‡4 Ptolemist 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*-compiler. 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 enumerated thoroughly 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, Brandt, Zimmer, & Jones (henceforth 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_o 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, religiously incapable of admitting that any skeptic has ever made any indubitable contribution to knowledge. The most recent misfire (Brandt *et al* 2014B) is placed at §B into the context of decades of similar 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*, (
learns he's oops-unknowingly been connubially sleep
his fellow German spies would snuff him yesterday, if
So, he seeks advice from his wise mom, who consol
never even enter their heads that their own choice as
possibly ever commit the ultimate espionage blunder.

You are protected by the enormit

The point might be kept in mind by observers of th
history-of-ancient-astronomy field, whose most emine
ety — in tandem with a MacArthur Fellow and a H
propagandists — spread behind backs (fn 18 below)
person could suspect dishonesty of the history-of-anc
ancient astrologer Claudius Ptolemy, who 4-times-o
Sun that were undeniably but captive-journal-unprinta
280^y-old indoor tables than to the real Sun. The prime
profitable joke on academe and the public, **for consecu**
History of Astronomy and the American Astronomica
Historical Astronomy Division (HAD) — which we sl
Seemingly incredible fact of the last 4 decades of the
published defense of Ptolemy has ever been valid a
smart[5] or honest,[6] as we are about to see again&again
the perpetrators of this fantasy-literature are protected
that such ultra-eminent forums and scholars could se
stabbing, slanderous, & deceitful: fn 18.) The gulf
dumb arguments that are insisted-upon (by people so
as Rains) is so beyond the comprehension — the very
onlookers & pressfolk, that the latter have not, canno
what has been the dispute's history, even though oft m

[2] Prime smear against dissent is Insanity (as with m
establishment-polishers & darlings Gingerich (fn 16) & Mac
scientifically-challenged (‡2 fn 8) MacG even mirrorlessly cal
Newton a Velikovskian "crank and a con-man": www.dioi.o

[3] Nobody disputes the 50-to-1 indictment. But no Rep
newsrags, & toob) dares broadcast such heresy-supportive tr
policy: *hide it from the public*. (Given the power-secretarial s
not even a challenge. Consider: ***would archons behave as c***
even a 1% chance the press would expose it?) E.g., in 1983,
so insisted on (at-the-last-minute, without-warning) deletin
projected DR article, that the paper was suppressed by *JHA*,
original unexpurgated text at www.dioi.org/vols/w91.pdf, Ra
you the public just can't be trusted with certain central facts, b
start believing something Unapproved. (Similarly at www.di

[4] DR has asked AAS to monitor HAD's "unprofessional" (
2015/12/29, & (www.dioi.org/jcx6q.pdf, email) 2017/6/26 (r
of dishonest archonal smearing: www.dioi.org/pm1.htm, vs w
to *DIO* recipients, a class which includes the AAS, whose ch

[5] Some authors may be able, but this breed of apology
such feeble and comically self-contradictory (Rawlins 1992
challenge ("like shooting fishstories in a barrel of monkeys
‡10). And that is exactly why Ptolemists eschew (‡2 fn 52)
(www.dioi.org/deb.htm) or spoken (‡3 fn 5), preferring chara

[6] Ptolemists' integrity-level (e.g., §B6 below) generally sho
deficient original paper than in subsequent failure to acknow

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

A1 In 2011, DR belatedly[7] 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-shortly before the resulting paper Brandt *et al* 2014B went to press the *Journal of Astronomical History & Heritage*'s Editor Wayne Orchiston asked DR to referee it, though WO didn't mention that its progress was already 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] Ptolemists 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 *JHA*ish inability (increasingly typical of the whole ever-less-scientifically-skilled[8] 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-immune[9] 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 responsively 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 but to a fellow politician, who despite "careful" read
science or history — or anything else — and thus (
offered no scientific guidance at all (unless one delu
science), instead — even while acknowledging that
in the astronomical history involved" — insisting c
to his clique, adding gratuitous psychological evalua
"apparent need to disparage those with different viev
half-century repeatedly (and reliably-always behind-
differing from its own reality-detached view of Ptol
insanity which The Leader is now at the last almost[1
(publicly) loyal J.Evans, J.Brandt, & possibly B.Sch
doubters — their consensus not at all well-known —
though *JAHH*'s guardian ["referee"] is still stuck d
calling skepticism an extreme position: "worth heari
while continuing its suppression for a 4th straight dec
sentence to the paper, telling an editor who obvious
History-of-science's Archbishop of TruthBury: "If th
see this paper in it." When *JAHH* supinely granted hir
been determined that it was not going to appear in any
(& refs) are ever pretending not to be, so *JAHH*'s i
for an excuse for nonpublication was to find out if
accept[13] editorial revisions, would suffice to dodge p

A3 DR's reply, www.dioi.org/owu8q.pdf, tried [A
per's softening was in hopes of making DR go awa
assed chumminess with its mentor (who has loathe
[A] DR unexpectedly refused to enter into any argu
power to *JAHH*, instead of its 1st referee (as if ther
[B] The paper was expanded to provide information
asking, www.dioi.org/oww2u, *JAHH* to point out DR
neutral journal to choose a different referee. The *JAH*
ment? Just run away. *JAHH* went silent, even bloc
why *DIO* is distributing the present *DIO* issue, with
Nothing new about this: it's just copying the equally

[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.

