Ancient Spherical Trig:
Journal for History of Astronomy Muff Solves Ancient Regulus Misplacement: Hipparchos-Evans Parallax Sign Error

Diller Verified on Klimata After 75¹ Shunning & Mob Hits

JHA’s Subtraction from the Sum of Human Knowledge

DR to Muffia: Is 14-out-of-14 Enuffa?

Pytheas Observatory Located
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History vs Unfalsifiability: Crock of Ages Clefted But JHA-Xerxesed
DIO’s rise in prominence (e.g., NYTtimes 2009/9/8 Science) has only fanned the cowering Journal for the History of Astronomy’s eternal loathing (www.dioi.org/qqq.htm#sdmh, or [3 fn 56 here]. When no [a] shunning DR or DIO, JHA runs articles on him which are reliably [b] destructive & [c] scientifically self-destructing, e.g., J.Evans 1987, B.Shaefer 2001. Article [3 here micro-shreds a JHA attack that achieves [a][b][c] all at once. As noted & developed in DIO 4.2 pp.55-57 (1994), leading classicist A.Diller in 1934 showed in the eminent journal Klio that Strabo’s Hipparchos klimatea were consistent with sph trig computations, using accurate obliquity 23°/3. How have history-of-astronomy’s “Muffia” & JHA cults thanked Diller for one of the great contributions to the history of math? Muffia founder O.Neugebauer abusively attacked Diller for 40°. ON’s competing pseudo-solution was long sacred though (Table 1) it fits only 6 of the 14 Hipparchos klimata, while the proscribed Diller-DR scheme has always fit virtually (now exactly) ALL 14 data at issue; yet M’kteeres for decades (1934-2002) kept certifying ON’s seemingly ageless crock. Finally, in 2002, just as Isis honored Diller-DR’s proof with publication ([3 fn 23), the 68° Muffia-dann broke: A.Jones’ JHA paper dumped Neugebauer’s folly. But the same paper (oft called “MujaHHA” within Xerxesially insta-replaces it with a new crotch ([3 fn 66 here), trying to weasel (DIO 11.3 §76 p.70) out of crediting Diller’s ever-more-obvious success. The last miniblock to the totality of Diller’s victory collapsed in 2009 (on April 1, aptly), when the only datum hitherto seen as not fitting Diller’s theory was found to do so after all (see here at [3 eq.3]) — and his hit-score became 14 of 14: on the nose in every case.

The Conservative Aspects of DIO’s Triple Eclipse-Induction
DIO’s solutions (www.dioi.org/cot.htm#jgskj of all previously unsoled ancient solar speeds use attested, normal ancient methodology & produce precisely all 6 attested 4-digit integers: 24 digits (no other theory does either: www.dioi.org/thr.htm#xzpt), assuming Greek-Zelukid use of now-lost 13th century BC eclipses. Finding no mismath or alternate eclipses to complain of, Muffiosi just scoff at data-remoteness. But neutral experts’ dates for Babylon observations’ start dovetail with DIO’s theory; Isis 83:474 (1992): c.1350 BC.

1 The NYTtimes article (link to full version: www.dioi.org/cot.htm#fxq) notes that DIO opponents display a lamentably common mental insenepatability. Cooperatively proving the point: during our 19th of existence, the publishers of the JHA & DIO (top UK & US history of astronomy journals; [3 fn 56 here) have never communicated, despite urgings at, e.g., DIO 1.3 fn 269, DIO 4.2 §3 §E3, DIO 11.2 p.30. (Rational, pacific discourse shows who’s right&numerate, so: why would archons tolerate peace?)
2 DIO both criticizes&praises JHA-Muffia output. Latter: §1 head, §§E7&fn 55&56, DIO 6 §3 fn 768. Shirt-unstuffings are entirely reactive to truth-warps by usual establishment anti-rebel ploys: money, shunning, money, censorship, money, kept “experts”, money, goues, money, threats, money.
3 E.g., 1991&1994 (see DIO 4.2 p.55 & fn 2), as well as J.Britton by phone (c.2000). No Muffioso has yet faced the Diller-DR theory’s suble-as-a-ton-of-hits preferablearity. History of astronomy forums (by contrast to Isis printing Thurston 2002) [vs now! DIO 22 §1 2018] won’t even cite the perfect fit Diller-DR have achieved. Britton&Jones just rank archon-authority above statistics! Muffthink lives.
4 We usually call this unrefered paper “MuHHA” to stress the Muffia-JHA cult-rooted cementsial here, which has never been any one scholar’s responsibility. Extra weirdness: MuHHA concealed (with-out citing DIO) most of the central points asserted in 1994’s DIO 4.2 (p.56 Table 1); of the central points noted [3 fn 23], [i] Neugebauer’s competing scheme was invalid. [ii] Hipparchos used sph trig; but the JHA then pretended that Diller didn’t prove this important contribution to math history, instead acting as if the Muffia-JHA gang was doing so itself! — this, after 68° of Muffia denial (e.g., Neugebauer 1975 p.337; DIO 7.1 §2) of the same proposition. (Similar side-switch-claim=jump: www.dioi.org/fff.htm#eqw.)
5 Which parallels our 2001 star-dating the Great Pyramid to c.2600 BC (Nature 412:699). This we’d known was a conventional figure; but DIO was unaware of the 1350 BC date when researching ancient lunar theory, so our 13th century BC results constitute an entirely independent multiple verification.

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Editor[2006-2012]: Dennis Duke, FSU. Publisher: Dennis Rawlins (DR), address above.

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Defly or dafly crafted reports, on apt candidates for recognition in our occasional satirical J. for Hysterial Astronomy will of course also be considered for publication.

Free spirits will presumably be pleased (and certain archons will not be surprised) to learn that: at DIO, there is not the slightest fixed standard for writing style.

