40'$ or $10'$, vs two randomly expected, a slight hint that Hipparchos observed them after writing the *Commentary*.) The group was named for bereaved Emperor Hadrian’s recently Nile-drowned teen boytoy (Rawlins 1992V fn 44) and presumably to thank the emperor for his visit and for favoring (perhaps initiating) imperial sponsorship of the Serapic astrological-medical superstition the Canopic temple specialized in.

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

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

L2 A passing alibi by Evans 1987 p.166 even imaginatively hints that perhaps there were, say, rocks just-south of Ptolemy’s putative observatory that just-so-happened to block just-enough southern sky as to make his putative observations’ declination-range deceptively look⁴² as if the observer were at Hipparchos’ latitude L — instead of where Ptolemy’s

⁴² Likewise, Schaefer 2001 proposes that atmospheric aerosols instead of rocks blocked Ptolemy just enough to fool us into mis-concluding that most of the catalog was observed at about Hipparchos’ Rhodos Island $L = 36^\circ\text{N}$. This requires assuming (contra Pickering 2002A §F) that Schaefer’s modern Alexandria daytime [mean] atmospheric stats applied to ancient nocturnal air, and that the cataloger was so dumb as to not realize that the very clearest nights were those appropriate to searching out dim stars. Since Schaefer (*op cit*) astonishingly neglected consulting the stars in Hipparchos’ *Commentary*, the paper doesn’t even realize that (given the virtual identity [§H3] of the antarctic circles of the stars collected by Hipparchos and by Ptolemy) his argument inevitably requires that Alexandria’s aerosols were much greater than Rhodos’ — again, conveniently, by enough for pseudo-indicating a Hipparchan latitude for the Catalog’s main observer. Dense turbidity proponents Schaefer and (less incautiously) Evans propose opacities that are obviously over-high for antiquity. Schaefer (*op cit*) choosing an opacity of 0.23 mags/atm happens to put the star γ Ara at $\mu = 6.7$ (Pickering, *op cit* §B2 independently verified by Rawlins): probability of Hipparchan capture $P < 1/1000$. The only way to make it remotely possible (for 0.23 mags/atm) that γ Ara could have been recordable by Hipparchos would be to adopt (instead of Rhodos City’s $L = 36^\circ.4$) the Rawlins 1994L §E4 Hipparchan position (§I6) for observing southern stars at Cape Prassonesi ($L = 35^\circ 53'$, vertical distance above sealevel $z = \text{c.}200\text{m}$), making $\mu = 6.2, P = 1\%$; but Schaefer has never acknowledged that Prassonesi could’ve been Hipparchos’ south-Rhodos observation post. (See discussion at Pickering 2002A §§B2 etc, regarding Schaefer’s intelligent [if only slightly mitigating, in this case] argument that γ Ara’s low P should be seen in the context of several other similarly situated stars [too few of which are of identifiably bright m], even while he himself remains impervious to the larger context of Pickering *op cit*’s numerous other strong, mutually-verifying clear-atmosphere proofs.) Still at Prassonesi: Evans’ preferred 0.2 mags/atm makes $\mu = 5.8, P \doteq 6\%$. (But he is creditably willing to admit the possibility of opacity as low as 0.17 mags/atm, which would leave $\mu = 5.4, P \doteq 1/3$.) Schaefer’s impressively-published case for an opacity which inadvertently worked against Hipparchos’ recording γ Ara makes an even more impressive impact upon us when we learn that Hipparchos actually did record γ Ara: it’s found at Hipparchos’ *Commentary* 3.2.6 (which Schaefer was unaware of at this time, since he didn’t consult that central work until Pickering told him about γ Ara face-to-face at the 2001 H.A.D. meeting). By contrast, γ Ara’s visibility to Hipparchos is reasonable by Rawlins’ 0.15 mags/atm opacity, which has the star’s μ at 5.2, very near Hipparchos’ capture-limit μ_0 , with capture probability $P \doteq 1/2$. (See Hipparchos-capture probability-function by Rawlins 1982C p.363 — and its later independent 2011 confirmation at www.dioi.org/cot.htm#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 ($L = 36^\circ 40'$). Rawlins’ $\mu = 3.2$, while Schaefer’s 0.23 mags/atm makes $\mu \doteq 6\frac{2}{3}$. The entire thick-atmosphere line-of-alibiing was squashed by Pickering 2002A (the best paper, ever, on the atmosphere debate). This (*ibid* §D9) is the 1st adducement (for this case) of Eudoxos’ sighting of α Car ($h < 1^\circ$), also of Hevelius’ recording of ν 1 Eri (*ibid* 14) — both far too dim for visibility by Schaefer’s opacity. Pickering (*op cit* §F) further demonstrates that bright stars were visible *on the horizon* in antiquity — most spectacularly by revelation of the hitherto-unconsidered fact that Hippocrates’ and Ptolemy’s achronychal rising/setting data for Arcturus and Saturn has to be referring to these objects’ ancient visibility *ON* the horizon since achronychal effects *cannot even be defined* at

