

‡4 Ptolemy Enormity

Ptolemy-Defense Cult Lays Yet ANOTHER Egg On Own Already-Unwipeably-Eggregious Faces ArchonBishop of TruthBury's Trowel & Slander

We now analyse the latest installment, this time from the *Journal of Astronomical History & Heritage* 2014, in a half-century serial display of unfailingly invalid archonal apology-defenses of Claudius Ptolemy, sacrosanct mascot-astrologer of the American Astronomical Society and its HAD: author of astrology's bible, the *Tetrabiblos*; science-fabricator; & *Almajest*-perpetrator. The spectacular result has been one of history's grandest compilations of establishment pseudoscience literature, all accomplished in the service of attempting to save the reputation of a "scientist" icon who was not a scientist at all, but (§A below) just a mathematician who faked science. Badly. Among Ptolemy's numerous clumsy AlmaJests [the silliest on scintillating display here in ‡2] were, e.g., [a] 2 different dates (37 days apart!) for the same Venus maximum-elongation, [b] plagiarizing Hipparchos' star catalog, [c] impossible-for-regular-observer ignorance of his own city's latitude, [d] 4 alleged solar "observations" which were (as no historian-of-science denies; or admits) many times nearer Hipparchos' old indoor tables than to the outdoor sky's actual Sun.

The *JAHH* paper in question, J.Brandt, P.Zimmer, & P.Jones (below known as Brandt *et al* 2014B), attempted mathematical analyses of the *Almajest*'s stellar declinations, observed by four successive ancient Greek astronomers over nearly half a millennium. The journal and authors contend that the dozen *Almajest*-contemporary declinations could be Ptolemy's observations, never warning the reader that zero evidence is provided to establish that claim, while simple, definitive, long-published, referee-urged proof to the contrary is below shown (§C5) to have been deliberately omitted. The paper's "bivariate least-squares" statistical analyses were not bivariate and thus didn't exactly find any least-sums S_0 of residual-squares, as is also demonstrated below (§C23). While observers' epochs E are nearly right (but not new), attempts to find their geographical latitude-errors x are revealed as grossly misguided, at a primitive level (§§C9&C12), though referee DR provided, ahead of publication, accurate x (& standard deviations) for all four of the ancient astronomers being analysed, solutions which could've been (but weren't) crudely verified by elementary arithmetic, as will be shown here (fn 34 or §C23). Our discussion's bluntness derives from the fact that, though Brandt *et al* 2014B is politely written, its knowing evidential omissions cooperate in trying to grant eternal life to an establishment myth — Ptolemy as Great Outdoor Astronomer — that rolls on, decade after decade, persisting only because the American Astronomical Society doesn't care that its Historical Astronomy Division is deeply invested in a pathetically obvious historical lie, viciously (fn 4) defended by those JHAD archons who long ago mistakenly decreed Ptolemy "The Greatest Astronomer of Antiquity"¹ and thus have faces so at risk of megga-eggitudinal disgrace that they must forever encourage pseudo-science-for-The-Cause of forever-pseudocontroversy, cult-obediently incapable of admitting that any skeptic has ever made an indubitable contribution to knowledge. Below, at §B, the most recent misfire (Brandt *et al* 2014B) is put into the context of decades of like uniformly baseless mobaganda (though those interested only in 2014's mismath may skip straight to §C), which has by now so brain-dirtied the mass of non-specialist historians that writing in opposition may be little more than preaching to the perverted.

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

Text for the Day:

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

You are protected by the enormity of your stupidity.

The point might be kept in mind by observers of the decades-ongoing spectacle of the history-of-ancient-astronomy field, whose most eminent journal and most prominent society — in tandem with a MacArthur Fellow and a Harvard prof serving as untruthbound propagandists — spread behind backs (fn 18) the slander that no-one but a **CRAZY**² person could suspect dishonesty of the history-of-ancient-astronomy field's ultimate hero, ancient astrologer Claudius Ptolemy, who 4-times-out-of-4 reported observations of the Sun that were undeniably but captive-journal-unprintably 50 times closer³ to Hipparchos' 280^y-old indoor tables than to the real Sun. The prime forums perpetrating this fantastic but profitable joke on academe and the public, **for consecutive decades**, are the *Journal for the History of Astronomy* and the American Astronomical Society's knowingly⁴ unsupervised Historical Astronomy Division (HAD) — which we shall refer to as the "JHAD" combine. Seemingly incredible fact of the last 4 decades of the Ptolemy Controversy: **not a single published defense of Ptolemy has ever been valid** and most have not been particularly smart⁵ or honest,⁶ as we are about to see again&again below. But as with oft-crafty Rains, the perpetrators of this fantasy-literature are protected by the very incredibility of the idea that such ultra-eminent forums and scholars could seem so stupid. (Also invisibly backstabbing, slanderous, & deceitful: fn 18.) The gulf between the pompous mask and the dumb arguments that are insisted-upon (by people some of whom are normally as smart as Rains) is so beyond the comprehension — the very universe — of emotionally normal onlookers & pressfolk, that the latter have not, cannot, will not believe the solid reality of what has been the dispute's history, even though oft meticulously documented in *DIO*.

² Prime smear against dissent is Insanity (as with media on pols' heresy) by megafunded establishment-polishers & darlings Gingerich (fn 16) & MacGenius Swerdlow (fn 4&18, ‡2 fn 35), scientifically-challenged (‡2 fn 8) MacG even mirrorlessly calling JHU-physicist & JHAD-satan Robert Newton a Velikovskian "crank and a con-man": www.dioi.org/j113.pdf, *DIO* 1.1 ‡3 §§D2-D3.

³ Nobody disputes the 50-to-1 indictment. But no Reputable Forum (including even popmags, newsrags, & toob) dares broadcast such heresy-supportive truth, either. The rigid decade-after-decade policy: *hide it from the public*. (Given the power-secretarial state of the nation's Free snicker Press, that's not even a challenge. Consider: **would archons behave as described here if they thought there was even a 1% chance the press would expose it?**) E.g., in 1983, the *Journal for the History of Astronomy* so insisted on (at-the-last-minute, without-warning) deleting the 50-to-1 evidential crusher from a projected DR article, that the paper was suppressed by *JHA*, being finally published by DR 16^y later: original unexpurgated text at www.dioi.org/j913.pdf, Rawlins 1999 §E. Understand the attitude: you the public just can't be trusted with certain central facts, because you might "misinterpret" them and start believing something Unapproved. (Similarly at www.dioi.org/vols/wi0.pdf, *DIO* 18 §§T13-T16.)

⁴ DR has asked AAS to monitor HAD's "unprofessional" (Schaefer 2002 p.40) behavior: 2002/10/2, 2015/12/29, & (www.dioi.org/jcx6q.pdf, email) 2017/6/26 (no reply) transmitting photographic proof of dishonest archonal smearing: www.dioi.org/pm1.htm, vs www.dioi.org/pm2.htm, tactics long known to *DIO* recipients, a class which includes the AAS, whose chief in 2017 joined the deaf&dumbers.

⁵ Some authors may be able, but this breed of apology never quite is. Evidentially countering such feeble and comically self-contradictory (Rawlins 1992V §§C31-C32) effusions is not a serious challenge ("like shooting fishstories in a barrel of monkeys": www.dioi.org/j13a.pdf, *DIO* 1.3 ‡10). And that is exactly why Ptolemyists eschew (‡2 fn 52) risking rational debate with DR, written (www.dioi.org/deb.htm) or spoken (‡3 fn 5), preferring character-assassination-stealth's bravery: fn 4.

⁶ Ptolemyists' integrity-level (e.g., §B6 below) generally shows up less in the (perhaps-unintentionally) deficient original paper than in subsequent failure to acknowledge its thesis-gutting flaws.

A The Shy Archon Triggering the Present Paper: Politics vs Science

A1 In 2011, DR belatedly⁷ responded to much-decorated astronomer Jack Brandt's welcome request to consult a 1982 unpublished DR ms on the 54 star declinations observed by ancient astronomers Timocharis, Aristyllos, Hipparchos, and (allegedly) Ptolemy — reported and analysed at *Almajest* 7.3. In 2014, much-too-quickly before the resulting paper Brandt *et al* 2014B went to press the *Journal of Astronomical History & Heritage's* Editor Wayne Orchiston asked DR to referee it, though WO didn't mention that its progress was already so far along towards publication that serious changes appear in retrospect not to have been feasible at the late date of *JAHH's* request. (Not the 1st time [e.g., Rawlins 2008S fn 42] Ptolemyists have asked skeptics to help them avoid blunders, even while undeterably determined to promote more cultism.) And, indeed, no changes were made, in response to central points challenged by DR's scientifically detailed 2014/8/26 referee report, www.dioi.org/jau8q.pdf, though an irregular sprinkling of (non-space-expanding) alterations was effected. The timing suggests that the paper's case for Ptolemy as outdoor observer was not going to be derailed by mere evidence, much less a full discussion of issues. Some other referees might care enough to regard such treatment as insulting — which would only divert from the main point: it's counter to a journal's obligation (and own best interests) not to take all pains to provide the most accurate and competent articles possible. (Not exactly an infectious ideal at brother history-of-astronomy journals, either.) The irony here (as is obvious from correspondence: fn 28): DR went to plenty of trouble in a cooperative, generous attempt to help *JAHH* be a more accurate and competent journal. It was disappointing to find that such considerations rank nowhere at the *Journal of Astronomical History & Heritage*, probably (despite *JAHH's* pathetic ultimate cultishness and non-bravery) less from iniquity than from *JHAish* inability (increasingly typical of the whole ever-less-scientifically-skilled⁸ history-of-astronomy field) even to begin to tell balanced, competent technical research from cultist apologia.