Contributors should send (expendable photocopies of) papers to one of the following referees — and then inquire of him by phone in 40 days:

Dennis Duke [ancient astronomy, data analysis], Physics Dep’t, Florida State University Tallahassee, FL 32306-4052, tel 850-644-0175.

Robert Headland [polar research & exploration], Scott Polar Research Institute, University of Cambridge, Lensfield Road, Cambridge CB2 1ER, UK; tel (44) 1223-336540.

Charles Kowal [celestial discovery, asteroids], Johns Hopkins University Applied Physics Laboratory, Johns Hopkins Road, Laurel, MD 20707. Retired. Tel 360-985-0492.

Keith Pickering [navigation, exploration, computers, photography, science ethics], 10085 County Road 24, Watertown, MN 55388.

E. Myles Standish [positional & dynamical astronomy], Jet Propulsion Laboratory 301-150, Cal Tech, 4800 Oak Grove Drive, Pasadena, CA 91109-8099. Ret. Tel 864-888-1301.

F. Richard Stephenson [ancient eclipses, ΔT secular behavior], Department of Physics, University of Durham, Durham DH1 3LE, UK; tel (44) 191-374-2153.

Christopher B. F. Walker [Mesopotamian astronomy], Dep’t of Western Asiatic Antiquities, British Museum, Great Russell Street, London WC1B 3DG, UK; tel (44) 171-323-8382.

G Shorts

G1 Girth-Wisdom. Ever heard of an enormously fat person getting shot in a duel?

G2 Dueling academics’ fear of cult or math mis-steps has 2 phases: [a] timidity ere risk slips occasionally attending exploration; [b] post-error shame. Fear #1 can cost discoveries; #2, integrity. (See §4 fn 28; §3 §E4, fn 12 & §5: www.dioi.org/hiv.htm#idc.)

G3 Needlessly-Divisive PC At War With Itself. [a] To PCers, “native American” implies: realer (North) American than a US-born WASP. [b] But in such also-laboriously-multisyllabic stroking-terms as “Italian-American”, “Serbian-American”, etc, the 2\th word implies that, though a descendant of immigrants, one is equal to a US-born WASP. But: aren’t these two ethnic-politics evergreens actually rather forked-tongue-contradictory?

G4 Morally-Superior? Or Navigationally-Challenged? Whites&blacks have been slavers throughout history. Yet the central implicit thesis of white-guilt-flogging, all-is-race-hate “historians” is that blacks couldn’t have shipped sardine-crowded slaves to the Americas in chains&filth, just as profitably&crueelly as whites did. But this view is itself flagrantly racist. The unfaced sole reason blacks didn’t ship slaves here is that, at that time: blacks didn’t know how to navigate ships across the Atlantic.

G5 Civil Rights & Civility Rights. When civil-rights “progress” is Mediumly boasted of, it is invariably, narrowly measured entirely in terms of ethnic-diversity stats achieved, but never in terms of whether general society has thereby improved in net civility, culture, safety, drugfree schools, happiness, intellectual diversity, and free speech: DIO 4.2 §9 G.

G6 Gov’t by 535 Messalinas. OK, so it’s hollerin’-good-showbiz for TV ‘snews spat-takeout in 3 Worst Ref-Star Longitudes & 3 Eclipses His 3 Worst Ref-Star Longitudes & 3 Eclipses:

Century BC Spherical Trig, But No Equation of Time

Needlessly-Divisive PC At War With Itself. [a] To PCers, “native American” implies: realer (North) American than a US-born WASP. [b] But in such also-laboriously-multisyllabic stroking-terms as “Italian-American”, “Serbian-American”, etc, the 2\th word implies that, though a descendant of immigrants, one is equal to a US-born WASP. But: aren’t these two ethnic-politics evergreens actually rather forked-tongue-contradictory?

A Klan-Klod-Klue

A1 Among the numerous 1 gymnastic hysteric-astronomy pratfalls enlivening JHA’s hefty (64 pp) James Evans 1987 double-lead-paper attack 2 upon (then-minority) Ptolemy-doubters was [JHA Editor-to-be] Evans’ lordly illustration of their dumb overestimation of ancient ocular accuracy. To illustrate his point Evans 1987 n.50 (p.275) presents his own non-telescopic (cross-staff) 1981/7/16 Seattle observational determination of the longitude of a star (λ Sgr) by using a lunar eclipse (as Hipparchos had) — which after Evans’ reduction produced a longitude erroneous by ~2°/3, thus according to him (idem) showing that the huge errors in some ancient observations were so ordinary that such were a poor basis for learning anything about ancient science. As further examples, Evans specifically mentions (idem & p.235) Hipparchos’ two hugely disparate Spica data (explained below: §B) which disagree by over 1°. He then draws for us a Muffioso lesson (emph added): “No better demonstration could be wished of the uncertainty attached to the method” of fixing stars’ longitudes by eclipses. However, when instructor Evans repeats the very same sermon (on Hipparchos’ eclipse-star errors) 1\l later at Evans 1998 p.259 (“This shows the size of the possible errors in ancient measurements of absolute star longitudes”), he slyly deletes mention of his formerly prominent 1981 eclipse-star measures — which shows that (during the 1987-1998 interim) Evans had read Rawlins 1991W fn 288 (below: §A2) and therefore learned that Evans’ and Hipparchos’ errors were NOT OF MEASUREMENT BUT OF BASIC SPHERICAL-ASTRONOMY MATHEMATICS (an embarrassment explained below at §A2). I.e., when ineducable educator Evans 3\l hand evidence somsaults, he just pretended he was right anyway, unable to admit DIO scored & “premier” JHA bellyopped: standard JHA honesty and inquiring empiricism. All of which sets up an irony whose fruitful blossoming is the present paper’s main subject.