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

L3 Hmm. Why do partisans allow their enthusiasm to proffer already-vulnerable-enough arguments without even testing them? Here, one need only, both for Hipparchos' and for Ptolemy's epoch and latitude, list the sky's stars (bright enough to be clearly identifiable in the Catalog) in order of [a] post-extinction magnitude μ and [b] apparent altitude h above the horizon. If, in Ptolemy's list [b], all the stars above $h = 6^\circ$ 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 sealevel), Hipparchos' list [a] exhibits a startlingly clear⁴³ split at a post-extinction magnitude μ_0 slightly less bright than 5 (obviously his effective limit for capture): the stars dimmer than μ_0 are not in the Catalog, while those brighter than μ_0 are.⁴⁴ Comparing these sensible results, to those gotten from applying the same Magnitude Split Test (*DIO* 9.1 1999 p.2) to The Greatest Astronomer of Antiquity's Alexandria, will (fn 43) give any scientist a hearty upchuckle.

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

M1 How did too much of the academic establishment get sucked into promoting astronomical history's ultimate pretender as the "Greatest Astronomer of Antiquity"? [a] Were public attacks on a famous scientist resented by science's politicians as endangering science funding?⁴⁵ — but astrologer-mathematician Ptolemy was not a scientist. Not empirical.

any other altitude than $h = 0^\circ$: see the lucid and irrefutable discussion at *ibid* §F11. Further, thanks to an amazing ms-recovery by B.Goldstein, we now have the fact (Rawlins 1993D §L8) that Ptolemy said in so many words that 1st magnitude stars (pre-extinction $m \doteq 1$) were visible on the horizon in antiquity. (In the exchanging-frauds tradition noted at §K1, some loyalists disbelieve this, thereby [*ibid* fn 93] assuming Ptolemy's "horizon-stars-dishonesty [in order to argue] his Catalog-stars-honesty".) But, if we assume Evans' preferred (0.20 mags/atm) modern-model opacity, then 1st magnitude stars' visibility on-horizon entails (*ibid* §L8) ancients' eyesight perceiving 12th magnitude stars ($\mu \doteq 12$); and Schaefer's 0.23 mags/atm similarly entails ancients seeing to 14th magnitude ($\mu \doteq 14$). So: why didn't Archimedes beat Clyde Tombaugh to the discovery of Pluto?

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

Compare sealevel-Alexandria Ptolemy's §L3 list [a]: V at 0.15 mags/atm, $V = 5/4$ (β 1 Sgr vs α Cru). And for mags/atm = 0.23, 0.3: $V = 4/5, 3/4$, resp (γ Ara vs α Phe in both cases).

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

⁴⁵ The half-century Ptolemy Controversy should have been over in half an hour — had all participants amicably cooperated at the outset and sat down together to compare data and enlighten each other. Had defenders realized early on, before positions hardened in ignorance of, e.g., the significance of Ptolemy's large error for Alexandria's geographical latitude L (§E1) and real ancient scientists' high-accuracy achievements (§M3), participants would (Panglossianly assuming open minds all around) have soon shaken hands, parted in peace — and moved on to more challenging historical mysteries. (As *DIO* long since has, most of our research on ancients being reconstruction of lost astronomy.) Instead, Ptolemy's promoters from day-one followed his example by knowing all the answers before consulting either empirical evidence (as against texts) or actual able scientists (as against cult gooroos). Decades of ugly and harmful warfare followed. Again: all needless. But as with many wars, when it becomes obvious to most observers who's going to lose in the long run, there is a bloody period when