[10] Gingerich's private ref-reports on DR's work can't resis
(Too remote from principled stands even to recognize one, O
DR *wants* to be shunned.) DR's atheism heaps extra aggra
Christian" of the Mennonite cult. Students of the psycholog
40^{y} obsession: bizarre details (& Cardinal Manning's perce

[11] As Gingerich again&again for decades has invariably
reports for allegedly scientific journals. Yet archons keep r
DR's character instead of his astronomy.

[12] One could add Swerdlow to the listlet of diehards, but

[13] In retrospect, it was predictable that *Journal of Astronom*
a paper showing its recent 2014 BZJ article was false in cla
Orchiston's demand for revision looked like the start of a
DR's requested self-censorship sufficiently adequate. (Th
the outset is verified by test in the next-last paragraph of
Since a durable cult lie (Hoskin to Thurston 1986/9/5) is th
DR cooperation with, e.g., *Polar Record* [Univ Cambridge
believed that this approach could kill the paper while never
the editor objected to parts or words (*he, not DR, knows his*
only have taken up DR's 2015/9/30 suggestion, at www.d
what competent fields' editors do & are for? — the job w
revision back for DR's OK; but, then, what if DR had replie

tantrum of the other non-US journal in this strange field: the *Journal for the History of Astronomy*, whose Editor Michael Hoskin, upon receipt of DR's constructive criticism of *JHA* refereeing of one of its papers, responded with threat and correspondence-cutoff. DR's criticism was supplemented by a polite and *admittedly valid* DR referee report: see the paper's recomputation at *JHA* 1984 June, which happened only because the author preferred honest and 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 L which was 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-archondum (§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 fleedom 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 p
the field's long-running pretend-obtuseness in failing
The present paper started by stepping on an Ozzie egg
Easter tradition of planting hidden Easter-eggs all ove
Well, this kid is ever entertained by an Easter-hunt's w
about us (§B), every one a squushable plant, indeed, r
of establishment insistence on tranforming a clums
scientist but a genius,[14] no less — a proposition as be
So we will next turn to enjoying the decades-long histo
pseudo-conviction (fn 50) that promotion of planted a
any more credible than adducing planted eggs to pro

Ptolemist archon: "you mean sh

B Those Skeptics Are the Crazy One
Refereeing at the *Journal for the Hist*

B1 Does it tell us something about the state of s
History of Astronomy, the "premier"[15] journal of its
science and deception in the shall-we-just-say Ext
the reputation of a fellow pseudo-scientist (and gra
Claudius Ptolemy? — known for centuries to inf
ous liar in astronomical history. *JHA*'s longtime #
teacher & deft Disraeliesque trowel-wielder Owen G
ancient faker and superstition-peddler[17] as "The Grea
ing that all who question this eminently-sensible-to-hi
to the simmering-if-generally-suppressed dispute inc
saleshype. We will first briefly examine a sample o
who disgraced themselves by eager enlistment in the
who knew that publishing their genuflections booste
dissent-burying archonbishops, thereby improving oc
lication, posts, grants — all with confidence that no m
arguments, they had nothing to fear from contradicti

B2 Ptolemy claimed to have outdoor-observed a
(*Almajest* 3.1) typically disagreed with the real Sun
which is over ***FIVE TIMES*** the angular distance from
this, while his report agreed with Hipparchos' indoor

[14] AAS-darling Gingerich 1980 p.264. Quoted at www.di

[15] Schaefer 2002 p.40.

[16] Gingerich 1976, Gingerich 2002. Disraeli (L.Strachey
p.244]): "when you come to royalty you should lay it on w
cinctly: *DIO 2.3* ‡6 §F. To enjoy Rob't Peary's like supplic

[17] Ptolemy authored astrology's bible, the *Tetrabiblos*, th
isn't) read if one wants a measure of how seriously to ta
1977 pp.70-71&79 for analysis of the book's escape-hatch
miscalculation (*ibid* p.69) which has escaped detection & th
in today's horoscopes, e.g., those on sale in our grocery-store

[18] Psychoanalyst Gingerich's intended-to-be-anonymous 2
2003X) called Ptolemy-skeptics just a tiny bunch of paranoid
smearing most of the scholars in the field, even WHILE he is
complaint that DR ***doesn't respect those who disagree w***
actually, yes. As we see from www.dioi.org/pm2.htm, a
DR's character (so relevant to the subject paper's mathe
Gingerich had forgotten he'd already revealed, at www.dioi
(On the reality of cohesive shunning of Ptolemy-skepticism

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 selected[22] 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 proferred 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

[19] The 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 1900y-old papyrus *P.Fouad* 267A, with solstice's time correct to ordmag 1h (actually to a fraction of 1h, 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 1h 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.