A2 As just noted: said irony’s core was revealed 1\l in Rawlins 1991W fn 288. Contra Evans, neither his own nor Hipparchos’ problems were observational. Both simply mis-computed the reduction of valid observational data by using invalid math: the wrong sign for their parallax corrections. For the 1981 Evans case, at mid-eclipse, the longitudinal lunar parallax pλ was virtually 1°/3. So Evans’ sign-confusion created a huge net error because, whereas longitude parallax pλ (the difference between toptocentric [observer-centered]: outdoor-visible] and geocentric [indoortabular] longitude: eq.2) is obviously supposed to be ADDED when converting a calculated geocentric lunar longitude into a toptocentric (observer-centered) longitude, Evans instead SUBTRACTED it as if reducing the size of the possible errors in ancient measurements of absolute star longitudes”).

A3 The US, whose power made English the world language, now has a lower percentage of English-speaking inhabitants than Denmark, Holland, Sweden, etc. The media responsible for this transformational achievement are so modest, they never even mention it. See also DIO 3 §L8 & fn 95-97, and DIO 4.1 §5 §A. Funnier yet: www.dioi.org/cot.htm#scsp.

1. Johnny Carson’s eulogy for Abe Lincoln: “without whom, we would not have the dunk-stuff.”
2. Typical of the modern Ptolemy salescorps, JHA Assoc.Ed Evans makes his attacks on DR only in captive arenas (safe there from reply or debate), while never citing any external source correcting his mis-science. Similar integrity: §3 fn 56. On error-admission fear: §4 §G2; www.dioi.org/mot.htm#jfrgs.
3. Rawlins 1991W’s math has been verified in detail by Hugh Tharston and John Britton. We thank both for an arduous, specialized task.
an outdoor topocentric observation to find geocentric longitude. Thus the sign mixup would naturally cause an error of about $-2^{2}/3$ or $-40^\circ$ — and the laughably impossible "observational" longitude he reports is indeed (Evans 1987 p.275 n.50): "too small by about 40°". (Typically, Evans has had no comment since, despite DR [South Bend, IN, 1997 June, face-to-face] and Hugh Thurston [by letter] gently bringing the matter to his attention.) After correcting for this Muff, we can verify the admirable smallness of the 1981 observational error of Evans (a dedicated student of ancient instruments and possessor of a steady hand, since the cross-staff requires it): merely one or two arcmin — just the sort of accuracy DR consistently ascribed to the best ancient-naked-eye observations.

A3 Only a scholarly cat-er to modern Hist.astron's cult-klan could straightforwardly pro-ceed to think that the looser angularity $2^{2}/3$ — nearly triple the lunar semi-diameter $x$, a saintly orificed $z$ observational and so by implication excuses the tight adherence of Ptolemy’s "observations" to indoor-calculations (i.e., frauds) while disagreeing hugely with the outdoor sky.

A4 Note that a major member of Ptolemy’s faked “observations” (Almagest 5.12-13) is also off by $2^{2}/3$. (See discussion at R.Newton 1977 pp.182-191. Also DIO 8 §1 fn 13.) Scribbling a drawing will give one idea of how ridiculous this is: mislocating a disk so grossly that the really and theoretical disks ($1^{2}/2$ wide in these lunar cases) don’t even come close to overlapping, the very feat Evans misclaimed he’d personally achieved in 1981 and is now too embarrassed and too steeped in Muffia academic integrity to retract.

A5 NB: After the three-fold (§A6) [now four-fold (§F3)] collapse of Evans’ implicit alibis (Hipparchos’ eclipse-stars & his own: §A1) for Ptolemy’s huge “observational” errors, the Muffia of course hasn’t abandoned its support [see §A1 sermon] for the same-old Ptolemy-worship the alibis were designed for. (Which figures, since evidence has little relation to that cult’s belief-system.) It hasn’t occurred to Mufosis (whose strong points don’t include philosophy of science) to ponder a simple question: if devotion to our favorite positions keeps leading us into embarassing crackdown-level muddles (e.g., §A1 & DIO 2.3 §8 §§C10-C15), does this not suggest that said positions are less than completely secure?*

*Both inquirers were told by Evans that he would look into it. But he never communicated what he found. Except by implication: the deft Evans text-surgery cited at §A1 fn 7.

7E.g., Rawlins 1982G p.263 & n.17, Rawlins 1985G passim, & Rawlins 1985H.

8E.g., Evans’ Ptolemy’s Almagest 3.1&7 solar equinoctial solstice “observations” of the Sun, which agree 50 times better (Rawlins 1987 p.236) with his indoor tables than with the actual outdoor Sun. See Thurston on R.Newton at DIO 8 §1 §A.