[b] Were Ptolemy's math proofs so admirable that it seemed incredible for him to have plagiarized data? — but (Rawlins 2003X p.502): what if he plagiarized the math, too? It was long believed that the *Alm*'s spherical trigonometry proofs were original — until 1901, when it was found that they were taken from Menelaos (c.100 AD): Pedersen 1974 p.73 n.9. [c] Inevitably-feeble attempts to logically back up prominently published one-sided (fn 11) salesmanship, pushing Ptolemy as The-Greatest, put one in mind of Aquinas' voluminous Reformation-germinating mistake of trying to defend by reason that which cannot be defended by reason. [d] Did damage to Ptolemy's sacred-grant-cow value trigger the shunning (§B) of R.Newton's valuable insights? With the *Almajest* as [i] the central surviving ancient work on mathematical astronomy but [ii] suffused with fraud, a grant-raising problem was presumably feared (perhaps needlessly: §J1), leading to attacks on Newton, but (far more tragic and longstanding) promotion of a now-widely-accepted misperception of *all of ancient astronomy* — just to cover for Ptolemy's fudges, by deliberately (fn 46), falsely claiming that everybody-did-it (§M2) in antiquity — a distortion that's gatewayed a 180° inversion of truth for a range of ancient-science issues, as detailed at §N, below. Above option [d] was the most likely place for the original flame of rage at Ptolemy-skeptics to have started. Newton used to note that the last century has seen numerous charges of historical fraud in the physical sciences, but none produced a fraction of the ferocity of Ptolemists. [Wherever there's a weak, rationally-indefensible tenet, advocates are left with no other way to protect it than (e.g., Rawlins 2017C fn 1)] by suppression, banishment, indiscriminate argumentation, and circulation of way-overdone baseless or irrelevant personal denigrations against opponents. So when we see such phenomena we should sense said weakness. As a general rule that can save plenty of time and bother:

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

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

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 alibi Ptolemy by wrenching academe’s view of ancient astronomy *to fit him* has caused as much damage to modern scholarship (§§M1&N) as Ptolemy visited upon ancient and (above, §A) medieval.

M3 “Acquittal” adds that ancient astronomers “were mathematicians who concerned themselves with proof, rigor, logic, and consistency rather⁴⁷ than with observational accuracy.” Gingerich 1976 p.477 approvingly quotes Neugebauer 1975 p.108, “It makes no sense to praise or to condemn the ancients for the accuracy or for the errors in their numerical results. What is really admirable in ancient astronomy is its theoretical structure. . . .” See also Neugebauer on Aristarchos’ data as non-empirical (Neugebauer, *op cit* pp.642-643; Rawlins 2008R §A1). How then did Aristarchos and Archimedes (*idem* & Rawlins 2012T §E1) find the solar diameter to ordmag 1’ accuracy? How did ancients find the mean distance to the Moon within c.2% (59 Earth-radii: *Alm* 5.13)? Or find their observatories’ geographical latitudes to ordmag 1’? (See [¶4 Table 1] or Rawlins

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

⁴⁷ To back The Acquittal’s fantastic crock that ancient scientists weren’t accurately empirical, Evans 1987 reports that on 1981/7/16 he observed from Seattle the longitude of star λ Sgr by measuring via cross-staff its angular distance from a lunar eclipse and found that his result was off by $2/3$ of a degree, nearly triple the angle from lunar center to limb! This error is about equal to that of indoor-Ptolemy’s impossible 135/10/1 lunar report (rightly deemed fraud by R.Newton 1977 p.190), but (seemingly more relevantly) also similar to outdoor-Hipparchos’ huge errors twice (*Alm* 3.1) when also comparing a star (Spica) to the Moon (during eclipses of —145 & —134). Pointing to all 3 large misses in the eclipse-star observations (by himself&Hipparchos), Evans triumphantly concluded: “No better demonstration could be wished” of 1° uncertainty in naked-eye observations, preaching that such Real-Science work “might temper [Ptolemy-skeptics’] judgements regarding the precision achievable” by ancients, not-very-implicitly asking: So, Msrs. Newton & Rawlins — NOW what’s so criminal about Ptolemy’s one-degree-erroneous observations?

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

Until Rawlins 1991W fn 288 and Rawlins 2009E §A & fn 7 applied the theory that Evans and Hipparchos had simply committed a sign-error in parallax when reducing each observation — a theory that decades later also proved just as neatly fruitful for Regulus’ hitherto-inexplicably huge —35’ Hipparchan error (¶3 §B6). Recomputation showed (*ibid*) that *all four* ordmag-1° observational errors of Evans (Seattle 1981) and Hipparchos (Spica twice and Regulus) shrink to ordmag 1’. (I.e., all four errors were primarily those of reduction, not observation.) Warned of his sign error by Rawlins 1991W and in 1997 by Thurston&DR, while Evans 1998’s text was still unfinalized, its pp.257-258 ever-so-subtly Memory-Holed his 1981 no-better-demonstration data and switched to a different eclipse (no observed data recorded: from 1977 Spokane, an eclipse never mentioned [Rawlins 2009E fn 7] during Evans 1987), continuing, while sticking to only Hipparchos’ slips, the same Sermon-on-the-Muff (¶3 §B4), just reprinting his 1987 argument (Evans 1998 pp.256-259) header “HIPACHUS [*sic*] AND PTOLEMY ON PRECESSION”] & kept pretending Hipparchos’ Spica data showed **Greek observational unreliability**. [Irony. **Evans has debased himself FOR NOTHING**: these random goofs *can’t alibi* Ptolemy’s **systematic** fit-to-theory errors.] All while *non-citing* Hipparchos’ usual accuracy or Rawlins 1991W fn 288! No-better-demonstration-could-be-wished [1] of the *JHA* Assoc Ed [now Editor!]'s citational integrity (*DIO* 8 1998 p.2) & [2] of cultist disconnect between evidence & conclusion: *when the former collapses, the latter stands entirely undiminished*, an invincibility also durably evident throughout the revealingly flip-flop history (Rawlins 1992V §§C31-C33) of the Ancient Star Catalog controversy. Today, Evans continues (Rawlins 2009E §A2; *DIO* 9.1 1999 p.2) decades of evading Thurston’s & Rawlins’ questions on the matter. (In 1997 June, DR asked Evans face-to-faces. Evasion. DR then asked Evans for his office phone number so the two could confer. Evans refused. And *JHA*’s determination to shun permits Evans to face no consequences for such stealth. Other than 2013 appointment to *JHA* Editorship.) Like Gingerich (§A; fn 5), Evans cannot ever be shown wrong by non-club outlanders. [On the of-course-Disappeared “notes from that [eclipse] evening”: ¶4 §B6.]