A2 Brandt *et al* 2014B p.332 claim that the 2nd century AD star-data of *Almajest* 7.3 “could have been taken by Ptolemy himself.” The evidence for this politically-convenient falsehood? Ptolemy was alive when they were recorded! — a fact which did not require a new article for broaching, since it's been published for decades (at least) and has never been in dispute. No other evidence is brought forth favoring the claim, because there isn't any supportive data whatever — all relevant evidences on the point are to the contrary (§C5 below). These were imparted to *JAHH* but never entered into its paper, which instead took seriously Ptolemy as observer, and promoted a fact-immune⁹ Ptolemy-alibiing pure-careerist like Brandt's Puget Sound neighbor J.Evans as quotable Neutral Expert. (Brandt *et al* 2014B p.333: “The situation has been nicely summarized by [Evans 1998 p.262].”) So DR responsibly submitted a paper, “Ptolemy's Fraudulence” (‡2 above), to the *JAHH*, whose chief, W.Orchiston (formerly established in Oztrollia, like *JAHH*, but lately transplanted to Thailand) turned it over not to a specialist in the relevant science

⁷ When asked to send Brandt his 3-decade-old star-declinations ms (later slimmed, revised, augmented with new discoveries of absolute latitudes, and published as Rawlins 1994L), DR took the time to profitably review his 1982-1994 conclusions, sending his further-revised 2011 thoughts in a letter, www.dioi.org/bjr3g.pdf, accompanied by the requested 1982 ms. Brandt certainly deserves credit for updating star-data (fn 40) and for stimulating DR's 2011 discovery (§C21), which everyone including DR had missed right along (even though Rawlins 1994L had already concluded that +159 was the Clean Dozen's epoch): for epoch +159 (unlike for +137) *the split between Clean Dozen & SickSix stars was overlaplessly clean*: §C17. (But Brandt *et al* 2014B didn't cite any of this.)

⁸ As we mourn the passing of technically able contributors to scientific history such as B.L.van der Waerden, C.Gillispie, W.Hartner, O.Neugebauer, C.Wilson, H.Thurston, A.Aaboe, R.Newton, & S.Goldstein, we realize that they are being replaced (as JHU's Harry Woolf warned DR 50^y ago) largely by non-scientists. The new breed has proven admirably industrious, but too-often inadequately trained in science's skills, criteria, standards, principles, and especially approach to evidence.

⁹ See below at, e.g., §B4.

but to a fellow politician, who despite “careful” reading could come up with no errors of science or history — or anything else — and thus (*in stark contrast to DR's ref report*), offered no scientific guidance at all (unless one delusionally regards shrinkoanalysis¹⁰ as science), instead — even while acknowledging that DR is “clearly quite knowledgeable in the astronomical history involved” — insisting on removal of anything embarrassing to his clique, adding gratuitous psychological evaluations¹¹ including accusing DR of an “apparent need to disparage those with different views”. This from a cult which has for a half-century repeatedly (and reliably-always behind-the-back) smeared, as insane, anyone differing from its own reality-detached view of Ptolemy — a genuine, mentally-disabling insanity which The Leader is now at the last almost¹² alone-in-the-bunker with, outside of (publicly) loyal J.Evans, J.Brandt, & possibly B.Schaefer. (Ptolemy's many well-known doubters — their consensus not at all well-known — are extensively listed here at ‡2 fn 1, though *JAHH's* guardian [“referee”] is still stuck dreaming-on of a 1/2 century ago, in calling skepticism an extreme position: “worth hearing” he pseudo-tolerantly offers, even while continuing its suppression for a 4th straight decade at his *JHA*.) The ref added a death sentence to the paper, telling an editor who obviously wishes to stay on the good side of History-of-science's Archbishop of TruthBury: “If this were my journal, I would not like to see this paper in it.” When *JAHH* supinely granted him full veto power over the paper, it had been determined that it was not going to appear in any form in *JAHH*. But censoring editors (& refs) are ever pretending not to be, so *JAHH's* initial tentative approach to exploring for an excuse for nonpublication was to find out if the durable myth, that DR would not accept¹³ editorial revisions, would suffice to dodge publishing archon-loathed heresy.

A3 DR's reply, www.dioi.org/owu8q.pdf, tried [A] to test whether demanding the paper's softening was in hopes of making DR go away; and [B] to check out *JAHH's* biassed chumminess with its mentor (who has loathed and libelled DR for decades). So [A] DR unexpectedly refused to enter into any argument over content, granting full veto power to *JAHH*, instead of its 1st referee (as if there were a difference, as we learned). [B] The paper was expanded to provide information about the referee's claue — vainly asking, www.dioi.org/oww2u, *JAHH* to point out DR errors — which would have caused a neutral journal to choose a different referee. The *JAHH's* brave reaction to this disappointment? Just run away. *JAHH* went silent, even blocking *DIO's* email address. Which is why *DIO* is distributing the present *DIO* issue, with the offending paper right here at ‡2. Nothing new about this: it's just copying the equally scientific, receptive, & ethical 1983

¹⁰ Gingerich's private ref-reports on DR's work can't resist personal remarks having no place in such. (Too remote from principled stands even to recognize one, OG actually claims [*DIO* 2.1 ‡3 §§C8&C12] DR *wants* to be shunned.) DR's atheism heaps extra aggravation upon this self-described “practicing Christian” of the Mennonite cult. Students of the psychology of hate might profitably investigate this 40^y obsession: bizarre details (& Cardinal Manning's perceptiveness) at ‡2 fn 5.

¹¹ As Gingerich again&again for decades has invariably done with DR papers (fn 10), *in referee reports for allegedly scientific journals*. Yet archons keep right on seeking his Special Insights — on DR's character instead of his astronomy.

¹² One could add Swerdlow to the listlet of diehards, but not from admiration of Gingerich: fn 22.

¹³ In retrospect, it was predictable that *Journal of Astronomical History & Heritage* wouldn't publish a paper showing its recent 2014 BZJ article was false in claiming Ptolemy's star-observership. Editor Orchiston's demand for revision looked like the start of an endless, wasteful game of never finding DR's requested self-censorship sufficiently adequate. (The theory that the paper was doomed from the outset is verified by test in the next-last paragraph of www.dioi.org/oww31.pdf, unsurprisingly.) Since a durable cult lie (Hoskin to Thurston 1986/9/5) is that DR is “impossible to deal with” (contra DR cooperation with, e.g., *Polar Record* [Univ Cambridge] & *Griffith Observer*), it was presumably believed that this approach could kill the paper while never appearing to censor anything. Note that if the editor objected to parts or words (*he, not DR, knows his tastes in this regard*: ‡3 fn 100), he need only have taken up DR's 2015/9/30 suggestion, at www.dioi.org/jav9u.pdf, to strike such (isn't this what competent fields' editors do & are for? — the job would've taken ordmag an hour) & sent the revision back for DR's OK; but, then, what if DR had replied “Done”? Fixers keep their plans flexible.

tantrum of the other non-US journal in this strange field: the *Journal for the History of Astronomy*, whose Editor Michael Hoskin, upon receipt of DR's constructive criticism of *JHA* refereeing of one of its papers, responded with threat and correspondence-cut-off. DR's criticism was supplemented by a polite, *admittedly valid* DR referee report: see the paper's recomputation at *JHA* 1984 June, which happened only because the *scientist* author preferred honest accurate results, and cast seed upon stone by (1983/4/27) recommending DR's refereeing skills to *JHA*. Likewise, when Curtis Wilson and Hugh Thurston insisted (e.g., Thurston 1995) on correcting a flock of *JHA*-unrefereed errors in Jones 1991H. In these and dozens of other instances (www.dioi.org/jha.htm/#hsbk) of serious DR-apprehended *JHA*-errors, that journal has never taken the initiative in correcting the situation, to produce accurate information — as if it really didn't care at all about such trifling considerations. A strict rule at *JHA* (at least vis-à-vis DR): if the erring author is no more honest than the *JHA*, then — no correction is made. (Prototype for *JAHH* 2015 and [†1] *Isis* 2017.)

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

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

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

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

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

Finally, DR emailed a friendly 2016/2/29 letter, www.dioi.org/oww2t.pdf, to WO and learned of his block — an arbitrary act which at last nakedly unveiled a proud new addition to JHAD cowering-archonduum (§A3). This confirmed that the usual heresy-containment info-control (standard for the last half-century: †2 fn 35) was being governed by the priorities of, in this instance, two colluding politicians. A final 2016/3/21 *DIO* letter (successfully sent to WO by alternate email address), www.dioi.org/oww3l.pdf, again tried collegially to allay hypothetical shyness (about editing a DR paper) by pointing out that DR could hardly complain of any WO deletions if he'd asked for them! As DR had, in his 2015/9/30 letter. The new letter ended with an easy test (fn 13) of the theory that the paper had been dead from the outset.

No reply. Which is perfectly consistent with the theory.

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

A6 We will shortly move on to putting *JAHH*'s 2014&2015 sellouts into the context of the disgrace of worshippers' ever-more bullet-hole-ventilated half-century pretense that

Ptolemy observed, playacting which is intended to prevent the public from catching on to the field's long-running thespian obtuseness in failing to admit publicly the ultra-obvious. The present paper started by stepping on an Ozzie egg — which reminded us of that old-time Easter tradition of planting hidden Easter-eggs all over a garden before unleashing the kids. Well, this kid is ever entertained by an Easter-hunt's worth of prior Ptolemy article-eggs all about us (§B), every one a squishable plant, indeed, regularly planted during a half-century of establishment insistence on transforming a clumsy data-faker into not only an honest scientist but a genius,¹⁴ no less — a proposition as believable as a rabbit-laid egg. So we will next turn to enjoying the decades-long history of this desiccated field's transparent pretend-conviction (fn 50) that promotion of planted apology for Ptolemy's observership is any more credible than adducing planted eggs to prove the Easter Bunny is real.

Ptolemy archon: "you mean she isn't?"

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

B1 Does it tell us something about the state of soft academe that the *Journal for the History of Astronomy*, the "premier"¹⁵ journal of its field, has for decades used pseudo-science and deception in the shall-we-just-say extreeeeemely peculiar cause of protecting the reputation of a fellow pseudo-scientist (and grant-cow), the ancient Greek astrologer Claudius Ptolemy? — known for centuries to informed scientists as the most notorious liar in astronomical history. *JHA*'s longtime #2 official, Harvard's Astronomy 101 teacher & deft Disraeliesque trowel-wielder Owen Gingerich, has repeatedly billed¹⁶ this ancient faker and superstition-peddler¹⁷ as "The Greatest Astronomer of Antiquity", claiming that all who question this eminently-sensible-to-him proposition *are the insane*¹⁸ parties to the simmering-if-generally-suppressed dispute inevitably triggered by such superlative saleshype. We will first briefly examine a sample of the succession of careerist soldiers who disgraced themselves by eager enlistment in the *JHA*'s decades-long War-for-Ptolemy, who knew that publishing their genuflections boosted prospects for favor with the field's dissent-burying archonbishops, thereby improving odds for future conference-invites, publication, posts, grants — all with confidence that no matter how outrageously unlikely their arguments, they had nothing to fear from contradiction, in any of their captive journals.