7 Evans 1987 n.50’s misadmonishment (§A1) is repeated in his later book: Evans 1998 pp.256-259; but this (post-DIO 1.3 fn 286) Spica sermon quietly avoids discussion of his Seattle observation of the 1981-7116 eclipse in connexion (just photo at p.48, 100s of pages distant from his Hipparchos-Spica comments, shifting attention instead to the previously unadduced eclipse of 1977/6-3/4, seen from Spokane. [Why must Evans go back 21st for a “recent” (ibid p.256) eclipse [mildly reminiscent of a Ptolemy ploy: www.dioi.org/cot.htm#cKnh], considering that Evans 1987 went back merely 6$^a$ to find a usable eclipse? Implication: 1977 is ere 1981, and JE here has his signmanship OK at last, so: seeee, he knew how to do it all along. The catch: unlike at Evans 1987 n.50, no 1977 data are reported as outdoor-measured by Evans, though he repeatedly [Evans 1998 pp.256-257] speaks of “observations” or “observed.” So he knows he screwed up the 1981 eclipse’s parallax-sign, but CAN’T admit that (§A2) DIO corrected it for him. (Note contrast to, e.g., DIO 2 §4 fn 18 & DIO 11.2 cover.) Or admit the falsity of his alibi-for-silence-on-errors pretense (DIO 9.1 fn 2) of not reading DIO. [Had he faced reality on Regulus at Evans 1998 pp.250f, he could’ve made the present Regulus discovery himself. More wages of shunning.) For Evans’ citation-practice integrity, see §3 fn 24. (NB: This chauvinist lawyer-for-Ptolemy [www.dioi.org/cot.htm#Nusr&y#t3] is led headship-apparent to the JHA’s M.Hoskin, hist.astron’s own Lord Sommers [DIO 2.3 §1 fn 18]. Who’ll dispute the aptness?) Another corrosive consequence of a cult’s living with the shame of knowing that its sacred mission (hyping derivative Babylonian astronomy & Ptolemy as original genius) is unadmitably indefensible.

9Indeed, Muffia desperation to reject non-cult common-sense has now reached the point where the clique has even (presumably unknowingly) brought in Velikovsky-circlet experts to denigrate RN-DR work. During my 1995/22/26 chat with B. van Dalen, he mentioned that the reason he (generally wonderful) paper van Dalen 1994’s n.1 had cited the 1989 Fomenko et al paper (which, with...
E8 Voting Dumbo or Dembo = seeking an advantageous relationship with con-men.
E9 “Modern art” = an antique. (But newer than the other “Emperor’s New Clothes”.)
E10 “Progressive” 12-Tone music = regressive music, 29 times24 less free than tonal.
E11 Woody Shakespeare = Christopher Marlowe.25
E12 Evidence for ESP = fraud and/or bad statistics. Always.26
E13 “Mainstream” Press (§G7) = media owned by 1% of 1% of the public.
E14 “Fringe” Press = media owned by the other 99,999% of the public.
E15 Bunnyrabbit religion = kill us (§A8) or we take you over by sheer numbers.
E16 Montezuma’s Real Revenge = Border-Dysentery.
E17 California = “sanctuary-state” portside open scar in the US’ Titanic.
E18 “A Affirmative Action” = rich-owned-media-promoted divide&conquer enragement (of the lowly Alemindles) about every inequity-groan by the big one: rich’s-prep.
E19 999 = number of theories28 the press has entertained throughout its pseudosearch to explain its eternal pseudomystery of ethnic groups’ intractable differing success-histories.
E20 Prothonotary = Ultimately, Soviet-spys Alger Hiss’ 2nd-least-favorite warbler.
E21 Christian = worship Satan’s & thus evil’s creator. Which seques smoothly to:

F Religion & Atheism
F1 Do popular religions fight human cloning because they resent the competition?
F2 I’m outraged at Danish newspaper-publication of Moslem-insulting cartoons. The European press should be ashamed of itself — for not publishing said cartoons continuously&prominently throughout the last 30 years, to stimulate Moslem immigrants into revealing their hothead-intolerance (too-often violent, even murderous) early enough to warn Europe that its own tragic internal combine of cheap-labor-exploiting Christian capitalists and bleeding-heart socialists was about to import a virulent religious cancer into Europe, and so risk poisoning (perhaps indefinately) the most civilized region of the world.
F3 Organized Religion as Celebrity-Philosophy. Celebrity-obsession & churches are pop-culture substitutes for reality. Media commentators justly laugh at celebrities who’re famous for being famous. Why not a parallel observation that mass-religion is believed-in primarily for being believed-in? (See www.dioi.org/rel.htm#msj.) What other evidence is there?

of the medibiz which (while capable of grand scientific miracles) funds polls’ campaigns and has wasted a massive fraction of net national medical costs upon minimally-useful, counterproductive, or dangerous passing-fads (see Shaw’s Doctor’s Dilemma), e.g., radical mastectomies, radium treatments, etc.sclerosis, HRT, hysterectomies, CTscans, mammograms, x-irradiation in general, etc.

24Because 712 is about 28.9 times bigger than 12!: simple math, unmentioned in any musicologist’s discussion of what purports to be mathematical music. And, ah, where’s the connexion of (an arbitrary permutation-straitjacket) to music’s uplifting humanity? In Vienna’s Zentral Friedhof, 12-toner A.Schönberg is wisely planted far from the honored grave-grove of Beethoven, Schubert, & Brahms.

25See [www.dioi.org/sha.html], and the Marlovian cases of, e.g., C.Hoffman, S.Blumenfeld, R.Barber; & see [www.dioi.org/rel.htm#msj.] the Shakespeare industry has parallels to that of professional Babylon&Ptolemy-astronomy hustlers: non-citation-and-or insult of opponents replaces logical argument; non-mention that most eminent experts disbelieve the Industry view (e.g., Hawthorne, H.James, Whitman, & Twain knew businessman Shakespeare was a front); hilariously glass-house ritual-claims that all skeptical induction is speculation. (Analogously mirrorless Muffa: www.dioi.org/th/hsj.htm.)

26ESP is as ridiculous as seeing with your nose or hearing with your tongue, but most ESP-brained hopefuls know little stats and (DIO 2.7 §1 §F3) even less of the acting & magic tricks con-men use.

27DIO 4.3 §1 §F8: “Question: how can there be peaceful multi-culturalism where 2 or 3 cults are competing to outmultiply everybody else?” See also DIO §8 §5 §O2.

28Back in the analogdisk (pre-CD) era, Steve Martin used to tell the following story: when he played records on his phonograph, they didn’t sound right, so he rose to stereophonic. Still sounded bad, so he went to quad. No luck, so on to octophonic. Failure after failure led finally to milliphonic: 1000 speakers. Still bad. Finally, he caught on: it was the needle. (See also DIO 2.3 §8 §C25.)