[21] For almost 40y, 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
tories' latitudes correct to ordmag 1′), could never h
investigation, had ancient scientists just unprogressiv
The cited clique's mass-slander of all ancient scien
believed among academics, who've no notion that
journals from learning that it is nothing but a wre
continuing pretense that indoor-cheater Ptolemy was
[d] The purely dreamt-up claim that it was standard p
to select outdoor data to fit indoor theory, merely
order to then turn around and defend Ptolemy as bein
literally-preposterous logic LEARNED FROM PT
agreeing with his theories, in order to then "prove
Shame-shame-shame on DR for accusing JHADsters

B4 Delambre 1817 had noted and Rawlins 1982C
failure of Ptolemy's 1025-star catalog (*Almajest* 7.5
6° above his horizon, indicating Hipparchos as the
Rhodos Island observatory (geographical latitude 3
Alexandria ($L = 31°12'$). So Schaefer 2001 con
another!) *JHA*-Pb-anti-RRN paper, that the catalog co
nonetheless because aerosols (atmospheric crud) bl
2002A's unanswerable responses: if this were the pr
dreds of stars in Hipparchos' *Commentary* would als
than Ptolemy's, but: ***it's the same* : END OF ANY R**
Schaefer know that? Simple: no *JHA* pseudo[24] ref
paper disputing Hipparchos' *Almajest*-catalog autho
Hipparchos *Comm*'s 100s of star-positions . His la
email claimed that no one could know anyway which
to. Which revealed he had no idea how Manitius 1
various phenomena, www.dioi.org/fff.htm#ngjm, to
biguously. And Duke 2002C p.33 cleverly noticed th
2001 had argued were Ptolemy's, 5 of these stars sha
with Hipparchos *Comm*, as Graßhoff 1990's brilliant
earlier. None of these definitive findings has caused

B5 It might seem suspicious to some that Ptolemy
planets, stars) has accurate mean longitude only fo
growing until it reached $-1°.1$ by Ptolemy's $+137$
1977's devastating new fractional-ending proof (sum
theft, uncomprehending loyalists kept insisting (e.g., C
an innocent solar error that infected everything else,
observed by him with his armillary astrolabe, unfortu
his zero point in celestial longitude λ. Until Rawlins 1
armillary astrolabe (*Almajest* 5.1 & 7.4) doesn't spin a
the equatorial pole; so an outdoor Ptolemy's longitu
$-1°.1$ would've caused the instrumental & actual ec
by $m \tan \epsilon = 29'$ (*idem* eq.2, where $\epsilon = 23°.7$, the
wave in celestial latitudes $\Delta\beta = 29' \cos \lambda$ and an er

[23] Ever-fertile Evans 1987 p.166 even argued that 6° of
southern view. The easily testable flaw in this alibi is explo

[24] Don't miss www.dioi.org/pm3.htm, longtime (1970-20
Astronomy Founder-Editor Michael Hoskin's efficiency: refe
Must be read to be believed. Lucky nobody will ever find it
Press" whose ever-advancing investigative impotency has
nearer the ultimate intimacy it aspires to: lapdancing a neec

to $\Delta\lambda = -29' \sin\lambda \tan\beta$, which gets substantial in the north. In the star catalog no such 1°/2-amplitude waves exist, so neither did an outdoor star-collecting Ptolemy. Of course, ever-openminded Ptolemists robo-countered this new shocker with their usual standard-weapon: scientific inability so truly embarrassing as to raise the question of whether impenetrable Ptolemism 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 (‡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 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 (‡2 §H1) blowing off a *63° degree phase-difference* with: "the phase is not exactly right" (Rawlins 1991W fn 152). All this to obscure that he can't find a cosine wave in the Catalog's zodiac stars, with amplitude even 1/3 as large as the 1°/2 he sought.

B6 In the 1987 *JHA*, Evans took his 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 Ptolemists could hope to accomplish among informed scholars. Still the case: e.g., Brandt *et al* 2014B.) Evans' main arguments:

[1] In 1981 Evans used an ancient cross-staff to measure the longitudinal distance of a star from the Moon outdoors, and his reduction showed the observational error was 2/3 of a degree (Evans 1987 p.275), meant to prove 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 his 1981 record ever since, hiding also from questions on the incident put by Rawlins and Thurston. (See ‡3 fn 11. Conman Frederick Cook ducked inquiry identically [Rawlins 2017A §B13]. Again: one can see why Evans is the ideal choice to carry on the *JHA* tradition of spotless integrity.) When later retelling the same argument (nearly-verbatim: Evans 1998 p.259), Evans conveniently forgets to discuss that 1981 eclipse at all (switching instead to adducing a previously unmentioned 1977 eclipse he hadn't outdoor-measured with) — and continues ineducably contending[25] for ancient errors of ordmag 1°.

[2] Ptolemy claimed (*Almajest* 7.4) to have outdoor-observed all 1025 stars of the *Almajest*'s Ancient Star Catalog. R.Newton 1977 proved that Ptolemy had instead stolen the catalog from Hipparchos by (as long suspected amo
1°/century precession onto Hipparchos' longitudes,
sion's falsity (actual precession then: 1°.38/cy) whic
0°.38/cy slippage, the fakes fell 1°.1 short of mean
Catalog would display large error-waves (details abo
with an armillary astrolabe mis-set by -1°.1 of cele
pressively far-fetched schemes to confuse this despe
spoofed above at ‡2 fn 11), repeatedly following clas
to the inherently unlikely and rejection of the likely.
apology flamed out when Gerd Graßhoff 1990 bril
DR had thought to do) for the mass-statistical corre
star-places, proving (as Alex Jones witnessed 1st-har
cataloger *Almajest*-editor G.Toomer's honest satisfac
plagiarized — thus vindicating Newton's & Rawlins'
Evans 1987, Schaefer 2001, & Schaefer 2002.