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

¹⁴ AAS-darling Gingerich 1980 p.264. Quoted at www.dioi.org/fj43f.pdf, *DIO* 4.3 †15 fn 43.

¹⁵ Schaefer 2002 p.40.

¹⁶ Gingerich 1976, Gingerich 2002. Disraeli (L.Strachey *Queen Victoria* 8.3, 1921 [Harbrace pbk p.244]): "when you come to royalty you should lay it on with a trowel." Gingerich summed-up succinctly: *DIO* 2.3 †6 §F. To enjoy Rob't Peary's like supplications, see www.dioi.org/cot.htm#dtrw.

¹⁷ Ptolemy authored astrology's bible, the *Tetrabiblos*, the tripe in which has got to be (but usually isn't) read if one wants a measure of how seriously to take Ptolemy as a scientist. See Rawlins 1977 pp.70-71&79 for analysis of the book's escape-hatchery, plus a fundamental Ptolemy gender-miscalculation (*ibid* p.69) which has escaped detection & thus survived for millennia, persisting even in today's horoscopes, e.g., those on sale in our grocery-store-checkout-counter literature-departments.

¹⁸ Psychoanalyst Gingerich's intended-to-be-anonymous 2000 referee report to *Isis* (outed in Rawlins 2003X) called Ptolemy-skeptics just a tiny bunch of paranoids — thereby inadvertently and delusionally smearing most of the scholars in the field, even WHILE he is echoed in the 2015 *JAHH* referee report's complaint that DR *doesn't respect those who disagree with him*. Can it get any weirder? Well, actually, yes. As we see from www.dioi.org/pm2.htm, a secret Gingerich referee-report slander of DR's character (so relevant to the subject paper's mathematical development!) on a basis which Gingerich had forgotten he'd already revealed, at www.dioi.org/pm1.htm, *applied to himself*. (On the reality of cohesive shunning of Ptolemy-skepticism: see †3 fn 6.)

hour. Similar giveaway factors for his three other solar “observations”, all of which agree just as closely with indoor calculation. (Interim question: given this stark&unquestioned circumstance about Ptolemy, think carefully about **what kind of scholar would dedicate himself to defending him, even to the extent of calling all skeptical scientists insane?** The answer has been, for nearly 1/2 a century: virtually anyone who said anything. And this field expects to be taken seriously by scientific scholars? *Seriously?*)

B3 History-of-science’s notion of a MacArthur-Genius, mathematically-challenged Noel Swerdlow, rejected the all-too-obvious explanation for Ptolemy’s rigged 140 AD solstice with two imaginative excuses:

The 1st was misconceived at a juniorhighschool level. The 2nd was a clumsy fantasy:¹⁹

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

[2] Swerdlow’s fantasy for explaining why Ptolemy’s four solar “observations” were (§J2) scores of times nearer Hipparchos’ indoor solar tables than to the outdoor Sun: all ancients selected²² outdoor data to agree with indoor theory. (N.B.: This would naturally justify destruction of the unused data.) Comments: [a] Even if it were true, the proffered alibi wouldn’t explain physically-impossible repeated 1°-off-the-mark Ptolemy “observations” that could never have been made outdoors in the 1st place! Especially again&again&again. (The human eye can see to about two ordmags better: roughly 1’, and the solar semi-diameter is 16’, so his equinox-solstice errors average about 4 times the distance from the Sun’s center to its edge: §B2.) [b] Further, we know that 2nd century BC Greek *scientist* Hipparchos reported real observations which disagreed with his theories and with each other (§3 fn 8); thus, faking or selecting data was not genuine ancient astronomers’ normal procedure. [c] So many accurate Greek astronomical achievements (e.g., lunar distance

¹⁹The deception has become deliberate because [a] the plain Hipparchan counter-evidence (item[2] at §B3) was sent to the journal before publication, and [b] has been known to the perps for all the decades since, causing not the slightest retraction.

²⁰ The incredible reasoning of Swerdlow (MacArthur&PhiBetaKappa!) is examined at R.Newton 1991 fn 20 and Rawlins 2018U §§B2-B3. He and J.Evans continue (in ignorance of both the observing technique and the historical record: details at §3 fn 96) to insist that solstices could not be measured accurately compared to equinoxes, despite several inconvenient facts:

[A] Outdoors Hipparchos’ solstices are about 4 times more accurate than his equinoxes: ‡2 §N7.

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

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

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

²¹For almost 40^y, virtually all journals in receipt of a DR paper on antiquity have not had the imagination to start elsewhere than Gingerich, when seeking refereeing, e.g., *PASP*, *JHA*, *Isis*, *Nature*, *JAHH*. Most, to their credit, later ignored his slander as irrelevant to the content, sought other advice, & published. The most grovelingly slavish — and the least concerned about veracity — were naturally also the least technically qualified (adamantly spurning politically-unacceptable expertise, by forever-cutoff of correspondence): *JHA* and *JAHH* [& *Isis*].

²² See ScAm 1979, quoting Swerdlow & Gingerich, but primarily dependent on Swerdlow, as DR learned directly from the piece’s unbilled writer, Paul Hoffman, along with Swerdlow’s and Hoffman’s private opinion of Gingerich — which agrees with that of most of the working scholars in the field, especially the best.

known within 2%, all 3 monthlengths accurate to 1-part-in-a-million or better, observatories’ latitudes correct to ordmag 1’), could never have been arrived-at over centuries of investigation, had ancient scientists just unprogressively copied their predecessors.

The cited clique’s mass-slander of all ancient scientists’ empiricism and ethics is widely believed among academics, who’ve no notion that they have been protected by skewed journals from learning that it is nothing but a wrench of history directly caused by the continuing pretense that indoor-cheater Ptolemy was the ultimate ancient astronomer.

[d] The purely dreamt-up claim that it was standard practice for ancient Greek astronomers to select outdoor data to fit indoor theory, merely models all ancients after Ptolemy, in order to then turn around and defend Ptolemy as being just like all ancients, *the very same literally-preposterous logic LEARNED FROM PTOLEMY who faked “observations” agreeing with his theories, in order to then “prove” his theories from these same data.* Shame-shame-shame on DR for accusing JHADsters of ineducability. . . .

B4 Delambre 1817 had noted and Rawlins 1982C had investigated the glaringly unique failure of Ptolemy’s 1025-star catalog (*Almajest* 7.5-8.1) to contain any stars lower than 6° above his horizon, indicating Hipparchos as the catalog’s observer, since his southern Rhodos Island observatory (geographical latitude 35°53’) stood c.5° north of Ptolemy’s Alexandria ($L = 31°12'$). So Schaefer 2001 contended at enormous length, in (yet-another!) *JHA*-Pb-anti-RRN paper, that the catalog could’ve been observed from Alexandria nonetheless because aerosols (atmospheric crud) blocked²³ low stars. Among Pickering 2002A’s unanswerable responses: if this were the problem, the southern limit of the hundreds of stars in Hipparchos’ *Commentary* would also be raised, so it should be 5° higher than Ptolemy’s, but: **it’s the same : END OF ANY REAL CONTROVERSY.** Why didn’t Schaefer know that? Simple: no *JHA* pseudo²⁴ referee noted that, throughout his 42pp paper disputing Hipparchos’ *Almajest*-catalog authorship, Schaefer had never consulted Hipparchos *Comm*’s 100s of star-positions. His later unembarrassed but embarrassing re-tort claimed that no one could know anyway which stars Hipparchos *Comm* was referring to. Which revealed he had no idea how Manitius 1912-3 & Graßhoff 1990 had used the various phenomena, www.dioi.org/fff.htm#ngjm, to pin down virtually every star unambiguously. And Duke 2002C p.33 cleverly noticed that among 13 deep south stars Schaefer 2001 had argued were Ptolemy’s, 5 of these stars shared undeniably huge positional errors with Hipparchos *Comm*, as Graßhoff 1990’s brilliant examination had already shown years earlier. None of these definitive findings has caused Schaefer or *JHA* to retract anything.

B5 It might seem suspicious to some that Ptolemy’s entire celestial system (Sun, Moon, planets, stars) has accurate mean longitude only for Hipparchos’ time, the mean error growing until it reached $-1°.1$ by Ptolemy’s +137 epoch. However, ignoring R.Newton 1977’s devastating new fractional-ending proof (summarized: ‡2 §I) of Ptolemy’s catalog-theft, uncomprehending loyalists kept insisting (e.g., Gingerich 1976 p.477) that this was just an innocent solar error that infected everything else, so Ptolemy’s star catalog was actually observed by him with his armillary astrolabe, unfortunately with that surely-honest error in his zero point in celestial longitude λ . Until Rawlins 1982C noticed a flaw in said vision: the armillary astrolabe (*Almajest* 5.1 & 7.4) doesn’t spin about the ecliptic pole but instead about the equatorial pole; so an outdoor Ptolemy’s longitudinal astrolabe-oops-mis-set by $m = -1°.1$ would’ve caused the instrumental & actual ecliptics to tilt-separate from each other by $m \tan \epsilon = 29'$ (*idem* eq.2, where $\epsilon = 23°.7$, the obliquity then), thus causing an error-wave in celestial latitudes $\Delta\beta = 29' \cos \lambda$ and an error-wave in celestial longitudes equal

²³Ever-fertile Evans 1987 p.166 even argued that 6° of rocks or trees might’ve blocked Ptolemy’s southern view. The easily testable flaw in this alibi is explored in mathematical detail at ‡2 §L.