Hipparchos’ Eclipse-Star Data 2009 December DIO-J.H.A 16 §1 5

A6 So much for the bad news. Now for the glad news: as on other occasions (Rawlins 1991W §§D1, O1, & S3), I have here become indebted to Muffa blundering for putting me onto a useful idea (though never so directly as in this gloriously delusional instance). For, Evans’ §A1 sign-muff quickly led me to wonder: could the same eclipse-parallax-sign-error also explain Hipparchos’ most notorious empirical disaster (§B2)? — his grossly discrepant attempts to place the star Spica via two of the three lunar eclipses we know he observed. (If so [and we are about to see that this theory is indeed valid: eqs.6&7], then all three of Evans’ ancient & modern sermon-star examples [§A1] — aimed at alibing Ptolemy & showing up skeptic R.Newton’s supposed naiveit about observational astronomy — are fallacious.) In Rawlins 1991W fn 288, it was remarked that the theory Bickled.

B Reconstructing Hipparchos’ Eclipse-Placements of Spica & His Neglect of the Equation of Time

B1 Fundamental astronomers attempting to find fundamental stars’ longitudes wrestled for centuries with an obvious inherent problem: 0° longitude is the Vernal Equinox, but that is the location of a solar event and the stars are invisible when the Sun is visible. The best-known pre-modern method was to use the Moon (or Venus) as a stepping stone: near sunset, find the arc between Sun and Moon while the former was still visible, then find the arc between star and Moon a little later (method nicely diagrammed by Evans 1987 p.235 Fig.4). Finally, use mostly simple arithmetic (Rawlins 1982C App.B) to find the arc between star and Sun. But, Hipparchos also applied an ingenious alternate method, which avoids such a rickety scheme: just measure how far a star is from the Moon at mid-eclipse, when the arc is guaranteed to be visible (though see fn 19) 180° from the Sun.

B2 Ptolemy tells us (Almajest 3.1) that Hipparchos used the eclipses of −145/4/21−22 and −134/3/20−21 to try locating Spica. The results: 173°1/2 & 174°3/4, resp. a terrible disagreement — over a degree! (Remember: the lunar semi-diameter is merely 1/4 degree.) So, we now apply the parallax-sign-error theory to both eclipses.

Velikovskian boldness and correctness has re-dated the Ancient Star Catalog by ordmag a millennium) is that it showed that one could prove anything with statistics. (Is the Muffa aware that the Fomenko believes that the Almajest is a late medieval document, and that the Nabonassar epoch [747 BC for most of us] is actually from the AD era? Full information available from the Velikovskians’ least favorable mongoose. Leroy Ellenberger, 3299 Utah St, St.Louis, MO 63116; phone 314-772-4286. See also the excellent Isis review of Fomenko’s book. A central technical flaw undoing the entire Fomenko et al analysis is revealed in the 1995-added note in DIO 4.3 §14.) Yes, one can prove anything with statistics — if the sample is biased or the math miscomputed. But it is up to the Mufa to show that Ptolemy not only stole the Catalog but clumsily attempted to hide this theft by a paper showing that Ptolemy not only stole the Catalog but clumsily attempted to hide this theft by some dangerous passing-fads (see Shaw’s Doctor’s Dilemma), e.g., radical mastectomies, radium treatments, mammograms, x-irradiation in general, etc.
For each eclipse, Hipparchos' method was:

\[ \Delta \lambda = \lambda_s - \lambda_M \]  

(b) Compute from his tables the longitudinal lunar parallax \( p_\lambda \), which is the difference between \( \lambda_M \) and the Moon’s true (geocentric) longitude \( \lambda_s \):

\[ p_\lambda = \lambda_s - \lambda_M \]  

[c] Without applying the equation of time, find via Hipparchos’ PH theory the Sun’s true geocentric longitude \( \lambda_S \) at the time (according to Hipparchos’ lunisolar theory) of mid-eclipse, which yields true geocentric \( \lambda_M \) by the simple equation:

\[ \lambda_M = \lambda_S \pm 180^\circ \]  

Adding eq. 1 to eq. 2 and subtracting eq. 3 produces an equation for the desired stellar longitude \( \lambda_s \):

\[ \lambda_s = \lambda_S + p_\lambda + \Delta \lambda \pm 180^\circ \]  

For the –145/4/21-22 eclipse: the outdoor longitude difference \( \Delta \lambda \) (between Spica & the Moon) at the time when Hipparchos’ indoor lunisolar theory predicted mid-eclipse (23:38 Lindos Mean Time), was about –33°8, so he likely measured close to \( \Delta \lambda = -33^\circ 5/6 \). [b] Hipparchos’ PH solar theory placed the Sun at about \( \lambda_s = 27^\circ 2/3 \) at this time.

9 Hipparchos might read a slightly different result because of Earth-spin. The systematic errors of his Ancient Star Catalog indicate that he averaged 19° of time-delay after setting the armillary astrolabe (by his reference-object) before getting the reading on his quarry-object. (See Rawlins 1991H §G4: 1/3 of –13° is about –4°.) Whether the same error held during careful, repeated eclipse observations, we cannot be sure; but it makes little difference, given the rounding roughness of ancient data.