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

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

Those archons who for generations have controlled journals, conferences, and funding in the history-of-ancient-astronomy field assume to themselves the god-like prerogative to classify — with that infallible judiciousness so amply sampled hereabouts — and exclusively publish, those who are equally reliable, trustworthy experts, as distinguished from those crazies who should be blackballed, exiled, unpublished, uncited. However, our question to deities isn’t: have they ever had *the faintest idea* of how an ancient astronomer actually worked, observed, reasoned, and achieved? (Or: have they ever sufficiently acquired a scientist’s attitude, for appropriate empathy with the scientists of yore?) No, the germane question is: how have so many of the field’s leaders so often concluded-for and tyrannically insisted-upon *the very opposite of the truth* — and on the most central issues — virtually across the board. A gang of moguls *with such a degree and breadth of misperception* of their own field’s realities may be unprecedented. If they are deliberately trying to acquire a reputation as the sore-dumb retards and fanatics of the history-of-science discipline, their plan could hardly be improved upon. (Which actually would be quite unjust, in that such men as Neugebauer, Aaboe, Britton, and others are brilliant despite their blindspots.) Think this too strong? Well, if you have the independence to actually read what follows in this section, you’ll be *taking evidence before making up your mind*. (We trust this doesn’t break a cult principle.) The specific delusions that have long been orthodoxy in this unreal field:

N1 Asserting that Ptolemy got his astronomical elements from his “observations” — the central JHAD-inversion of this controversy. Contra: [1] A particularly penetrating yet simple demonstration of the truth is due to Thurston 1994P (§D), who noticed back in the 1940s that Ptolemy’s iterative proofs of planetary orbital eccentricities start with highly precise estimates, but by the final iteration they’re round as can be. Real iterations proceed in the reverse direction. [2] Another instance of JHAD inverse-perception of ancestry-direction is shown below at §N15 item [F]. [3] Mars’ *Alm* mean synodic motion is accurate to ordmag 1’/century, yet the “observations” on which Ptolemy’s proof of it is purportedly based are off by ordmag 1° (Rawlins 1987 p.237). [4] The *Alm* proof of Mercury’s mean synodic motion was already shown above (§F2) to be pretense, by one who had started with the answer and (if the *Canobic Inscription*’s elements were also based on alleged observations then he) TWICE — *differently* — fabricated the “observations” to prove it. [5] The centrally phony aspect of the whole *Alm* is seldom understood (P.Huber the happy exception — Rawlins 1991W fn 224), namely, its near-exclusive use of *n* observations to solve geometrically for *n* orbital elements. From the often excellent results sampled here throughout, we realize that ancients’ actual historical evolution towards accurate elements obviously involved repeated adjustments from numerous observations. As a mathematician not a scientist, Ptolemy never sensed the value of overdetermination — where the number of equations of condition exceed the number of unknowns sought.

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

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

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

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

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

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

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

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

N8 *It will be convenient to here list in one place prominent instances, of the pile-on passion of the most elite historians-of-science, for trashing ancient empirical science:* Neugebauer 1975 pp.108, 284, 642-643. Gingerich 1976 p.477. ScAm 1979. Van Helden 1985 pp.6-7 & 168 n.8. Evans 1987. Evans 1992 p.68. Evans 1998 pp.273-274 & n.32. Duke 2008W p.287. Shcheglov 2016 pp.687&693. (Further discussion at §M3 & ‡3 fn 8.) Another interlude: Ptolemy’s *Tetr 1.1* astrology-promo suggests he’s fighting more resistance than historians-of-science know, “most events of a general nature draw their causes from the enveloping heavens. But . . . everything that is hard to attain is easily assailed by the generality of men . . . there are specious” criticisms of astrology, but doubts of astronomy “could be made only by the blind”. JHAD’s best anti-fraud defense of its hero might be to plead him “Illegally Blind” §D. See also §E4; & §E6 on his night-shy eyeballs.