²⁴Don’t miss www.dioi.org/pm3.htm, longtime (1970-2013) “premier” *Journal for the History of Astronomy* Founder-Editor Michael Hoskin’s efficiency: refereeing&verdict between breakfast&lunch! Must be read to be believed. Lucky nobody will ever find it quoted in our vaunted watchdog “Science Press” whose ever-advancing investigative impotency has carried its transformation into lapdog ever nearer the ultimate intimacy it aspires to: lapdancing a needy establishment.

to $\Delta\lambda = -29' \sin \lambda \tan \beta$, which gets substantial in the north. In the star catalog no such $1^\circ/2$ -amplitude waves exist, so neither did an outdoor star-collecting Ptolemy. Of course, ever-openminded Ptolemyists robo-countered this new shocker with their usual standard-weapon: scientific inability so truly embarrassing as to raise the question of whether impenetrable Ptolemyism has become a medical problem. Exhibiting the science-grasp of Ptolemy's fellow-crank-liar F.Cook, MacArthur-Genius N.Swerdlow attacked $29' \tan \beta$ as indefinable near the celestial North Pole, where $29' \tan \beta = \infty$; this, merely from his own innocence ($\ddagger 2$ fn 8) of undergrad math: celestial (DIO 3 §A2 [d]) or terrestrial (DIO 21 ‡3 §C11) longitudes' conversion to great-circle measure entails multiplication by $\cos \beta$, thus $\text{gt-circ } \Delta\lambda$ can't exceed $29'$. Swerdlow's response: he hides (DIO 8 ‡5 §J4). Meanwhile, Evans' attack on the (§B5) absent-error-waves argument confused sine waves with cosine waves ($\ddagger 2$ §H1) blowing off a 63° degree phase-difference with: "the phase is not exactly right" (Rawlins 1991W fn 152). All this to obscure that he can't find a cosine wave in the Catalog's zodiac stars, with amplitude even $1/3$ as large as the $1^\circ/2$ he sought.

B6 In the 1987 *JHA*, Evans took his 1st large step towards proving he had the appropriate irreproachable honesty & ideological loyalty to succeed (as he did in 2013) then-Editor M.Hoskin, by taking-up no less than *sixty-four* handsome *JHA* pages with two successive Pb papers (Evans 1987), wielding the brand of original math we saw in the previous section, arguing Ptolemy *might* have been honest. (After the revelations of Johns Hopkins University Press' *The Crime of Claudius Ptolemy* [R.Newton 1977: deftly summarized by Thurston 2002S], acceptance of even this weak possibility was about all that Ptolemyists could hope to accomplish among informed scholars. Still the case: e.g., Brandt *et al* 2014B.) Evans' main arguments (see also $\ddagger 2$ fn 47):

[1] In 1981 Evans used a cross-staff to measure the longitudinal distance of a star from the mid-eclipse Moon. "I find on examining my notes from that evening" the longitude's error was $c.-40'$ (Evans 1987 p.275), proving Ptolemy-skeptics were wrong to deny that ordmag 1° errors were not unusual for ancients. However, Rawlins 1991W fn 288 and Rawlins 2009E later showed that if Evans' reduction hadn't bungled his lunar parallax correction's sign, the error would've been merely ordmag $1'$. Evans' response: he's hidden "my notes from that evening" ever since, hiding also from questions on the incident put by Rawlins and Thurston. (See $\ddagger 3$ fn 11. Conman Frederick Cook ducked inquiry identically [Rawlins 2017A §B13]. Again: one can see why Evans is the ideal choice to carry on the *JHA* tradition of spotless integrity.) When later retelling the same argument (nearly-verbatim: Evans 1998 p.259), Evans conveniently forgets to discuss that 1981 eclipse at all (switching instead to adducing a previously unmentioned 1977 eclipse he hadn't outdoor-measured with) — and continues ineducably contending²⁵ for ancient errors of ordmag 1° .

[2] Ptolemy claimed (*Almajest* 7.4) to have outdoor-observed all 1025 stars of the *Almajest*'s Ancient Star Catalog. R.Newton 1977 proved that Ptolemy had instead stolen the

²⁵ Seeing his own 1981/7/16 record proved DR right on that observation, Evans 1998 nonetheless repeats his Evans 1987 argument that Hipparchos' ordmag 1° errors in his 2 observations of Spica (-145 & -134) bolster the case for large ancient observational errors. Evans 1998 doesn't tell his readers that Rawlins 1991W fn 288 had long since shown these observations had *also* been reduced by Hipparchos with the same parallactic sign-flip & that when corrected for this, both his hugely erroneous placements of Spica were merely bunglings of raw observational data accurate to ordmag $1'$. Later, Rawlins 2009E §E showed Hipparchos had made the very same parallactic reduction-slip when seeking Regulus' longitude, causing the worst error of all his fundamental stars ($-35'$). When corrected, the error turned out to be merely ordmag $0^\circ.1$. Evans' furtive (*totally unmentioned*) 1981→1977 eclipse-switch renders it undeniable he's seen DR's detection of his flub. Has he found fault with it? No. So he & his equally ethical colleagues combine to fake that detection's non-existence, by cultwide nontention. Their "reply" is their usual: run away and hide. And all Reputable forums (societies, journals, pop-sci mags, & press), which are seen by the naïve public as Watchdogs of academe, look away for $1/3$ of a century, & still counting. Understand the stakes here: Evans' 1987 *JHA* & 1998 Oxford University Press outdoor "evidence" & sermon constitute *the most prominent & solid-looking of all arguments for archons' precious central myth of ancient science's fumbling non-empiricism*.

catalog from Hipparchos by (as long suspected among astronomers) just adding $2^\circ 2/3$ of 1° /century precession onto Hipparchos' longitudes, a fabrication betrayed by the precession's falsity (actual precession then: $1^\circ.38/\text{cy}$) which ensured that, after 2 $2/3$ centuries of $0^\circ.38/\text{cy}$ slippage, the fakes fell $1^\circ.1$ short of mean reality. Rawlins 1982C added that the Catalog would display large error-waves (details above: §B5) had anyone observed its stars with an armillary astrolabe mis-set by $-1^\circ.1$ of celestial longitude. Evans 1987 tried impressively far-fetched schemes to confuse this desperate situation (the funniest by far was spoofed above at $\ddagger 2$ fn 11), repeatedly following classic Ptolemy-apologist robo-attraction to the inherently unlikely and rejection of the likely. But Evans' voluminous star-catalog apology flamed out when Gerd Graßhoff 1990 brilliantly tested (as neither Newton nor DR had thought to do) for the mass-statistical correlation of Hipparchos' and Ptolemy's star-places, proving (as Alex Jones witnessed 1st-hand), even to formerly-pro-Ptolemy-as-cataloger *Almajest*-editor G.Toomer's honest satisfaction, that the catalog had indeed been plagiarized — thus vindicating Newton's & Rawlins' prior pioneering tests and disproving Evans 1987, Schaefer 2001, & Schaefer 2002.

C Latest Into the Lists

C1 As to the ancient star-declinations issue: what is history-of-science journals' record? Well, both *Centaurus* (in 1982) and the *Journal of Astronomical History & Heritage* (in 2014) received competing solutions to the *Almajest* 7.3 data. In both cases the journal reliably chose the partially inaccurate solution (featuring amateurish procedure and mis-math), while refusing to publish the expertly computed, completely accurate one — presumably because of its heresy in showing (§C5) Ptolemy faked data. Further, both journals refused to acknowledge the content of subsequent communications demonstrating their folly.

C2 Brandt *et al* 2014B is the most recent attempt to exonerate Ptolemy, arguing that the fact that some among *Almajest* 7.3's star-declinations δ are about right for his own time is (Brandt *et al* 2014B p.332) "unlikely to be a coincidence. Hence, [these] observations could have been taken by Ptolemy himself." But said chronological fact is hardly either new or probative, and the reader is deliberately (§C5) not told of other data which are both — and which definitively contradict Ptolemy's observership, all of which were communicated to the *JAHH* & authors ere publication.

C3 Of *Alm* 7.3's 54 star-declinations δ reported by 4 ancient observers, BZJ's 2014 project examined 53: Timocharis 11 stars, Aristyllos 6, Hipparchos & Ptolemy 18 each. These data had already been studied by Pannekoek 1955 (1st to appreciate the accuracy), R.Newton 1977; also Rawlins' 1982 bivariate least-squares study, which *Centaurus* refused to publish, though this scrupulous 46pp paper was sent to K.Moesgaard on 1982/7/14, requesting *Centaurus* publication. Moesgaard's overseer, Editor O.Pedersen, spitefully published instead a mathematically unsophisticated²⁶ monovariate paper, Maeyama 1984 (received at *Centaurus* a year later,²⁷ in 1983 June), whose standard deviations for the four

²⁶Maeyama 1984 is graphical by trial&error and is monovariate ($\ddagger 3$ fn 100), finding nearly accurate values for E , though with estimated (nonmathematically guessed) standard deviations σ_E .

²⁷Maeyama 1984 p.308 acknowledges that he had seen DR's paper in 1983. It appears possible that, until noticing this, BZJ were in some doubt as to whether the DR 1982 ms (unmentioned in Brandt *et al* 2011) was really done then (perhaps supposing that Rawlins 1982G was just based on guesswork not statistics), as suggested by their ultimate omission to state in Brandt *et al* 2014B that DR was specifically the establisher of the Timocharis-Aristyllos split (earlier presciently guessed by Neugebauer 1975 p.34), as well as of *the whole concept* of finding the 4 observers' latitudes & latitude-errors from the data. Why else say (Brandt *et al* 2014B p.331) that DR "quoted" the Timocharis-Aristyllos dichotomy? (In fact, Rawlins 1982G split Aristyllos off from Timocharis and gave both astronomers' dates, explicitly on the basis of star declination studies [calculated & tabulated in the unpublished 1982 ms], adding the novel finding that all five precise ancient Greek star collections showed that their observers knew their geographical latitude L to ordmag $1'$. See §C14 below.) Or why-

Greek observers' epochs E were eyeballed not computed.

C4 Brandt *et al* 2014B attempted modest improvement & useful checks on previous work by bringing in modern satellite-determined data, and providing independent (if shaky) statistical indication of the separation of Timocharis & Aristyllos (the split 1st statistically proposed in Rawlins 1982G). DR was asked to referee the paper: *DIO's* report, www.dioi.org/jau8q.pdf, is on the *DIO* website (as are our letters²⁸ in this connexion), and that report (looking for any possible basis to be positive about) recommended publication of the new material.