10 See Rawlins 1991W §K10. PH theory’s tables (possible tiny discrepancy suggested: ibid fn 199) at Almajest 3.2&6; \( \lambda_s \) was (similarly to the case of the Hipparchos lunar observations reported at Almajest 5.3&6) pre-computed for the tabular time of eclipse. The present results agree with Hipparchos’ consistent neglect to apply the equation of time even to lunar data, as was earlier induced on quite independent grounds by Toomer, Jones, & DR. (Rawlins 1991W §§N1&N8. To repeat the note made there at the time: we thus have no equation of the time’s use before Ptolemy.) This omission has a serious effect on calculations (luckily, allowing us to be sure of the eq.time’s neglect), as do the 0°.4-amplitude & 0°.2-amplitude periodic errors of the Hipparchos-Ptolemy solar&lunar theories, resp. (Hipparchos worked by apparent time, though PH is for mean.)

11 Hipparchos’ likely location on the island of Rhodos (Rawlins 1994L §§F-G), near Lindos city: 36°08’N, 28°05’E. Keep in mind that in antiquity (in the absence of reliable mechanical clocks) the so-called “Apparent Time” was customarily via sundial. Hipparchos’ clock-stars (Hipparchos Comn 3.5) would allow night-timekeeping. But there is also the “moondial” possibility, especially easy while a lunar eclipse is proceeding: a sundial (or equivalent) could find pretty accurate time just by adding 12° to the Moon’s hour angle or (when moonlight was bright enough) to sundial-shadow position. The method is slightly corrupted by lunar parallax. For the –140 eclipse, the time-error would add 1° to the absolute magnitude of \( \Delta \lambda \). (PH assumed for Lindos, not Alexandria.)

12 See Rawlins 1991H §C6 for the standard Almajest 3 solar orbit which Hipparchos used during all the period (§D5) which includes all three of the eclipses here discussed.

D Two Unjustly Neglected Nobels

D1 And the Nobel Prize for Chemistry goes to . . . Barry Bonds. (Bonds isn’t smiling. He’s not quite into every kind of needling.)

D2 The Nobel Prize for Physiology goes to PC for its epochal discovery that the human brain is the only living organ in the entire universe with 0% genetic determination.

E Definitions

E1 Barbara Rawlins = only princess ever to marry her court jester.

E2 White House = ultimate Oscar. (Worse: www.dioi.org/pro.htm#wzf & #qcv.)

E3 Jesus’ post-crucifixion non-public circulation = Resurrectile Disfunction.

E4 IINO (antiself-pronounced: I-no) = Independent-in-name-only, calls self Independ­ent but, TV-zombied: votes just for Dumbos&Dembos. (Talk about wasting war; antiRobinHood-bails not nationalizes the big banks that elected&own him; and now (fn 2) seeks mandated non-universal coverage. Leftists keep alibiing him and Hoping he’ll turn out as dreamed, talking of holding-him-to-his-promises (how?); and (fn 2) seeks mandated non-universal coverage but, as prez, ipped to his media’s paid shepherds, not the American sheeple they inflamed. All a replay of a David Low cartoon c.1937 when England yet hoped Mussolini was OK, this long, kept alive by him (even while warning him to behave), to preserve the dream that anti­reds Hit&Muss (Low’s perfect abbrev) were net­pluses (Liddell-Hart 1970 p.8), despite crushing waves of counter-data. Low, having in mind the same what-DOES-it-take question as above, draws Muzz as stock­villain in a stage melodrama, snarlingly leaning against Brit hero, who wags a finger: “Benito Mussolini, have a care! You have ruined the woman I love, killed my aged mother, sunk the British fleet and set fire to the Empire — but BEWARE! Do not go TOO FAR!”

E5 “Bank” = as prex to “Robbery”, has lately gone from objective to adjectival.

E6 “Bank” = as prefix to “Robbery”, has lately gone from objective to adjectival.


E8 Candidate Obama promised non-mandated universal health coverage but, as prez, ipped to his media’s paid shepherds, not the American sheeple they brainwashed. All a replay of a David Low cartoon c.1937 when England yet hoped Mussolini was OK, this long, kept alive by him (even while warning him to behave), to preserve the dream that anti­reds Hit&Muss (Low’s perfect abbrev) were net­pluses (Liddell-Hart 1970 p.8), despite crushing waves of counter-data. Low, having in mind the same what-DOES-it-take question as above, draws Muzz as stock­villain in a stage melodrama, snarlingly leaning against Brit hero, who wags a finger: “Benito Mussolini, have a care! You have ruined the woman I love, killed my aged mother, sunk the British fleet and set fire to the Empire — but BEWARE! Do not go TOO FAR!”

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E12 See Rawlins 1991H §C6 for the standard Almajest 3 solar orbit which Hipparchos used during all the period (§D5) which includes all three of the eclipses here discussed.
**C Occasional DIO Prescience?**

DR thought\(^{18}\) OsImpson wouldn’t get off (1st time around). But otherwise DIO commentary scored some hits outside scientific history. (For indications inside: www.dioi.org/vin.htm.)

**C1** Our opposition to mammograms was published in 1992 (DIO 2.1 §11 [A8 [b]]. In 2009 Nov, the dam broke and the severe risk-benefit balance we discussed is now (partially) out in public. What will distant-future commentators say of a mass program (much driven by patients’ you-never-know fears, doctors’ fears of malpractice suits, & perhaps researchers’ wish for a national data-base) expensive in time, aggravation, pain, terror, with little if any net gain other than to mediz biz income? (See fn 23.)

**C2** The same DIO section condemned as lunacy Reagan’s theory that trillions in national debt would be paid off by the re-stimulated economy. How long will the media continue to be in awe of Reagan? We note that a) hyper-accelerated the US’ already glaring rich-vs-poor gulf, & [b] started us down the road to ever-deeper, vicious-circle debt-addiction, which could end in a sudden war of desperation, or a slomo foreclosure-sale (who’s funding stimulus-paybacks?) akin to the under-rated, DIOLuciously tasteless 1979 farce-film, America.\textit{C3} In 1998, we urged (DIO 8 §5 [C]) a reconstructive “Gospel According to Judas”. In 2006, the National Geographic Society revealed a long-secret 1970s find of a “Gospel of Judas” papyrus in Egypt. One of the books soon growing out of that was J.Archer & F.Moloney \textit{Gospel According to Judas} 2007. None of this related to our interpretation of Jesus as a fiscally canny typical cult-guru, privately enjoying the percs of wealth to the point of disillusioning idealist Judas, who (John 11.12-19.29) told Jesus that the funds might better be spent on the poor instead of Jesus’ needs of the flesh. (See www.dioi.org/rel.htm#thbp.)