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

⁴⁸ Irony [due to yearlength-estimators’ dependence on truncated (Rawlins 2018U ‡2 §C1) prior solstices]: despite 4 known reliable solstices (Rawlins 2018U Table 3), no known ancient got an accurate yearlength. In 1977, Brigham Young Univ astronomer H.Kimball Hansen conceived a simple method ancients could’ve used: [1] Find a stable stone point on a hill which near an equinox casts a North-South shadow at apparent noon on a stable stone surface below, both stone locations being more secularly immobile than human equipment. [2] On some date around an equinox (no need to be just at one, merely when solar declination-motion is near-maximal), mark where the noon shadow is. [3] Note when it returns there 20 years later, and divide the interval by 20. The result, in just 20^s, will be several times more accurate than any yearlength known to have been adopted in antiquity (even though these were based on intervals an ordmag longer) all of which were seriously erroneous, for reasons (analysed at *ibid* §§C-D&Q) which do not apply to the remote Babylonian data which ultimately&fortunately made possible the hyper-accurate Greek lunar periods of Rawlins 2017E §§B2-B4.

N10 Historians-of-science unexceptionally ignore the perfectly Occamite 3-for-3⁴⁹ hit-success of the spare atmospheric-refraction theory that explains and fits both ancient Earth-

⁴⁹ Only 2 ancient Earth-circumference C values were widely adopted: Eratosthenes' (really Sosstratos': Rawlins 2008Q) 256,000 stades, and Poseidonios' 180,000 stades later. Ptolemy adopted each in succession, though they exhibit a previously unexplained gross disparity, the former being over 40% larger than the latter. *DIO's* revolutionary simultaneous solution of BOTH these C values from the same simple theory (math & sources at *ibid* eq.28) realizes that each Greek C differs from actual C (216,000 stades) by almost exactly a factor of 6/5 (within c.1% in each case), Eratosthenes' high by 6/5, Poseidonios' low by 5/6. Hmmm. It happens that there are 2 very obvious stay-at-home Earth-measure methods (one even semi-attested at Pliny 2.65.164): [1] Pharos-flame-visibility & [2] double-sunset (Rawlins 2008Q §A4). Resolution arises since atmospheric refraction causes horizontal light rays' curvature to be 1/6 Earth's, thus the lighthouse-flame method's result is expanded by factor 6/5, while the double-sunset method's result is contracted by factor 5/6. However, to see this, one must be able to follow the mathematical physics. There is as yet no evidence that any historians-of-science have ever done so, during the 1/3 of a century (summary and citations going back to 1982 provided at Rawlins 1996C fn 47) since *DIO* published this hyper-neat triple solution. (Are they even aware that navigators' familiar formula for the horizon's "dip" has been based upon identical 6/5-mathematics for over a century?!) These 2 *DIO* matches exceptionally are achieved without fudging the stade at all, but just by adopting the standard 185 meter value. [Thus serving as the final, controversy-ending proof that 185m was indeed the true length of high antiquity's stade.] Our achievement here may be compared to the controversy's endlessly wheel-spinning metrological-solution literature: sniffing&sifting through ancient lore in search of hints of oddball stades, which of course existed all over the place before the Ptolemies presumably regularized the measure by defining their empire's royal stade at 185 meters, commonly miscalled "Attic", which we may instead (below) dub the "sexagesimal" stade. Unique in the centuries-long history of the debate, *DIO's* solution is physical not metrological. Again (§N10), note that this resolution matches all 3 targeted data within about one percent: [a] Eratosthenes' C , [b] Poseidonios' C , & [c] the 185 meter stade that is now (Rawlins 2008Q §J1) accepted by virtually all serious scholars. By contrast, ALL the usual solutions for C (invariably just metrological reshapes), that keep filling journals' pages, can only match 1 out of the 3 (and even that match is usually several times looser than 1%). Notice the astronomer-deflating surprise that the Pharos solution of the famous Sosstratos-Eratosthenes C is geographical, not astronomical. [Speculation follows.] But refutation is effected when we realize that the unclever (but low-refraction) presumably-Kleomedean-astronomical-survey-based 185 meter stade implies $C = 216000$ stades (since the product equals actual $C = 40$ million meters) but 216000 is the cube of 60, hinting that (before Sosstratos cleverly but wrongly found for $C = 256000$ stades, c.270 BC) scientific surveyors had obtained an accurate C to which Greek science had naturally applied standard Greek sexagesimal division to the Earth's meridians (the process' step 1 is even attested: ‡3 fn 111) to define the stade so that $C \equiv 60^3$ or 216000 stades. I.e., sexagesimalization (triple division by 60) of Earth- C 40 million meters produces a geodetically correct "sexagesimal" stade of 185 meters, fine for 600 stades/degree, but not for Sosstratos-Eratosthenes' later-famous (but seriously-too-high) 700 stades (of 185m each) per degree. But: when did this hypothetical achievement occur? Traditional games at the Olympic stadium began before 300 BC, but when were the stade-long-footrace markers now found there (Engels 1985 p.298) established? (If athletes ran in various states' races, there must have been gradual standardization.) Dinsmoor 1950 pp.250-251 presents for 5 ancient stadiums the disparate lengths of the traditional 1-stade footrace, when the sole 185m one is also the only one (Athens, reconstructed +143; Gibbon *Decline* . . . Chap.2 [Mod.Lib.ed 1:41]) that's post-Ptolemy I. In the Hellenistic world, where else than Egypt would terrain allow a long flat North-South arc, of ordmag 1000 km. Given that the Alexandria-Meroë arc's curvature is apt to a meridian circle of $C = 39870000$ m, a Strabo-attested 10000 stades interval (at 700 st/degree) signifies ancient knowledge (within 1 part in 100s), that the cities' latitude-difference is 14° 1/4, thus able reliably to indicate Earth- C so accurately that hypothetical surveyors' 185m conclusion was trustworthy to within ± 1 m. Was Kleomedes' famous Alexandria-Aswan legend a myth (DR's former opinion), or a remnant of Ptolemy I's post-conquest land-survey of his empire? (With similar possessiveness, less scientific William the Conqueror reckoned his own new booty in the Domesday Book.) This would be technically possible using theodolites (transit instruments) we know existed (consistent with the superior technology that effected the conquest of Babylon, which lacked such advances as theodolites and trigonometry tables), given Timocharis' accurate theodolite-based stellar declinations c. -300 (*Almajest* 7.3; Rawlins 1994L). Was Timocharis chief of the project? Hitherto un-noted credit: Timocharis knew Alexandria's 31° 12' latitude precisely (*ibid* §F6), while neither of the other two later star-observing Alexandrian astronomers quite did so (*ibid* §§F7&F9). From solar observations (e.g.,