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

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

[B] All of the four ancients assumed a geographical latitude L when they observed stars' zenith distances Z by *transit instrument*, then converted the Z data into declinations δ via the equation

$$\delta = L \pm Z \quad (1)$$

(minus-sign for southern transit, plus-sign for northern upper transit, where Z complements altitude h : $h + Z = 90^\circ$), so L 's error carries directly, additively, fully into the δ data, the systematic error of whose mean is therefore the error of L . This would seem to be obvious, but the 1st researcher ever to perform the test upon star data, to show contra-conventionally the admirably small error in ancient star-observers' L -error, was DR, for the History of science Society: Rawlins 1982G. From the δ data contemporary with Ptolemy, all analysts since (including Brandt *et al* 2014B) have concluded that there is but tiny error (ordmag 1') in the observer's assumed geographical latitude L . So it should not be hidden from the reader (as it is, throughout Brandt *et al* 2014B) that *when Ptolemy reduces transit data* (via eq.1), he uses an Alexandria $L = 30^\circ 58'$ (*Almajest* 5.12-13), **which rules him out as the declinations' observer** since this L is in error by $-14'$ (Alexandria being at $L = 31^\circ 12'$).

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

else say that DR was (*ibid*) merely "interested in checking" the latitude-errors? — as if the discovery of these had been around for years. Why, throughout, is Maeyama 1984 usually cited ahead of DR's earlier 1982 works, when both are mentioned? It seems especially strange to find DR's unambiguous priority, in computing separate dates for Timocharis & Aristyllos reported thusly at Brandt *et al* 2014B p.334, www.dioi.org/bzj.pdf, "Until the early 1980s [their dates] . . . were taken to be the same. Currently, the dates are considered to be different (Maeyama, 1984; Rawlins, 1982a, 1982b, 1994)." This becomes even harder to explain when we find that the earlier, refereed (otherwise nearly identical) version of the paper, www.dioi.org/bzj0.pdf, has the verbatim-same wording except for the citations, which were simply chronological back then: "Rawlins (1982, c.1983, 1994); Maeyama, (1984)."

²⁸ DR: www.dioi.org/bjr3g, www.dioi.org/owu8g, www.dioi.org/owu8q, www.dioi.org/owucm.pdf, www.dioi.org/owv9u.pdf, www.dioi.org/owwt2, & www.dioi.org/oww3l.pdf, the last promising not to contact WO further if no reply.

²⁹ Were there a case for a +57 observer, the most tempting identification would be Heron, who recorded an Alexandria-midnight +62/3/13-14 lunar eclipse. But, except in the minds of the most refined of Ptolemy's alibi-artists (don't miss *JHA* Editor James Evans at ‡2 fn 11), the case for non-fabrication vanished long ago: §B6.

had he faked them by adding 2 2/3 centuries worth of his false 1°/cy precession, namely, tacking 2°40' onto all Hipparchos' stellar longitudes. (Closely agreeable date, assuming the star-declinations were faked similarly.) No mention of this Coincidence in Brandt *et al* 2014B! — though urged by referee DR. It has been explicitly well-known for over a century that virtually the same date matches the date for which Ptolemy's 1025-star Catalog's fakes would seem correct, were they real, which few scholars believe anymore. E.g., Peters & Knobel 1915 p.15 noted that +58 is the date when Ptolemy's misprecessed 1025-star *Almajest* 7.5-8.1 catalog is correct. There is no sign that BZJ knew of this match prior to DR's referee report, nor does such vital information appear in Brandt *et al* 2014B subsequent to it, either. This positively belongs at the paper's p.333, where we instead hear that besides his star-declinations (emph added): "Ptolemy offers additional *evidence* for his [(false) precession] value elsewhere in the *Almagest* (e.g., [Toomer 1984 p.]338)" — innocent of the A.Jones-witnessed fact that upon viewing Graßhoff 1990's evidence decades ago, Toomer agreed that the Catalog stars came from Hipparchos — which renders irrelevant the faked "Ptolemy evidence" cited to Toomer 1984 *loc cit*. The *DIO* ref-report asked: "So are we also to ascribe the Catalog to the same secret observer, who thus must have created a catalog of over 1000 stars though no one ever mentioned his or its existence?" [D] For the δ data *Almajest* 7.3 gives for Ptolemy's era, there is (in residuals) a fully clean split (perhaps unknown before DR's 2011 letter and 2014 ref report to BZJ), between the suspect 6 star-declinations δ which Ptolemy analyses, and the unsuspect 12 data (which he doesn't analyse in *Almajest* 7.3) — if one adopts the independently-arrived-at epoch E (+159) and geographical latitude-error x (+4') already found through bivariate analysis (Rawlins 1994L) upon the unsuspect data, without any selection among or tampering with these 12 data, long-separately-recognized and separately treated by Ptolemy. Significance of this for Brandt *et al* 2014B's peculiar new split is emphasized below at §C21.

C6 *JAHH* readers *have a right to know* §C5's four central considerations, but Editor Wayne Orchiston (WO) has not felt the need to inform³⁰ them. So much for the integrity of the paper's Ptolemyist conclusion. And of the *Journal of Astronomical History & Heritage*.

C7 Brandt *et al* 2014B p.331 claims that its analysis is a bivariate repeat of DR's 1982 analysis but (as warned in www.dioi.org/jau8q.pdf, the 2014/8/26 *DIO* referee report on the paper) it is really³¹ just a try (like Maeyama 1984) at solving a bivariate problem monovariately. Except for Ptolemy (where different samplings³² obviated a comparison), the resulting epochs E and their standard deviations are mostly about the same as those of DR 1982, after the standard deviation of Aristyllos' epoch E was fortunately brought into near-agreement with DR's recommendation, www.dioi.org/jau8q.pdf, before publication (compare www.dioi.org/bzj0.pdf, vs www.dioi.org/bzj.pdf, for Aristyllos). A peculiarity of Brandt *et al* 2014B is that values for x & its standard deviation σ_x are not given anywhere: not even when BZJ attempts recounting and repeating Rawlins' analysis (which explicitly supplies and tabulates both x & σ_x). Instead, BZJ present miscalculated (§C9) values for a single entity, "accuracy", which they confusedly seem to regard as sufficiently equivalent.

C8 The *DIO* referee report warned³³ that the "accuracy" values given at Brandt *et al* 2014B p.331 are "astonishingly low".

³⁰The debate's existence is mentioned, but without the key indicting details. Perhaps the authors & editor feel that their admirably full and neutral bibliography suffices to provide representation for dissent. But there is no excuse for silence in the text (which is all most readers see) on the undeniably indicative items listed above at §C5.

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

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

³³BZJ were helpfully provided sufficient advice to inspire proper caution: [1] were given all the right answers for E , x , and both's standard deviations, [2] were told that their own values for "accuracy" looked remarkably too small, & [3] were repeatedly warned not to do the problem monovariately.

C9 The erroneous³⁴ figures for “accuracy” rs in Brandt *et al* 2011 and Brandt *et al* 2014B were an ordmag too small, presumably because they were mistakenly found³⁵ (as hinted at in Brandt *et al* 2011) by [a] searching monovariately for the E that minimizes the sum S of the squares of the residuals, [b] subtracting the subsequent mean residual from each datum, [c] with the adjusted data, re-computing the problem nullivariately for an independently estimated best E , [d] computing “accuracy” rs by meaning the minuscule leftover residuals. (Our reconstructions of data via this procedure are in fn 35.) Perhaps we could dub this the “least-non-squares test”. The impossibility of BZJ’s numbers are easily seen: when Brandt *et al* 2014B p.331 puts the “accuracy” of Aristyllos & Hipparchos at $0^\circ.004$ & $0^\circ.003$, this translates to 14&11 **ARCSECONDS**, resp — obviously a fantasy (and BZJ were warned of this on p.4 of www.dioi.org/jau8q.pdf, DR’s invited referee report), considering that the data’s rms is admittedly $0^\circ.1$ on the same page: Brandt *et al* 2014B’s Table 2. (Equally incredible: *idem* lists rms values for methods of Maeyama, Rawlins, & BZJ — that agree with each other to a 1000th of a degree!)

C10 Interlude: From where did BZJ get §C9 [d]’s wacky idea that one should simply *mean* the residuals? Answer: from misconstruing p.283 of their preferred prior analysis, Maeyama 1984, where it is stated that (following determination of epoch E), “The epoch [where S is minimal] will be our first approximation. The resulting *mean deviation* at this epoch will then correspond to the mean systematic error³⁶ . . .” Brandt *et al* 2014B mis-read this as referring to a simple *averaging* of leftover residuals. BZJ’s procedure and cue from

³⁴ In addition to the reversal-test revealed at §C11 — showing the invalidity of the paper’s method — there is this equally obvious consideration: in Brandt *et al* 2014B p.331 eq.1, the coefficients of the unknowns are 1 and $0.3338\cos\alpha$. The rms value of the latter in these investigations is about 1/4, so the standard deviations for x and E should exhibit a ratio of about $1'$ in L to $4'$ in E . In Rawlins 1994L Table 3 and below in Table 1, this is roughly true. But no such symmetry appears anywhere in Brandt *et al* 2011 (BZJ’s *BAAS* 2011 abstract) or Brandt *et al* 2014B p.331. (Note: The paper Zimmer *et al* 2013 admirably takes no part in evaluating anyone’s x — or anything at all about Ptolemy.)

³⁵ BZJ’s initial abstract, www.dioi.org/bzj11.htm, Brandt *et al* 2011, gave figures for “accuracies” (where we flip BZJ’s unconventional C–O signs): Timocharis $E = -295$, 11 stars $rs = -0^\circ.022$, Aristyllos $E = -258$, 6 stars $rs = +0^\circ.004$, Hipparchos $E = -128$, 18 stars $rs = -0^\circ.010$, Ptolemy $E = -115$, 18 stars $rs = -0^\circ.005$. Later, Brandt *et al* 2014A p.6 & Brandt *et al* 2014B p.331 have (now signlessly), for the same samples & dates, rather different rs : Timocharis $0^\circ.012$, Aristyllos $0^\circ.003$, Hipparchos $0^\circ.004$, Ptolemy $0^\circ.009$. Our speculative reconstructions (via §C9’s [a]-[d]) alter the experiments but (in a delicate problem) get agreements with some among BZJ’s above false rs values. So maybe this or something like it was BZJ’s procedure?

[Accurate bivariate least-squares result follows each observer’s reconstructed BZJ data, in brackets; plus actual minimal residual-square sum S_m , to show that most BZJ solutions do not approximate it.] Timocharis 12 stars: $E = -295$, $rs = -0^\circ.022$, $S = 2745'^2$.