**C4** On 2009/11/30, DIO posted (www.dioi.org/pro.html#cpp) the charge that the Demos’ partisan pose (which yet cons some of the Left) was a fake, predicting that 2008 would bring as phony a Choice as Johnson “versus” Goldwater in 1964, when the War candidate was for war, and the Peace candidate was for war. The issue [as also at §B2] is separate from that of the war’s wisdom. The point here is simply the “bipartisan” truth behind the pretense that US elections still mean much, on the most important issues.

**C5** [Added 2011: 1864 fake war-choice had Lincoln “vs” own General (!) McClellan of Peace Party Dems.] DIO 8 (§5 fn 22) [1998] suggested the identity of Deep Throat. On 2009/10/17, DIO 1 learned of ongoing detailed expert research indicating that Mark Felt’s sole source was indeed our nominee. We await further developments.

**C6** The following was posted at www.dioi.org/pre.htm on 2007/11/20 (2nd to the day before Obama’s swearing-in) under the header, “Hilla the Hun Against the World”:

> “While some are regarding it as a celebr-joke, an argument can be made that spectacularly wealthy and greedy world-rule-dreaming mega-forces are behind [the Obama candidacy’s challenge to Hillary], since who else would care to push a neophyte whose sole standout-qualification is that he looks as international as any other Miss Universe.”\(^{19}\)

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\(^{18}\)See DIO 4.2 §9 [F3] & DIO 6 §4 [SC].

\(^{19}\)It took Maureen Dowd over 2\textsuperscript{nd} to catch up to DIO on the “Miss Universe” recognition: \textit{International Herald Tribune} 2009/10/12 p.9. What is disappointing, about the Dembo “base” that Tribkleedown Obama sucker for the nomination, is how inedible it remains. (How does the Left think Obama got backed by more money than all other candidates? The tooth fairy left it under his pillow? Actually, much of the Left still believes his campaign’s deliberate deceit: that most of his funding came from average folk.) What’s left-of-the-left is so dazzled by Historic Justice in electing a “black” prez (the rich’s C.Thomas-ploy, which reliably kryptonizes Dembos), it’s in-denial on the realities (§E7) right before them. The above 2007 posting is enlightening only in that it shows how widespread in 2009/10/20 that these betrayals were cynically planned not just from Day-One but (like the US’ Iraq occupation) from Day-Minus-One. \textit{What does Obama have to do before the Dembo base realizes it was snookered?} Dumbos act inversely inedible: branding him socialist, though he’s a GOP dream-come-true. But this may be just theatre to keep pushing centrist rightward.) Pre-election: Obama used ambiguities & kept-press hype to convince hopeful college kids he was the peace-option and the populist, & promoted universal medical coverage (so who’d need insurance companies?) somehow mandate-less. Post-election: keeps Bush “Defense” Sec’y; expands Hippparchos’ Eclipse-Star Data 2009 December DIO 16

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\(^{13}\)We assume accurate observation and the ancients’ common practice of rounding quantities to fractional degrees. Our fits here are almost too good (fn 22), which can be due to [a] Hippparchos having came averaged a careful series of mid-eclipse data for each eclipse, and-or [b] DR having acquired Ptolemy’s habit of favoring (postulating likely Hippparchos) roundings that lead to exact agreement. But the putative latter-factor’s net effect is trivial. (Rawlins 2018U [C] realized Hippparchos’ 1\textsuperscript{st} Rhodes observation was the —146 S.Solst, 1\textsuperscript{st} of the 3 cardinal-pnt data he computed the PH orbit from, after his —145 Vernal Equinox, in time to predict mid-eclipse for his planned —145/4/21 Spica placement.)

\(^{14}\)Toomer 1984 p.227 n.21 correctly points out the accuracy of Hippparchos’ longitudinal parallax correction for the luni-solar observation he made on —126/5/2 at 6:20 Rhodes Apparent Time. (See also Neugebauer 1975 pp.92 & 323.) His correction was rightly positive (so he [or a member of his school] had by this late point in his career straightened out the signage of his procedure): 1\textsuperscript{7}8 + 1\textsuperscript{7}12. (That is, +12/12, apt to a lunar distance of well over 60 Earth-radii.) The actual parallax was
those used by Ptolemy 3 centuries later. Since parallax tables are contracted by spherical trigonometry, this finding confirms once again the contention of Diller, van der Waerden,16 Dicks 1994, and DIO that spherical trig throw in the 2nd century BC.

**D2** Note that Ptolemy himself indicates same through his suggestion (Almagest 3.1) that Hipparchos’ Spica discrepancy may have been in his calculation of the parallax correction.

**D3** The italicized §D1 point is utterly self-evident,17 yet it has been missed by scores of prominent, well-paid professional historians-of-astronomy, each of whom has read the same passage dozens of times. And we may be sure that JHA & like establishment publications will not miss a beat in continuing to sanctify these same can’t-see-nose-before-face archons who, in Histon-Craven circles, are the arbiters of accepted wisdom. And acceptable scholars.

**D4** (Neglected, quite ambivalent hints that sph trig might be even older than Hipparchos are found at fn 16 & §D5. Also Rawlins 1985G §8: 2nd table, the ancient data of which could actually be due to Ptolemy and thus not pre-Hipparchos.)