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

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

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

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

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

N13 Selling crude priestly Babylonian indoor astrology as “impressively accurate” (Jones 1991H p.118), and so brilliant it inspired Greek astronomy, according to Neugebauer [1975 p.622 believes in “the advanced state of astronomical techniques” in Babylon] and others of his persuasion (Rawlins 1991W fn 73; Rawlins 1996C fn 128) — even though the sole empirical datum traced in either temporal direction is Greek→Babylonian (Dicks 1994 fn 37). The greater antiquity of raw records from Babylon may say no more than that clay outlasts papyrus. Unlike for Greek astronomy, no record exists of how Babylon arrived at its naked celestial tables (Dicks *op cit* §C4; *DIO* 13.1 ¶2 §H.) Clue: virtually all useful astronomical texts from Babylon *post-date its conquest by Greece*, and its periodic functions are not trigonometric ones (Greek astronomers had trigonometry from no later than c.130 BC) but approximations thereto, by zigzag or even step functions, suggesting (¶3 fn 120 below), though not necessarily proving, mere derivative degeneration from its conquerors' superior technology. (See also Rawlins 2018U §J4.) An obvious & devastating point, never previously emphasized for its implications: *not a single trig table survives in any form from Seleukid-era Babylon*. Babylon made no solstice or equinox observations (Neugebauer 1975 p.366), or meridian or vertical observations of any kind, because (Rawlins 1991W §E3) Babylon had no transit instruments (vs Greek celestial transit data from c.300 BC onward, at Alexandria and Marseilles), and accordingly didn't know or care what the city's geographical latitude *L* was. Which may explain why the only attested figure for it, 35°, [a] is found strictly in Greek records, not a word on *L* anywhere in cuneiform material, *another crushing blow to Babylonianist pretensions*, and [b] is too far north by 2°28'! — 148 nmi. Finally, while Greek planetary order was physical — Mer-Ven-Mar-Jup-Sat — Babylon's was astrological, Beneficent→Maleficent: Jup-Ven-Mer-Sat-Mar.