[$E = -277 \pm 18'$, $x = -0^\circ.076 \pm 0^\circ.077$, $S_m = 2441'^2$.]

Aristyllos 6 stars: $E = -258$, $rs = +0^\circ.003$, $S = 147'^2$.

[$E = -258 \pm 10'$, $x = +0^\circ.016 \pm 0^\circ.045$, $S_m = 147'^2$.]

Hipparchos 17 stars (Alioth $\delta = 67^\circ 3/5$): $E = -128$, $rs = +0^\circ.004$, $S = 446'^2$.

[$E = -133 \pm 8'$, $x = -0^\circ.001 \pm 0^\circ.021$, $S_m = 392'^2$.]

Ptolemy 18 stars $E = +111$ (Brandt *et al* 2014B Fig.5 no-prop-mot), $rs = -0^\circ.005$, $S = 2539'^2$.

[$E = +115 \pm 13'$, $x = +0^\circ.004 \pm 0^\circ.052$, $S_m = 2521'^2$.]

³⁶ In his 1983 Aarhus talk Maeyama did not yet know that “mean systematic error” relates to error in the observer’s assumed latitude. He later disremembered that he learned this from DR’s ms: fn 27 above. The results displayed at Maeyama 1984 p.292 Table 1 are not from bivariate but monovariate least-squares — and not even via calculus: just by graphing trial&error to find S . Nonetheless, the values found for x (though not recognizing it as latitude-error) and E are roughly correct, since Maeyama in-effect was running a double-monovariate test and had the good fortune that the unknowns’ correlations were not too serious. And at least (unlike BZJ) he realized that the leftover residuals after the 1st monovariate test were to be fed into the 2nd such, to find the value of x which *minimized the sum of the residuals’ squares*. However, for x ’s standard deviation σ_x , Maeyama 1984 Table 1 column d wrongly lists σ_0 , the mean error of a single observation. The resultant errors range as high a factor of nearly 7 (the Hipparchan 44-star sample).

Maeyama 1984 are clear from Brandt *et al* 2011, though Brandt *et al* 2014B p.331’s false presumption is that rs will serve instead of x . But what then of x and its standard deviation σ_x — neither even mentioned by BZJ? While Maeyama 1984’s estimates of σ_x are (fn 36) off by serious factors, Brandt *et al* 2014B’s misconception produces nothing at all! — no σ_x whatever.

C11 The invalidity of Brandt *et al* 2014B’s procedure (above, §C9) is easily established by performing it in reverse: assume an E_0 (instead of an x) and solve for x via monovariate least-squares — then find rs by summing the residuals towards finding e : but the sum is flat zero! (A hint that gauging accuracy here requires rms, not means.) Thus the error in E would be deemed zero. And any linear function in Brandt *et al* 2014B eq.1 would produce the same result. This for any assumed E_0 — so, by the same reasoning Brandt *et al* 2014B used for finding rs , we must conclude that all starting values for E_0 (before launching the foregoing monovariate analysis) turn out to be errorless.

C12 Moreover, any of those who’ve since 1982 tried vainly to improve upon DR would have remarked (had they done a valid 2-unknown simultaneous least-squares) that the rs for the Greek observers is zero in all four cases. None has. A useful extra check: in the cases where correlations happen to be tiny (e.g., Timocharis & DR’s Hipparchos analysis where $n = 19$ stars), one can come quite close (since x has a unity coefficient in Brandt *et al* 2014B’s eq.1) to finding x ’s error σ_x through just dividing σ_0 by \sqrt{n} .

C13 In a true bivariate solution, e & x are least-squared simultaneously.³⁷ All the figures given in the 1982 manuscript and in Rawlins 1982G were so accomplished. (By hand, incidentally. The later computerized solutions, 1994, 2011, & present Table 1 here [identical to ‡3 Table 2 above, except for Timocharis] barely differed at all.)

C14 Before 1982, no one had ever used these data to find the accuracy of the four Greek astronomers’ observatory-placements. Misled by the crudity of the data of most of Ptolemy’s *Geographical Directory* (*GD*) & the rigid infectious mantra of certain history-of-science archons, that ancient Greeks were non-empirical (Rawlins 2008R §A), many had long ago gotten the idea (persisting to the present in the History of science Society’s rulership: ‡1) that ancient geography was typified by position errors of ordmag 1° . That was why Rawlins 1982G — whose main analysis showed $1'$ precision in the solar transit work of 3rd century BC Alexandrian astronomers — emphasized this revelation (in a brief footnote on stars: fn 27 above) to a History of science Society audience: DR’s 1982 discovery that bivariate least-squares had determined for the 1st time, from the *Almajest* 7.3 declinations, just how well Greek astronomers could know their geographical latitudes L — which of course led on to the question of why the *GD*’s coordinates were so awful (‡1 §I; ‡3 §II1; Rawlins 2008S). DR’s papers have called L ’s error x , and epoch-error e (epoch E solution minus tester’s assumed E_0); if the mutual solution is done truly bivariate, it will find not only e (thus E) and e ’s standard deviation σ_e , but x and its standard deviation σ_x , as well as the single-datum standard deviation, σ_0 . All these solutions are displayed here in Table 1.

C15 If it seems odd that, previous to 1982, no one had found the accuracy of the four ancient observatories’ location (see Rawlins 1985G §3 conclusion), let’s expose something even more revealing: in 36^y since 1982, **no one else has computed it correctly, either**. The two post-1982 papers both waste precious journal-space extensively on graphs & histograms, all to do the analyses inferiorly, e.g., monovariately finding that E where S is minimal, S_0 . (And the archons of history-of-ancient astronomy imagine they have the capacity or right to judge, shun, condemn, & censor the scrupulous, competent researches of scientists in such matters? Note the parallel to the *Journal of Astronomical History* &

³⁷Note problems at fn 42 & esp. fn 45 if done otherwise. Monovariate tests might successively minimize the squares of the residuals by finding the e that does so, then the x that does, then etc, etc, whittling S into ever-smaller remoteness from minimum, at each cycle. This would be the cumbersome, tediously-iterative serial-monovariate approach. But why not just elegantly solve x & e at-a-swoop (with trivial iterativity from non-linearity), with total exactitude, through true simultaneous bivariate least-squares — as was done back in 1982&1994, and here in Table 1.

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

Obsvr	$E \pm \sigma_E$	Adop L	Its Error x	Actual $L \pm \sigma_L$	σ_o	σ_r
Timoch	-302 ± 08^y	$31^\circ 12'$	$+1'.5 \pm 1'.9$	$31^\circ 10'.5 \pm 1'.9$	$\pm 6'.1$	$\pm 5'.2$
Aristyll	-258 ± 10^y	$31^\circ 15'$	$+1'.0 \pm 2'.7$	$31^\circ 14'.0 \pm 2'.7$	$\pm 6'.1$	$\pm 4'.9$
Hipp	-131 ± 05^y	$36^\circ 08'$	$+0'.2 \pm 1'.2$	$36^\circ 07'.8 \pm 1'.2$	$\pm 5'.2$	$\pm 5'.0$
Anon	$+159 \pm 09^y$	$31^\circ 15'$	$+4'.4 \pm 2'.0$	$31^\circ 10'.6 \pm 2'.0$	$\pm 6'.0$	$\pm 5'.6$

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

C16 In 1994, 12^y later than 1982, DR discerned a new method for finding each observer's *assumed* geographical latitude: from nulls³⁸ in his data's fractional-endings' frequency-profiles (as explained in Rawlins 1994L §F) which, by subtraction of x , easily produces each observer's absolute *actual* latitude L . All four least-squares-fitting E and epochs L (Timocharis 11 stars; Aristyllos, 6; Hipparchos, 19; Anonymous, 12), along with their standard deviations (σ_E & σ_x), as well as single-datum standard deviation, raw (σ_o) and with the effect of rounding³⁹ removed (σ_r). All these desiderata are produced here in Table 1, slightly⁴⁰ improved (see fn 38) vs the values of ‡3 Table 2 above or Rawlins 1994L Table 3. Strangely, Brandt *et al* 2014A, the refereed version of Brandt *et al* 2014B, www.dioi.org/bzj0.pdf, claimed that Rawlins 1994L had latitudes "close to our values" — this, even though BZJ *have to this day never solved for any of these latitudes*. So www.dioi.org/jau8q.pdf, the DIO 2014/8/26 referee report, at pp.3-4, suggested that this point be clarified and *that Rawlins 1994L's x values and absolute L values for all four observers* be printed, *since BZJ had brought up the point*, and had supplied various⁴¹ of DR's other numbers (the majority correctly) — preferably along with a sentence on the novel though simple means which DIO had invented while pioneering this entire line of inquiry. But, probably because DIO's x values especially & hugely disagreed with JAHH's "accuracy" values, the published article did none of these things.

C17 Following such odd doings, Brandt *et al* 2014B performs somersaults of arbitrariness,⁴² and unorthodox implicit weighting, while splitting the "Ptolemy" 18 stars into two groups (after dropping three stars at p.332, then a reshuffled four at Fig.10) — groupings

³⁸ The sole non-fit for the dozens of data in the nulls experiment was Timocharis' Aldebaran. Rawlins 1994L fn 39 suggested that the original North Polar Distance may have been $81^\circ 1/15$, recorded (conventionally for unit-fractions) as $81^\circ 15'$, but later misrecognized (like ‡3 fn 44) as 81° & 15 arcmin, thus $\delta = 8^\circ 3/4$, as at *Almajest* 7.5. Thus, reconstructed true $\delta = 8^\circ 14/15$ or $8^\circ 56'$, which also shrinks a poor residual. And Arcturus obviously bears a 1° scribal error; restoring the original and eliminating outsized-residual for Zubenelgenubi (sloppily-rounded $\delta = -5^\circ$), we have the Timocharis entry in Table 1 here. (For Timocharis' results based on non-reconstructed data, see ‡3 Table 2.)

³⁹ Timocharis & Hipparchos used a precision of $p = 12$ intervals/degree; for Anonymous, $p = 8$; Aristyllos, $p = 4$. The inverse of $p \cdot \sqrt{12}$ is the rms of the effect of average rounding, in degrees.

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

⁴¹ E.g., at Brandt *et al* 2014B p.331, for all 3 observers, our 1982 ms' epochs E & σ_o are relayed, conspicuously omitting our x & σ_x .