C Latest Into the Lists

C1 As to the ancient star-declinations issue: what
Well, both *Centaurus* (in 1982) and the *Journal of*
2014) received competing solutions to the *Almajest* 7
ably chose the partially inaccurate solution (featuring
while refusing to publish the expertly computed, co
because of its heresy in showing (§C5) Ptolemy fake
to acknowledge the content of subsequent communic

C2 Brandt *et al* 2014B is the most recent attempt
fact that some among *Almajest* 7.3's star-declination
(Brandt *et al* 2014B p.332) "unlikely to be a coincider
have been taken by Ptolemy himself." But said chro
probative, and the reader is deliberately (§C5) not to
which definitively contradict Ptolemy's observership
the *JAHH* & authors ere publication.

C3 Of *Alm* 7.3's 54 star-declinations δ reported
project examined 53: Timocharis 11 stars, Aristyllo
These data had already been studied by Pannekoek
R.Newton 1977; also Rawlins' 1982 bivariate least-sc
to publish, though this scrupulous 46pp paper was
requesting *Centaurus* publication. Moesgaard's ov
published instead a mathematically unsophisticated[2
(received at *Centaurus* a year later,[27] in 1983 June), v

[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 noncitation. 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***.

[26] Maeyama 1984 is graphical by trial&error and is mono
values for E, though with estimated (nonmathematically gu

[27] Maeyama 1984 p.308 acknowledges that he had seen
that, until noticing this, BZJ were in some doubt as to w
Brandt *et al* 2011) was really done then (perhaps supposi
guesswork not statistics), as suggested by their ultimate
that DR was specifically the establisher of the Timocharis-
by Neugebauer 1975 p.34), as well as of ***the whole con***
& latitude-errors from the data. Why else say (Brandt *e*
Timocharis-Aristyllos dichotomy? (In fact, Rawlins 1982
gave both astronomers' dates, explicitly on the basis of star
in the unpublished 1982 ms], adding the novel finding that al
showed that their observers knew their geographical latitude

Greek observers' epochs E were eyeballed not computed.

C4 Brandt *et al* 2014B attempted modest improvement & useful checks on previous work by bringing in modern satellite-determined data, and providing independent (if shaky) statistical indication of the separation of Timocharis & Aristyllos (the split 1st statistically proposed in Rawlins 1982G). DR was asked to referee the paper: *DIO*'s report, www.dioi.org/jau8q.pdf, is on the *DIO* website (as are our letters[28] 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 Ptolemist conclusion. (Though R.Newton is cited as a skeptic on Ptolemy, none of his or DR's damning evidence appears anywhere in the paper, and the reader will not even learn that DR doubts Ptolemy, much less what his reasons are. Surely an odd way to treat a conscientiously helpful referee.)

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

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

$$\delta = L \pm Z \tag{1}$$

(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
tacking $2^\circ 40'$ onto all Hipparchos' stellar longitud
the star-declinations were faked similarly.) No m
et al 2014B! — though urged by referee DR. It ha
a century that virtually the same date matches th
Catalog's fakes would seem correct, were they real
E.g., Peters & Knobel 1915 p.15 noted that +58 is
1025-star *Almajest* 7.5-8.1 catalog is correct. There
prior to DR's referee report, nor does such vital inf
subsequent to it, either. This positively belongs a
hear that besides his star-declinations (emph added
for his [(false) precession] value elsewhere in the A
— innocent of the A.Jones-witnessed fact that up
decades ago, Toomer agreed that the Catalog stars ca
irrelevant the faked "Ptolemy evidence" cited to To
asked: "So are we also to ascribe the Catalog to th
have created a catalog of over 1000 stars though no o
[D] For the δ data *Almajest* 7.3 gives for Ptolemy's
split (perhaps unknown before DR's 2011 letter an
suspect 6 star-declinations δ which Ptolemy analys
he doesn't analyse in *Almajest* 7.3) — if one adopts
(+159) and geographical latitude-error x ($+4'$) al
(Rawlins 1994L) upon the unsuspect data, without a
these 12 data, long-separately-recognized and separ
of this for Brandt *et al* 2014B's peculiar new split is

C6 *JAHH* readers *have a right to know* §*C5's f*
Wayne Orchiston (WO) has not felt the need to infor
the paper's Ptolemist conclusion. And of the *Journa*

C7 Brandt *et al* 2014B p.331 claims that its anal
analysis but (as warned in www.dioi.org/jau8q.pdf,
the paper) it is really[31] just a try (like Maeyama
monovariately. Except for Ptolemy (where differen
the resulting epochs E and their standard deviations
DR 1982, after the standard deviation of Aristyllos'
near-agreement with DR's recommendation, www
(compare www.dioi.org/bzj0.pdf, vs www.dioi.org/b
Brandt *et al* 2014B is that values for x & its standar
not even when BZJ attempts recounting and repeati
supplies and tabulates both x & σ_x). Instead, BZJ p
single entity, "accuracy", which they confusedly see

C8 The *DIO* referee report warned[33] that the "
2014B p.331 are "astonishingly low".

[30] The debate's existence is mentioned, but without the
& editor feel that their admirably full and neutral bibliog
dissent. But there is no excuse for silence in the text (whic
indicative items listed above at §C5.

[31] Language like that at Brandt *et al* 2014B p.331 makes
determined, the accuracy immediately follows."