N14 Failure to learn anything from the first of *DIO*'s eclipse-cycle solutions (¶3 §I33), in which equating 9660 synodic months with 781 sidereal years (the interval between two *attested* local-midnight lunar eclipse records, Babylon —719/3/8-9 & Alexandria [Heron] 62/3/13-14) neatly recovers all 10 digits of Ptolemy's previously mysterious final luni-solar equation (Rawlins 1996C eqs.21-31): 8523 tropical years = 105416 synodic months. (Had JHADists not shunned this remarkable match, they might well have anticipated the vaster discoveries of §N16, long before *DIO*. Similarly at Rawlins 2009E fn 7.) This is also the 1st irrefutable evidence for ancient use of sidereal→tropical transformation of period-relations (Rawlins 1996C eqs.26-27), a process later extensively employed in *DIO* 11.2, which allows (here, in fn 8 [4]) reconstruction of Venus' accurate pre-blunder synodic motion.

N15 Among the most egregious of all inversions of ancient astronomical procedure: our uniformly on-the-nose huge-cycle-solutions of no less than 4 long-mysterious periodic lunar mysteries (§§N14 and §§N16-N17) fall upon locked-up minds, disbelieved without a glance by jeering JHADists who (frustrated by inability to find error in heresy's math but determined [§B3] to reject the obviously probable in favor of the preconception-accordant improbable) resort to whatever dodge will serve. Besides jeerleader Jones' private carelessly unchecked & glaringly one-sided misfire (§N16), there's a long-popular theory (item [E]: put into writing privately by P.Huber, likely inspired by Neugebauer [also primarily a mathematician, like Ptolemy]) that is so inverse-contrary to sensible scientific practice as to gain special popularity among gaping cult-minds, thirsty for any refutation of undeserving outsiders' proposals, thus not just suggesting but insisting-upon JHADists' pure speculation that very long period relations *must* have originated from splicing together a few much shorter relations, & no other hypotheses need apply. (The truth was 180°-opposite from this rigid position, of course: ancients' more-convenient short relations descended from less-handy ultra-long empirical ones [*DIO* 11.2], for reasons about to be explored, below.) Which demonstrates yet again our JHAD's unerring attraction to the erring. Considerations:

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

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

is attested at all; [2] is the sole ancient method even nearly capable of such hyper-accuracy; [3] automatically spits out ratios; and, [4] inducing the three solutions without manipulating a single digit, one finds ratios that are *exact* matches for all six 4-digit factors (24 digits!) in the previously-unexplained ancient records. During the 1 1/2 decades since publication of this unexpected and fruitful discovery, no scoffer has found in the analyses [a] any math error, nor [b] any alternate eclipses also producing the data we have solved-for (which would show our solutions' non-uniqueness), nor [c] any below-horizon eclipses used in the *DIO* analyses. Nor [d] the courage to cite or debate this theory in print: fn 6. **NB:** Whereas the selected pairs' back-end eclipses are spread across 4 classical-era centuries, the front-end eclipses are **4 times more tightly grouped**, in a single productive century: the 13th BC.

N17 It is revealing that the JHAD clique has not only failed for decades to understand the central (§M3) significance of the *Almajest's* three lunar periods' high accuracy — but to then let shunning dictate refusal to cite the 1st solution (§N16) anyone has yet achieved, for **how** these periods were obtained?! That's non-citation with an *impressively* unanimous lockstep. . . . (Is there a prize we don't know about, for this special brand of perfection?) To summarize, cultists keep secret from *JHA* readers and the larger public BOTH: [1] the historically revolutionary empirical significance (§M3) of the inescapable fact that all three ancient lunar-speeds are micro-accurate, **and** [2] the eclipse-cycle source (§N16) of such accuracy. (Let *Animal House's* Dean Wormer top THAT Double Secrecy.) And don't miss the saddest&sadist-masochist part: all disbelieving archons are religious Babylonianists, but their coherent disdain for target-heretics trumps even worship of Babylon, as their rabbotic shunning requires every single cringing cultist to forgo reveling in the *mathematical recovery of the greatest heritage from Babylonian astronomy*, not to mention **the earliest major scientific achievement by any civilization**: observation and *preservation for a millennium* of precious eclipse-records, now-lost, but invaluable to classical-era scientists, who, without the huge *N* (§N15) which Babylon's data repeatedly made possible, could never have found those above-cited astonishingly accurate lunar speeds, that are among the prize glories and proofs of empirical Greek astronomy. Rejectors of *DIO's* 3 big-cycle solutions have the advantage that all 3 proposed front-end eclipse records are long gone. But: [a] The early front-end eclipse for the parallel 9660^u = 781^y case does survive (§N14 & ‡3 §I33). [b] Is it JHAD-banned to use intelligence to induce Greek astronomers' possession of the §N16 front-end eclipse-records? — just as *DIO* induced (§N19) Hipparchos' -157 solstice & adoption of Kallippic motion, both of which (after same JHADists' scoffs) turned out (§N19) to be anciently attested. In this field, what is intelligence for, if not to revive lost antiquities? Were the earlier data extant, there'd be no inductive mysteries here. Who desires a field with no challenges, no advances beyond texts?