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

which by either version of the paper were previously unknown to Ptolemy or anyone else. The p.332 grouping is E (Early 6 stars) and L (Late 9 stars), which does not follow the traditional split, namely: the "SickSix" stellar declinations (which Ptolemy "deduced" his false precession from) versus the "Clean Dozen" real declinations (which his precessional math ignored): "our groupings have no simple connection to Ptolemy's selected six stars" (Brandt *et al* 2014B p.334). Why? Well, R.Newton 1977 pp.220-225 rightly argues that Ptolemy typically fabricated the SickSix from $1^\circ/\text{cy}$ precession in order to equally-typically then "prove" said precession from the fabrications: ‡2 §B2. The new grouping would undercut this view by fracturing the SickSix. Problems with the new split (as noted in DR's 2011/3/15 letter to Brandt): the traditional split is simple, is Ptolemy's own. Further, DR's 1994-adopted +159 epoch (Rawlins 1994L fn 45: all 12 Clean Dozen stars with *no deletions*) was understood in 2011 for the 1st time to *establish no residuals-overlap* (fn 7) in the Clean-Dozen-vs-SickSix split, a finding nowhere cited in Brandt *et al* 2014B, which keeps mis-rendering DR's also-uncited +159 date, though it was repeatedly, www.dioi.org/bj3g.pdf, www.dioi.org/jau8q.pdf, put to BZJ. As is obvious from Brandt *et al* 2014B's Figs.7&8 (C–O),⁴³ star-residuals' proximity to each other is *time-dependent*. E.g., the residuals of Alioth and Aldebaran are $18'$ apart in +128 (Ptolemy group L date of Brandt *et al* 2014B's Abstract, Table 2, and pp.332&334) but are within $2'$ of each other in +159. Indeed, as DR's 2014/8/26 ref report noted, if we eliminate none of the Clean Dozen and run a bivariate least-squares on them *exactly as they stand*, the solution is $E = +159 \pm 9^y$, $x = +4' \pm 2'$ ($L = 31^\circ 11' \pm 2'$: Alexandria) and the extremest residuals are symmetrically within $10'$ of zero: Betelgeux + $10'$ & Pollux $-10'$. For contrast, one may examine the results of applying, to the Sick stars, the very same test just done on the

actually higher than that of the non-eliminated stars Altair, Castor, Pollux, & Regulus. So the final published version (p.332) expanded the justification for eliminating the Unhelpfuls to include that their zero error occurred later than 200 AD (a criterion eliminating Altair [p.334 vs p.335&Fig.8], though it was nonetheless retained as a member of group L) which only adds on a date-based bias among those small-declination-speed stars — though the date is the very entity one is seeking. (In dropping Betelgeux, DR was guilty of a similar mis-step in 1982 [repeated in a different way by BZJ in 2014] which was cleared up in 2 stages, in 1994 and — thanks to Brandt's restimulation of interest — in 2011. But this was explained in DR's 2011 letter to Brandt, to no effect.) The ref report also noted a mistaken claim (p.334) that one of the SickSix stars is among the Unhelpful, though none are (by either of the paper's two versions of grouping) — and, in 2014's DIO referee report, www.dioi.org/jau8q.pdf, we guessed (though not even told the membership of groups L&E) that the paper's statement that three of six E stars were Sickies should read four of six. This turned out to be the case (Alcyone, Capella, Spica, Alcaid) — again, for either version of grouping. Yet no correction was made before publication. Is this odd slip vestigial of an early trial-version of the selection process, during the sample-forming's shopping-around period (similar to the slip at Duke 2005T p.173, noted at Rawlins 2009S §K6; and don't miss fn 22) — before settlement upon the final versions of L&E? This further hint of arbitrariness is one of the factors vitiating the paper's lengthy, impressive-appearing search for groupings of stars different from the skeptics' simple acceptance of *Ptolemy's own groupings*. In the final paper, no correction occurred for the above-cited ref-noted fact that Aldebaran was eliminated from BZJ's groups L&E, though, again, it was moving faster in declination than non-eliminated stars: the selection of the Unhelpful Threesome was published unaltered at p.332. (The paper's last version of groupings [in Fig.10] restored Aldebaran while booting Altair & Castor.) More important than these errors is the general misconception that slow declination motion is ground for dismissal (even while Brandt *et al* 2014B believes it is looking for latitude "accuracy") — when these are *the very stars that least-flexibly measure latitude-error*. Real bivariate investigation would know that and would know that getting E exactly right cannot occur without simultaneously doing likewise for x , since all the correlations are non-zero, and some are non-trivial.

⁴³ But labelled O–C. The various Brandt *et al* 2014B Figures confuse O–C (Observed-minus-Calculated) with C–O (evidently a routine weakness among historians-of-astronomy [though here fortunately harmless], e.g., ‡2 §F8, & www.dioi.org/fff.htm#bvmn). And Brandt *et al* 2014B's eq.1 (p.331) is founded upon a confusion of errors with residuals (possibly just a misunderstanding of the Rawlins 1982 ms' eq.2), thereby equating Observed-minus-Calculated with what is actually just Calculated. If taken seriously, this makes Observed equal to twice Calculated!

Clean Dozen, eliminating ever-problematic⁴⁴ Arcturus — leaving a consistent set we might as well call the “SickFive” — the resulting (unweighted) residuals are mostly about 1/2 degree, the smallest⁴⁵ being 17'. No overlap at all. A lovely split. So there's just *no need*⁴⁶ to get fancy over dividing the “Ptolemy” 18 stars. Unless one is extremely, *extremely* determined to undermine acceptance of R.Newtonian skepticism about Ptolemy — by any sleight necessary.

C18 For finding epoch *E*, Brandt *et al* 2014B adheres to depending on each star's “crossing time” (the year when its residual is zero) & “slope” (rate of change of declination/year). Though of some interest and utility as rough checks (on better procedures), these approaches are sub-prime (especially when compared to standard approaches — which are perhaps avoided by BZJ since they give results in accord with R.Newton?), repeatedly necessitating debatable decisions on deletions and weighting. It's almost as if it was decided to hunt up results every which way but the best: full bivariate least-squares. E.g., small-slope stars' low weight (for *E*-determination) is automatically accounted-for by least-squares, so there is no need to delete such stars — additionally: doing so will obviously degrade the solution for *L* (as already noted at fn 42), though the paper indicates no awareness of this as it deletes 3 or 4 stars (not quite the same ones), from one section to another.

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

C20 The paper concludes with a long, illustrated section (slightly altering §C17's L-vs-E regroupings that replaced Ptolemy's simple split) which tests for clusterings in stars' crossing-times. This is a patently poor basis for eliciting anything valuable, for the obvious reason that the crossing-times' reliabilities are highly disparate (§C19), due to slopes that vary from nearly the full possibility (0'.3338/yr) to virtually zero — the latter producing nearly valueless crossing-times, which lead to exclusions and inclusions based on virtually random happenstance. The cluster-analysis deletes (p.335) Castor, Altair, Betelgeux, & Sirius (not consistent with earlier deletions [p.332] of Aldebaran, Betelgeux, & Sirius [§C19]), yet in both cases, stars with slopes weaker than some of these are retained. (See, e.g., fn 42 above.) E.g., why does the clustering section of the paper eject Castor but keep Pollux, whose slope is smaller? — probably because their mutually wan slopes (*nearly*

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

⁴⁵If we re-check the residuals via monovariate test for *L* at Ptolemy's alleged observation-year, +137, instead of +159, we find the Clean Dozen more poorly fitting, residuals ranging from $-8'1/2$ (Zubenelgenubi) to $+15'1/2$ (Betelgeux), the departure from zero of the worst is half again larger. *This is one of several recommendations for using the full Clean Dozen and epoch +159* — others being: a lower median error (3' vs 4'), even despite a higher σ_0 (6' vs 5'); an untampered sample (no deletions); and agreement with the *Suda*'s date for Ptolemy, Marcus Aurelius, +160, not the Ptolemy-claimed epoch: Antoninus, +137. (More exactly, the two epochs are 160/7/14 and 137/7/20 Alexandria App.Noon.) Note: a monovariate *e* solution for the same stars leads to +150, a *serious difference* (see §C13 on simultaneity). And, since such automatically assumes $x = 0$, we have $L = 31^\circ 15'$ (see §C16 above, & Rawlins 1994L §F8), which is 3 nautical mi north of Alexandria's $L = 31^\circ 12'$, whereas the +159 bivariate solution $x = 4'$ closely reflects the $+3'$ error in the observer's overlarge assumed $L = 31^\circ 1/4$, and so is effectively right-on: $L = 31^\circ 11' \pm 2'$ (*ibid* Table 3). All of these neatnesses render it doubly strange that Brandt *et al* 2014B persistently refused to recognize DR's discovery of +159, misprinting it (over warnings) again&again as +131. See §C21 below.

⁴⁶See at www.dioi.org/jc01.pdf, *DIO 14* ¶1 §J2; & www.dioi.org/jc03.pdf, *ibid* ¶3 fn 13, the parallel case of now-needless metrological theories that keep getting proposed to weakly explain the already strongly explained ancient Earth-size of Sostratos-Eratosthenes.

horizontal) in Fig.7 magnify a tiny difference (just a few arcmin: less than σ_0 !) into a difference of *most of a century* in crossing-times. But, while Castor's track crosses zero 8 decades too early (21 BC: p.335) for “verifying” the group E epoch (already established earlier in the paper at p.332 as +57), Pollux's track accidentally crosses zero conveniently near the pre-desired date.

C21 A peculiarity related to the question of accurately locating the “Ptolemy” stars' epoch *E*: Brandt *et al* 2014B *repeatedly* ignores (fn 45 above) the request, by the *DIO* referee report, www.dioi.org/jau8q.pdf, to correct its *repeated* misrendering of Rawlins 1994L's date (for Anonymous' Clean Dozen stars) as +131 instead of Rawlins 1994L Table 3's actual published value, +159. This is a 28^y difference, which matters, as we saw at §C17 & fnn 42&45. (The misprinted epoch, +131, was merely Rawlins 1994L's date for *Hipparchos* — not Ptolemy's Anonymous — additionally shorn of its minus sign!) Importantly, +159 is an epoch which, for the 1st time, renders stark and unambiguous (§C17) the Clean-Dozen-vs-SickSix split (by contrast to *any* of the many other Anonymous epochs *E* that were flirted-with in the article or the referee report), and in doing so implicitly jettisons as needless (fn 46) Brandt *et al* 2014B's elaborately-derived unorthodox groupings. (See fn 42 above, for the advantages of adopting what is after all Ptolemy's own split.)