[32] Slightly true also of Hipparchos, where Rawlins 198
declinations from non-*Almajest* sources, a supplement 1st

[33] BZJ were helpfully provided sufficient advice to inspir
answers for E, x, and both's standard deviations, [2] wer
looked remarkably too small, & [3] were repeatedly warne

C9 The erroneous[34] figures for "accuracy" rs in Brandt *et al* 2011 and Brandt *et al* 2014B were an ordmag too small, presumably because they were mistakenly found[35] (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 1000^{th} of a degree!)

C10 Interlude: From where did BZJ get §C9 [d]'s wacky idea that one should simply ***mean*** the residuals? Answer: from miscontruing p.283 of their preferred prior analysis, Maeyama 1984, where it is stated that (following determination of epoch E), "The epoch [where S is minimal] will be our first approximation. The resulting ***mean deviation*** at this epoch will then correspond to the mean systematic error[36] . . . ." Brandt *et al* 2014B mis-read this as referring to a simple *averaging* of leftover residuals. BZJ's procedure and cue from Maeyama 1984 are clear from Brandt *et al* 2011, th presumption is that rs will serve instead of x. But w σ_x — neither even mentioned by BZJ? While Maey off by serious factors, Brandt *et al* 2014B's miscon σ_x whatever.

C11 The invalidity of Brandt *et al* 2014B's proce by performing it in reverse: assume an E_o (instead o least-squares — then find rs by summing the resid flat zero! (A hint that gauging accuracy here require would be deemed zero. And any linear function in E the same result. This for any assumed E_o — so, by used for finding rs, we must conclude that all starti foregoing monovariate analysis) turn out to be error

C12 Moreover, any of those who've since 1982 t have remarked (had they done a valid 2-unknown s for the Greek observers is zero in all four cases. N cases where correlations happen to be tiny (e.g., Ti where $n = 19$ stars), one can come quite close (si *et al* 2014B's eq.1) to finding x's error σ_x through j

C13 In a true bivariate solution, e & x are leas figures given in the 1982 manuscript and in Rawli hand, incidentally. The later computerized solution [identical to ‡3 Table 2 above, except for Timochari

C14 Before 1982, no one had ever used these Greek astronomers' observatory-placements. Misle Ptolemy's *Geographical Directory* (*GD*) & the rigi of-science archons, that ancient Greeks were non- had long ago gotten the idea (persisting to the pres rulership: ‡1) that ancient geography was typified by why Rawlins 1982G — whose main analysis showed 3^{rd} century BC Alexandrian astronomers — emphas on stars: fn 27 above) to a History of science Socie bivariate least-squares had determined for the 1^{st} ti just how well Greek astronomers could know their course led on to the question of why the *GD*'s coor Rawlins 2008S). DR's papers have called L's error minus tester's assumed E_o); if the mutual solution i only e (thus E) and e's standard deviation σ_e, but x the single-datum standard deviation, σ_o. All these s

C15 If it seems odd that, previous to 1982, no on cient observatories' location (see Rawlins 1985G § even more revealing: in 36^{y} since 1982, **no one el** The two post-1982 papers both waste precious jour tograms, all to do the analyses inferiorly, e.g., mo minimal, S_o. (And the archons of history-of-anci capacity or right to judge, shun, condemn, & censo of scientists in such matters? Note the parallel to t

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[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^{y} 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: $E = -295$, $rs = -0^\circ.022$, $S = 2745'^2$.
[$E = -277\pm18^{\text{y}}$, $x = -0^\circ.076\pm0^\circ.077$, $S_m = 2441'^2$.]
Aristyllos 6 stars: $E = -258$, $rs = +0^\circ.003$, $S = 147'^2$.
[$E = -258\pm10^{\text{y}}$, $x = +0^\circ.016\pm0^\circ.045$, $S_m = 147'^2$.]
Hipparchos 17 stars (Alioth $\delta = 67^\circ 3/5$): $E = -128$, $rs = +0^\circ.004$, $S = 446'^2$.
[$E = -133\pm8^{\text{y}}$, $x = -0^\circ.001\pm0^\circ.021$, $S_m = 392'^2$.]
Ptolemy 18 stars $E = +111$ (Brandt *et al* 2014B Fig.5 no-prop-mot), $rs = -0^\circ.005$, $S = 2539'^2$.
[$E = +115\pm13^{\text{y}}$, $x = +0^\circ.004\pm0^\circ.052$, $S_m = 2521'^2$.]

[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 1^{st} monovariate test were to be fed into the 2^{nd} 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_o, the mean error of a single observation. The resultant errors range as high a factor of nearly 7 (the Hipparchan 44-star sample).