N18 Rigid, total, as-usual-unanimous, high-odds-oblivious rejection of 5 new data-match-based proposals central to the field: [1] Computing from his saros-based 4868^y Great Year, Aristarchos originated the "Babylonian" month, 29^d191°00'50", decades before Babylon (‡3 §G4; or Rawlins 2002A eqs.4-8). [2] DR's 2001/6/27 British Museum lecture showed (*ibid*: eqs.9-11; or ‡3 §G5) Aristarchos applied to this the Metonic cycle (235 months ≡ 19^y), resulting in his Metonic "tropical" year $Y_{As} = 365^d1/4 - 15/4868$ (fatefully off by 6^m, virtually same as Metonic cycle itself). [3] Years before item [2] was discovered, the matching recovery of exactly this yearlength was discerned in Vatican-held ms data listed under Aristarchos' name on Vat. gr. 191 fol. 170^v (data at Neugebauer *op cit* p.601), $\tau \xi \varepsilon \delta' \kappa' \xi \beta'$ or 365 4' 20' 60 2'; if expressed as the continued-fraction $365^d1/[4 + 1/(20 + 2/60)]$ (Rawlins, *op cit*: eqs.12-13), this is $Y_{At} = 365^d1/4 - 15/4868$, verifying above item [2]. All without altering any Vatican document number and much aided by Neugebauer 1975 p.602's perceptive interpretation of ξ as sixtieths. [4] The sidereal-year companion data, Aristarchos' $\tau \xi \varepsilon \delta' \iota' \delta'$ or 365 4' 10' 4' from Vat. gr. 381 fol. 163^v (Neugebauer 1975 p.601), we write as continued-fraction $365^d + 1/[4 - 1/(10 - 1/4)]$ (‡3 §G2; Rawlins, *op cit* fnn 14-15), yielding sidereal year $Y_{As} = 365^d1/4 + 1/152$ (good within a few time-seconds), again altering no Vatican-ms digit; and, again, a hit: the

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

N19 Resisting unwelcome results from newly-translated papyrus *P.Fouad* 267A: [1] The papyrus' vindication of the previously unknown Rawlins 1991W (§§K8-K9&M4) discoveries that Hipparchos sought a -157 S.Solstice and that he (twice: *idem*) used Kallippos' 365^d1/4-year solar motion. Both his -157 solstice (a different day: Rawlins 2018U §§L-M) & use of Kallippic motion are explicitly provided on the 1900^y old papyrus. (Kowal & Van Brummelen have enjoyed similarly unexpected years-later vindications, which the reader should look up, to share the felicity.) [2] Simultaneous solution (*ibid* §§K4-K5 & eq.25) of *P.Fouad* 267A's decade-old double-mystery: [a] Why did it specify daytime for the -157 solstice, though Duke's accurate calculation from its data got night-time? [b] What exactly caused the odd remainder of its tropical year, $365^d1/4 - 1^d/309$? The field was informed of these discoveries in 2015. Inert reaction: ‡3 fn 66.

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

N21 Carrying §N20's handbooks-as-primary-science transformation to an unsurpassable apogee, as the field's archonship improvidently⁵⁰ for decades banished anyone who objected to selling the clumsiest (§§C and G) faker in astronomical history to academe & the public as **"THE GREATEST ASTRONOMER OF ANTIQUITY."**

Again — we're not supposed to guffaw?

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

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

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⁵²"The Acquittal of Ptolemy." Written by Swerdlow-dazzled Paul Hoffman, unsigned; instigated by Editor Dennis Flanagan who told Rawlins on 1979/2/7 that he didn't like pipsqueaks who tear down giants, adding that Ptolemy might not be a giant, but Robert "Newton is a pipsqueak." Did Flanagan even know that Newton was the scientifically brilliant Space Sciences Supervisor of the Johns Hopkins Applied Physics Lab? Not if he listened to the mathematically-challenged [§N7 and fn 8] trio cited in the piece he published [Swerdlow, Gingerich, & V.Thoren], reflecting the kind of muttered slander (more at fn 35) created and spread behind backs by parties many of whom even today keep believing that if they can just preserve or salvage some sliver of doubt that Ptolemy faked, they are thereby not utterly convicted of the vilest brand of academic misbehavior, in their decades of gang-smearing those who were — the ultimate irony — on the side of truth right along. Which merely adds ethical incomprehension to scientific. And why would a party have ever in the 1st place resorted to slander & shunning & running instead of inviting debate, *IF* (§M1) it genuinely believed evidence & competence backed its position? Why did peace never break out? *DIO* 16 p.2 fn 1 (2009), emphasis in original: "Rational, pacific discourse shows who's right & numerate, so: **why would archons tolerate peace?**"