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

C23 As a final quietus to *JAHH*'s monuementally stubborn 2014 adventure, we now show how easily a scrupulous journal could have checked on whether its or our solutions for *x* were correct. All *JAHH* needed to do was: vary the $x \& e$ of their solutions to see if their residual-squares-sum *S* was minimal, i.e., equal to our minimum, S_0 . (Which is the square of the appropriate σ_0 in Table 1, multiplied by the number of degrees of freedom.) Or: for any of the four ancient astronomer's star-residuals, [1] subtract *DIO*'s tabulated⁴⁷ *x* for that astronomer, and then [2] just re-run Brandt *et al* 2014B's monovariate test for him. BZJ will then encounter a sorta-pleasant surprise: all four astronomers' values of *S*, the sum of residuals-squared, will be found to have declined (comparisons in fn 35), showing that Brandt *et al* 2014B's *S* generally (except for Aristyllos) didn't get very near optimal (extremal) solutions S_m . (Due to low correlations, the differences are not huge; but they show that true bivariate procedures were not applied by BZJ.) Even so, the suggested better solution being found by crude means (fn 36), results in *S* usually near but not quite at the lowest *S* possible. This goal can, however, be accomplished through a true bivariate least-squares (as in Rawlins 1994L), which efficiently finds the point in $x-e$ space where *S* is a minimum. If BZJ have any doubts that *DIO* has found THE actual minimum *S*, they need only conduct the very same test, using our $e \& x$ values, to find an *S* lower than their own. Using the slightly improved values (vs the referee report) of Table 1 above, the *S cannot be decreased further* (more than micro-trivial noise) by varying either *x* or *e*. Our solutions for *x* are just 0-4 arcmin. The size may be small, but the issue isn't: the

⁴⁷DR's latitude-errors *x* for Timocharis, Aristyllos, & Hipparchos have for decades been available, for all to check, at Rawlins 1994L pp.44-46 & Table 3 — virtually the same as in his 1982 ms, to which Brandt *et al* 2014B p.331 acknowledges access. The BZJ paper's sampling differs from Rawlins 1994L's 19 stars (vs BZJ's 17) for Hipparchos & 12 stars (vs BZJ's 18) for Ptolemy, but the *x* that's appropriate for BZJ's sampling was provided at pp.3-4 of www.dioi.org/jau8q.pdf, the *DIO* 2014 ref report. (Due to minuscule differences in adopted star-places, the *x* value that will produce minimal S_m may not be super-precisely identical to *DIO*'s; but further trials will easily find it, very, very nearby.)

central point here is (as 1st revealed in Rawlins 1982G) that ancient scientists found L to ordmag 1' accuracy. For that reason, as well as Brandt *et al* 2014B's p.331 advertising 1' accuracy, the most precise solutions for x are appropriate. This becomes important (fn 45) for the Clean Dozen, where $x = 4'$, closely reflecting the error in the observer's adoption (independently demonstrated in Rawlins 1994L §F8) of $L = 31^\circ 1/4$ for Alexandria, which is 3' (close to $4' \pm 2'$) larger than the reality: $L = 31^\circ 12'$.

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

D Watching a Cemental Field Resort (& Slipper) to the Bottom — How Archons Justify Printing&Printing&Printing Just One Side

D1 Observing unbroken consecutive decades of *unexceptionally* invalid defenses of the indefensible myth of outdoor Ptolemy, one may justifiably draw conclusions.

D2 The truth behind the unprincipled⁴⁸ — sometimes (e.g., fn 18) even vicious — stubbornness⁴⁹ of those determined to protect Ptolemy from public exposure by any means (www.dioi.org/mot.htm#xcpf) is that they are not protecting him but themselves and/or their gooroo — resorting to any sloppy argument, any curtailment of free discourse necessary to prevent the larger scholarly community as well as the public from learning that the field's most powerful archons (controlling the funding and thus the career-security, rewards, & awards of those who volunteer to espouse and do battle for sacred myths) made two huge and related blunders (see ¶2 §M2: “*to fit him*”), when they long ago prematurely announced Ptolemy an honest observer and misperceived Greek astronomy as non-empirical.

(See, e.g., ScAm 1979, discussed above at fn 22; and more thoroughly at ¶2 §M3 & fn 52.)

D3 Being politicians, Ptolemaist archons are the sort of people whose idea of intellectual engagement tends (for obvious reasons) not towards weighing scientific arguments but to [1] slandering (¶2 fn 5) their opposites as fools, knaves, and nuts (before discussing evidence — if ever doing so at all), while [2] pointing⁵⁰ innocent onlookers to the bemedalled, Reputable people who've taken their side: after all (as we ask at above p.87, in the Text-For-the-Day intro to this article), how could such cynosurae seem so Enormously Stupid — as they must be or act, if skeptics are right?

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

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

⁴⁹ Schaefer 2002 rightly deemed the Ptolemy Controversy the hottest in the entire field of history of astronomy. Which is why the decades-long near-hermetic suppression of one side of the debate is so: impressive. And as ethically repulsive as the tactics employed — *by the chiefs of the field* — as so unambiguously documented here and in, e.g., ¶2 fnn 1, 3, & 5.

⁵⁰ Without citing the various powerful evidential proofs that Ptolemy stole the star catalog, Schaefer 2013 p.47 instead revealingly resorts to sociology to aver that we can't KNOW so because herd-loyal Ptolemaists (like BZJ) still exist: “neither side [is] able to produce decisive evidence to convince the other side.” (But one thing we can be sure of: inserting such archon-comforting & gratuitous irrelevancy is sure to get a paper published at *JHA*.) One trusts that jollypol Schaefer is smiling as he watches the JHAD show. And one recalls Thurston's quote from Bishop Berkeley, “I observed how unaccountable it was, that men so easy to confute should yet be so difficult to convince.” Another *DIO* motto (DR), which extends also to many other faiths: “Why does anyone continue believing a tenet he cannot defend in discussion?” (For these & other *DIO* germs, see www.dioi.org/mot.htm.)

⁵¹ Does the fatal crime, Ya-Disrespected-Me, sound familiar? Seen any mob or blaxplo films lately?

— publication *or even citation* (¶3 §A1 item [A]) of expert criticism of this Special Literature (especially *DIO*'s), revealing defenders' fatal mismatch. No exposure or admission of *JHA*'s cringing Editor Evans' 1987 parallax screwup & suppression of his experimental record (§B6 item [1]) will ever appear in the irredeemably dishonest *JHA*, though undoing this now-conscious deceit is a required&essential part of any counter to Ptolemaism, since it is the Pb-paper-prominent “empirical” centerpiece of the JHAD's fantastic 1987-to-1998-to-eternity tenet that huge, Ptolemy-sized observational errors were normal in antiquity. Further, no mention is allowed of definitive evidence (fn 3) of JHAD crimes against academic decency (such censorship constituting just one more crime to be henceforth protected by censorship!): evidence-hiding (as just noted), data-fudgery (¶3 §§C-G: a half-dozen examples), slanderous lies (§B1; ¶3 fn 5), thereby implicitly revealing what has been for 40^y the awful hidden truth, namely, that the *entire* Believer side of the Ptolemy pseudo-debate has actually long since become *no side at all* (as with creationists), their output having no coherent case whatever, thus resorting to tactics as cited, plus increasingly farout & embarrassing coulda→musta alibi-scenarios. (Deepest dregs at ¶2 fn 11.)

D5 Such childishly obvious illusionism, as delineated at §D4 above, utterly confounds the non-specialist part of academe, as well as the *increasingly non-investigative* (and ever-seminumerate) “science” press, and is the key to the endless pretense of Ptolemaist *historians-of-science* — knowingly careless of concomitant hurt to academe's accurate perception of ancient *history* — that archons have not-either been proven as *Notoriously* foolish as Raines: if just a few of puppeteer Gingerich's claque can forever (§A2, & Rawlins 1992V §C24) keep publishing contrived even-if-laughably-transparent-to-scientists defenses, sapping an ever-befuddlable lapdog press-corps, then the prime long-term public perception is secure:

Not a single Ptolemy-defense archon was ever wrong on his honesty.

(Gerald Toomer the admirable rule-proving exception: §C5.)

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

D6 Beyond Ptolemaists' lack of science's attitude is the mundane matter of skills. Virtually every member of their clique, whatever his eminence, has no high scientific expertise⁵² relevant to the Ptolemy controversy. Non-specialists — unable to understand the debate's technical details (or too busy to take the time) — are oft impressed with networking archons' too-oft-network-granted posts, awards, university connexions, etc. And are thus so easily diverted from the seemingly obvious point that just because a Ptolemaist is an astronomer doesn't mean that he knows much about *positional astronomy*, *orbit theory*, & *statistics*, which are the specialties one needs for research into ancient astronomy. (Astrophysics, planetary astronomy, & spectral analysis are worthless for it.) These are the very specialties of such astronomers as R.Newton and *DIO*'s Myles Standish and DR. Lack of such expertise shows up in the truly historic fumbblings of, e.g., Swerdlow & Evans, as shown above in §B and the especially comedic do-not-miss topsyturvyfest at ¶2 §N — and now in the serial-learning-experience stats (§C8) of *JAHH*'s 2014 paper.

D7 Concluding: we anticipate that (unless made shy by our 2014 referee report, www.dioi.org/jau8q.pdf) Ptolemaist archons are already typically (§D3 above) pointing to Brandt's many awards&posts, as if relevant, proud that yet another Reputable Figure has, after reviewing the evidence, decided to help the needy establishment by coming out for Ptolemy, hoping no-one will notice the §A1-obviousness of the fact that eventual discordant-evidence-sterilized Brandt *et al* 2014B's conclusion was all-along set in cement.

Slippers.

⁵² In mathematical history-of-astronomy, dimbulbs + careerists + thespians + pols *now constitute a majority*. If able, honest scientists ever rejoin&review the present era, it'll be remembered, with eyes aroll, as the field's Dork Ages, when reason was punitively proscribed in favor of Invincible Innocence. Archons will stoop to ANY tactic, to postpone that day indefinitely. Understandable. For them.

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