[37] Note problems at fn 42 & esp. fn 45 if done otherw minimize the squares of the residuals by finding the e th etc, whittling S into ever-smaller remoteness from min cumbersome, tediously-iterative serial-monovariate appro at-a-swoop (with trivial iterativity from non-linearity), with bivariate least-squares — as was done back in 1982&1994

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

Obsrvr	$E \pm \sigma_E$	Adop L	Its Error x	Actual $L \pm \sigma_L$	σ_o	σ_r
Timoch	-302 ± 08^y	$31°12'$	$+1'.5\pm1'.9$	$31°10'.5\pm1'.9$	$\pm6'.1$	$\pm5'.9$
Aristyll	-258 ± 10^y	$31°15'$	$+1'.0\pm2'.7$	$31°14'.0\pm2'.7$	$\pm6'.1$	$\pm4'.2$
Hipp	-131 ± 05^y	$36°08'$	$+0'.2\pm1'.2$	$36°07'.8\pm1'.2$	$\pm5'.2$	$\pm5'.0$
Anon	$+159\pm09^y$	$31°15'$	$+4'.4\pm2'.0$	$31°10'.6\pm2'.0$	$\pm6'.0$	$\pm5'.6$

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 nulls[38] 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 rounding[39] removed (σ_r). All these desiderata are produced here in Table 1, slightly[40] 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 various[41] 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 $81°1/15$, recorded (conventionally for unit-fractions) as $81°15'$, but later misrecognized (like ‡3 fn 44) as $81°$ & 15 arcmin, thus $\delta = 8°3/4$, as at *Almajest* 7.5. Thus, reconstructed true $\delta = 8°14/15$ or $8°56'$, 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 $\delta = -5°$), 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 previousl
The p.332 grouping is E (Early 6 stars) and L (La
traditional split, namely: the "SickSix" stellar de
his false precession from) versus the "Clean Dozen
sional math ignored): "our groupings have no sin
six stars" (Brandt *et al* 2014B p.334). Why? We
argues that Ptolemy typically fabricated the SickS
equally-typically then "prove" said precession fro
grouping would undercut this view by fracturing the
(as noted in DR's 2011/3/15 letter to Brandt): the t
own. Further, DR's 1994-adopted $+159$ epoch (Rav
stars with *no deletions*) was understood in 2011 for
overlap (fn 7) in the Clean-Dozen-vs-SickSix split,
2014B, which keeps mis-rendering DR's also-uncite
www.dioi.org/bjr3g.pdf, www.dioi.org/jau8q.pdf, p
et al 2014B's Figs.7&8 (C−O),[43] star-residuals' pro
E.g., the residuals of Alioth and Aldebaran are $18'$
of Brandt *et al* 2014B's Abstract, Table 2, and pp
other in $+159$. Indeed, as DR's 2014/8/26 ref rep
Clean Dozen and run a bivariate least-squares on th
is $E = +159\pm9^y$, $x = +4'\pm2'$ ($L = 31°11'\pm2'$: A
are symmetrically within $10'$ of zero: Betelgeux +
may examine the results of applying, to the Sick sta

actually higher than that of the non-eliminated stars Alta
published version (p.332) expanded the justification for eli
zero error occurred later than 200 AD (a criterion elimina
it was nonetheless retained as a member of group L) wh
those small-declination-speed stars — though the date is t
Betelgeux, DR was guilty of a similar mis-step in 1982 [r
which was cleared up in 2 stages, in 1994 and — thanks
2011. But this was explained in DR's 2011 letter to Brand
mistaken claim (p.334) that one of the SickSix stars is amo
of the paper's two versions of grouping) — and, in 2014's *I*
we guessed (though not even told the membership of gr
three of six E stars were Sickies should read four of six.
Capella, Spica, Alcaid) — again, for either version of gro
publication. Is this odd slip vestigial of an early trial-v
sample-forming's shopping-around period (similar to the s
2009S §K6; and don't miss fn 22) — before settlement up
hint of arbitrariness is one of the factors vitiating the paper
groupings of stars different from the skeptics' simple acce
final paper, no correction occurred for the above-cited re
from BZJ's groups L&E, though, again, it was moving fas
the selection of the Unhelpful Threesome was published u
of groupings [in Fig.10] restored Aldebaran while booting
errors is the general misconception that slow declination
Brandt *et al* 2014B believes it is looking for latitude "accu
least-flexibly measure latitude-error. Real bivariate inves
that getting E exactly right cannot occur without simulta
correlations are non-zero, and some are non-trivial.

[43] But labelled O−C. The various Brandt *et al* 2014B
Calculated) with C−O (evidently a routine weakness an
fortunately harmless], e.g., ‡2 §F8, & www.dioi.org/fff.ht
(p.331) is founded upon a confusion of errors with resi
the Rawlins 1982 ms' eq.2), thereby equating Observed-r
Calculated. If taken seriously, this makes Observed equal

Clean Dozen, eliminating ever-problematic[44] Arcturus — leaving a consistent set we might as well call the "SickFive" — the resulting (unweighted) residuals are mostly about 1/2 degree, the smallest[45] 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*

[44] To 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.

[45] If we re-check the residuals via monovariate test for L at Ptolemy's alleged observation-year, $+137$, instead of $+159$, we find the Clean Dozen more poorly fitting, residuals ranging from $-8'1/2$ (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_o ($6'$ 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 $L = 31°15'$ (see §C16 above, & Rawlins 1994L §F8), which is 3 nautical mi north of Alexandria's $L = 31°12'$, whereas the $+159$ bivariate solution $x = 4'$ closely reflects the $+3'$ error in the observer's overlarge assumed $L = 31°1/4$, and so is effectively right-on: $L = 31°11' \pm 2'$ (*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.

[46] See at www.dioi.org/vols/we0.pdf, *DIO 14* ‡1 §J2 & ‡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 (jus
difference of ***most of a century*** in crossing-times.
8 decades too early (21 BC: p.335) for "verifying" t
earlier in the paper at p.332 as $+57$), Pollux's track
near the pre-desired date.

C21 A peculiarity related to the question of ac
epoch E: Brandt *et al* 2014B *repeatedly* ignores (
referee report, www.dioi.org/jau8q.pdf, to correct
1994L's date (for Anonymous' Clean Dozen stars
Table 3's actual published value, $+159$. This is a
saw at §C17 & fnn 42&45. (The misprinted epoch
date for *Hipparchos* — not Ptolemy's Anonymous –
Importantly, $+159$ is an epoch which, for the 1st
(§C17) the Clean-Dozen-vs-SickSix split (by contra
epochs E that were flirted-with in the article or the re
jettisons as needless (fn 46) Brandt *et al* 2014B's elab
(See fn 42 above, for the advantages of adopting wh

C22 A shock that might give historians-of-scien
motions of the myth of observer Ptolemy: *the d
competence over 30y ago*, by R.Newton & D.Raw
(*Centaurus* & *JAHH*) have published on the issue si
discovering nothing new that's valid. Indeed, as
(p.47), R.Newton would judge these efforts primaril
knowledge". Which anyone could've foreseen by
got deeply involved in graphical solutions by trial&
the former as the prime prior research!

C23 As a final quietus to *JAHH*'s monucementa
show how easily a scrupulous journal could have c
for x were correct. All *JAHH* needed to do is: va
their residual-squares-sum S was minimal, i.e., equ
square of the appropriate σ_o in Table 1, multiplied
Or: for any of the four ancient astronomer's star-re
x for that astronomer, and then [2] just re-run Bra
him. BZJ will then encounter a sorta-pleasant sur
S, the sum of residuals-squared, will be found to
showing that Brandt *et al* 2014B's S generally (exc
optimal (extremal) solutions S_m. (Due to low correl
they show that true bivariate procedures were not ap
better solution being found by crude means (fn 36),
at the lowest S possible. This goal can, however, be
least-squares (as in Rawlins 1994L), which efficient
is a minimum. If BZJ have any doubts that *DIO* ha
need only conduct the very same test, using our *e*&
own. Using the slightly improved values (vs the r
cannot be decreased further (more than micro-trivi
Our solutions for x are just 1-4 arcmin. The size

[47] DR's latitude-errors x for Timocharis, Aristyllos, & H
for all to check, at Rawlins 1994L pp.44-46 & Table 3 –
which Brandt *et al* 2014B p.331 acknowledges access. The
1994L's 19 stars (vs BZJ's 17) for Hipparchos & 12 stars
appropriate for BZJ's sampling was provided at pp.3-4 of
report. (Due to minuscule differences in adopted star-place
may not be super-precisely identical to *DIO*'s; but further

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 unprincipled[48] — sometimes (e.g., fn 18) even vicious — stubbornness[49] of those determined to protect Ptolemy from public exposure by any means (www.dioi.org/mot.htm#xcfp) is that they are not protecting him but themselves and/or their gooroos — 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 §M: "***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.)
D3 Being politicians, Ptolemist archons are the sort of people whose idea of intellectual engagement tends (for obvious reasons) not towards weighing scientific arguments but to [1] slandering (‡2 fn 5) their opposites as fools, knaves, and nuts (before discussing evidence — if ever doing so at all), while [2] pointing[50] 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 Disrespectful[51]

[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 Ptolemists (like BZJ) still exist: "neither side [is] able to produce decisive evidence to convince the other side." (But one thing we can be sure of: inserting such archon-comforting & gratuitous irrelevancy is sure to get a paper published at *JHA*.) One trusts that 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]) o
ature (especially *DIO*'s), revealing defenders' fatal
of *JHA*'s cringing Editor Evans' 1987 parallax screw
record (§B6 item [1]) will ever appear in the irredee
this now-conscious deceit is a required&essential pa
is the Pb-paper-prominent "empirical" centerpiece
to-eternity tenet that huge, Ptolemy-sized observat
Further, no mention is allowed of definitive eviden
demic decency (such censorship constituting just one
by censorship!): evidence-hiding (as just noted), dat
amples), slanderous lies (§B1; ‡3 fn 5), thereby imp
the awful hidden truth, namely, that the *entire* Belie
has actually long since become ***no side at all*** (as
no coherent case whatever, thus resorting to tactic
embarrassing coulda→musta alibi-scenarios. (Deep
D5 Such childishly obvious illusionism, as delin
the non-specialist part of academe, as well as the *in*
seminumerate) "science" press, and is the key to the e
of-science — knowingly careless of concomitant hu
ancient ***history*** — that archons have not-either been p
if just a few of puppeteer Gingerich's claque can fo
keep publishing contrived even-if-laughably-transpa
ever-befuddlable lapdog press-corps, then the prime

Not a single Ptolemy-defense archon w

(Gerald Toomer the admirable rule-proving exceptio
To normal folk, this may seem a puzzling, feeble, e
those who thrive (& fiscally survive) on a vanity of
antithesis of the scientific attitude of inquiry, and of
D6 Beyond Ptolemists' lack of science's attitude
ally every member of their clique, whatever his emi
relevant to the Ptolemy controversy. Non-specialist
technical details (or too busy to take the time) — are
too-oft-network-granted posts, awards, university c
diverted from the seemingly obvious point that just
doesn't mean that he knows much about *positional*
which are the specialties one needs for research in
planetary astronomy, & spectral analysis are worthl
ties of such astronomers as R.Newton and *DIO*'s M
expertise shows up in the truly historic fumblings
above in §B and the especially comedic do-not-miss
the serial-learning-experience stats (§C8) of *JAHH*'s
D7 Concluding: we anticipate that (unless m
www.dioi.org/jau8q.pdf) Ptolemist archons are al
to Brandt's many awards&posts, as if relevant, pro
has, after reviewing the evidence, decided to help
out for Ptolemy, hoping no-one will notice the §A
discordant-evidence-sterilized Brandt *et al* 2014B's

Slippers.

[52] In mathematical history-of-astronomy, dimbulbs + car
majority. If able, honest scientists ever rejoin&review the
aroll, as the field's Dork Ages, when reason was punitively
Archons will stoop to ANY tactic, to postpone that day inc

Almajest. Compiled Ptolemy c.160 AD. Eds: Manitius 1912-3; Toomer 1984.
B&J = J.L.Berggren & A.Jones 2000. *Ptolemy's Geography*, Princeton.
J.Brandt *et al* 2011. BullAmerAstronSoc *43*:#129.02.
J.Brandt *et al* 2014A. *DIO*-refereed 2014/7/8 paper; too-soon became Brandt *et al* 2014B.
J.Brandt *et al* 2014B. Journal of Astronomical History & Heritage *17.3*:326.
J.Delambre 1817. *Histoire de l'Astronomie Ancienne*, Paris.
Dennis Duke 2002C. DIO *12*:28.
Dennis Duke 2005T. Centaurus *47*:163.
J.Evans 1987. JHA *18*:155 & 233.
J.Evans 1998. *History & Practice of Ancient Astronomy*, Oxford U.
GD = *Geographical Directory*. Ptolemy c.160 AD. B&J. Complete eds: Nobbe; S&G.
O.Gingerich 1976. Science *193*:476.
O.Gingerich 1990. JHA *21*:364. Review of R.Newton 1982.
O.Gingerich 2002. Isis *93.1*:70.
Gerd Graßhoff 1990. *History of Ptolemy's Star Catalogue*, NYC.
Hipparchos. *Commentary on Aratos & Eudoxos* c.130 BC. Ed: Manitius, Leipzig 1894.
Y.Maeyama 1984. Centaurus *27*:280.
Karl Manitius 1912-3, Ed. *Handbuch der Astronomie* [*Almajest*], Leipzig.
R.Newton 1977. *Crime of Claudius Ptolemy*, Johns Hopkins U.
R.Newton 1991. DIO *1.1* ‡5.
C.Nobbe 1843-5. *Claudii Ptolemaii Geographia*, Leipzig. Repr 1966, pref A.Diller.
A.Pannekoek 1955. Vistas in Astronomy *1*:60.
PK = C.Peters & E.Knobel 1915. *Ptolemy's Catalogue of Stars*, Carnegie Inst., Publ.#86.
Keith Pickering 2002A. DIO *12*:3.
D.Rawlins 1977. Skeptical Inquirer *2.1*:62.
D.Rawlins 1982C. Publications of the Astronomical Society of the Pacific *94*:359.
D.Rawlins 1982G. Isis *73*:259.
D.Rawlins 1985G. Vistas in Astronomy *28*:255.
D.Rawlins 1991W. DIO&Journal for Hysterical Astronomy *1.2-3* ‡9.
D.Rawlins 1992V. DIO *2.3* ‡8.
D.Rawlins 1994L. DIO *4.1* ‡3.
D.Rawlins 1994R. DIO *4.3* ‡14.
D.Rawlins 1994S. DIO *4.3* ‡15.
D.Rawlins 1999. DIO *9.1* ‡3. (Accepted JHA 1981, but suppressed by livid M.Hoskin.)
D.Rawlins 2003X. Isis *93.3*:500.
D.Rawlins 2008R. DIO *14* ‡2.
D.Rawlins 2008S. DIO *14* ‡3.
D.Rawlins 2009E. DIO&Journal for Hysterical Astronomy *16* ‡1.
D.Rawlins 2009S. DIO&Journal for Hysterical Astronomy *16* ‡3.
D.Rawlins 2017A. DIO&Journal for Hysterical Astronomy *21* ‡3.
D.Rawlins 2018U. DIO *20* ‡2.
B.Schaefer 2001. JHA *32*:1.
B.Schaefer 2002. Sky&Tel *103.2*:38.
B.Schaefer 2013. JHA *44*:47.
ScAm 1979. Scientific American *240.3*:90. Commissioned by ScAm Ed. D.Flanagan.
Strabo. *Geography* c.20 AD. Ed: Horace Jones, LCL 1917-1932.
S&G = A.Stückelberger & G.Graßhoff 2006. *Ptolemaios Handbuch Geographie*, U.Bern.
Suda Lexicon. Compiled c.1000 AD. Ed: Ada Adler, Leipzig 1928-1938.
Tetrabiblos. Compiled Ptolemy c.160 AD. Ed: Frank Robbins, LCL 1940.
Hugh Thurston 1995. JHA *26*:164.
Hugh Thurston 2002S. Isis *93.1*:58.
Gerald Toomer 1984, Ed. *Ptolemy's Almagest*, NYC.
Peter Zimmer *et al* 2013. AmerAstronSocAbstracts *221*:#130.01.