**D5** It has been (Rawlins 1991H fn 7 & §C4 & [Rawlins 2018U sect cit]) theorized that the PH solar theory & tables were based upon observations of −146 & −145 and that his subsequent UH tables were based upon observations of −142/9 & −134/6 (thus could not be earlier than the latter date) — independently suggesting that Hipparchos’ PH tables were used by him during the period −145/4/21-22 to −134/3/20-21.

### E Sources of Error in Hipparchos’ Placement of Regulus

**E1** We now turn to the 3rd (and only other) eclipse known to have been observed & reported by Hipparchos — an eclipse which happens to have occurred near the star Regulus. Two initial comments: [i] Only 2 stars’ explicit Hipparchos longitudes survive (Almagest 7.2): Regulus 119°5/6 and Spica 174°, ideal fundamental stars, the nearest 1st magnitude stars to the ecliptic. [ii] For Spica, the discrepant eclipse-based results he complains of (§§A5&B2) evidently (fn 22) induced him later to opt for placing this star instead18 by conventional astrolabe technique (which was in fact more reliable than his mis-guided eclipse method); however, Regulus is the classical bright star with the largest negative Hipparchos’s error for his ancient Star Catalog’s epoch (−126:28: Rawlins 1991H §F4): −35°. Rawlins 1991W (fn 147) remarked aloud at the enormity of this error (which led Ptolemy into a fraudulent copy of it: DIO 8 §11 [7]), despairing as to whether its explanation would ever become known. (Another fruit of having at last the solution to the Regulus longitude mystery: Shevchenko 1990 had proposed that Hipparchos’ Moon-star

### B3 Butcher Lincoln

Some months before the 1862/9/17 Battle of Antietam, Abraham Lincoln for days sporadically spent hours alone weeping at the death of one youth: his young son Willie, who had died February in the White House of typhoid fever. Question: Is there a credible record (any record?) that, following the September wounding & death of tens of thousands of youths at Antietam, Lincoln shed as many tears? — Any16 tears? After Cold Harbor, his hirings — “Butcher” Grant — did

**B4** We know the trendresult of freemarket capitalism is monotonic wealth-concentration. (Vis-à-vis entropy, this is the anti-twin of physicists’ heat-death nightmare.) An organism can’t function robustly if all blood is in the palm. A restorative conscatory-redistribution not just of income but of wealth would be helpful. (If the richest 2% own perhaps most of it, this might be a wiser source of Stimulus money than the middle class’ future. Obama&co act like the idea never even occurred to them.) But there’ve been no provident Solons for millennia. Instead, the French Revolution, Stalin, and D.MacArthur (Japan) indicate: only hideous bloodletting gets it done (too often net-counter-productively), since all establishments (incl. academe: above p.2; or www.doi.org/err.html#bog) can& will use ANY means to hold onto power&riches. Will world greed-death equilibrium arrive (whether or not secularly stable) when all but rulers are unlanded peons and—or “terrorists”?
B Some Lives Are More Precious Than Others

B1 Is it coincidental that the 1st time in US history when the Prez & VicePrez effectively authorize torture, coincides with the 1st time US warlords are realizing what’s unsettlingly new about starting war? It’s not just our cannonfodder-underling soldiers that are at risk, as in the old days. This time, WE RULERS could get killed, too, e.g., by an imported-nuke. So, the more torture, the greater the chance of interdicting such Litho-unheard-of-insolence.

B2 We hereby launch our book "Courthitz Free Press" — a Courthitz defends "Free Press" — with this essay. The only important exception seemed to be Regulus. But the present results resolve the problem [indicating that Regulus alone among major Hipparchos-Ptolemy stars was not placed by astrolabe], so we may conclude that all the Hipparchos principal stars’ astrolabe-based placements occurred in the evening, just after the Sun’s setting, using a crescent Moon: Rawlins 1991H §G2.

B3 Inductive detectives’ highest ecstasy is the experience of coherent fruitfulness: when a theory already successful in one case is applied to an independent case and the very same theory comes up aces. (E.g., Jones & Duke at DIO 11.2 [2003] cover & p.33; A.Diller’s vindication below at §E3; www.dioi.org/ceem.htm#xviv.) Our outstanding mystery here is Hipparchos’ Ptolemy-super-innermost Hipparch super-stars-placemint (§E1), and our so-far successful theory is that eclipse-parallax-sign-error accounts for Hipparchos’ horrible stellar time errors. If the theory is valid, it can also explain the only other attributed (Almajest 7.2) Hipparchan stellar longitude, the very worst of the lot: Regulus?

B3 We now apply §B3’s method — already good with both his two eclipse-based Spica observations (§B5&B6) plus Evans’ 1981 case — to Regulus & the nearby other Hipparchos-observed eclipse record we have: Almajest 6.5&B9.

E3 For the −140/127-28 eclipse: [a] At tabular−mid-eclipse (22°), actual Δλ was 5°07′, so (especially given his now-famous proclivity for integral20 data), he likely expressed the measurement as exactly Δλ = 5°. [b] Hipparchos’ PH theory gives λ = 305°09′ (Almajest 6.5 makes it 305°08′), so he would record λ = 125°1/6. [c] Almajest lunar theory distance = 54°3/4. [d] So for Rhodos, Almajest parallax tables, Δp = 29° = 1°2/6 which would become 1°2 after sign-mistake. [e] Indeed, 35 years, adding in 8° (c. 1°6) of Hipparchos-Ptolemy 1°100′ precession21 (from −140 to catalog epoch −126.28): λ = 305°1/6 − 1°2/6 + (−5°) − 180° + 1°6 = 119°5/6.

E5 It is wonderful to find that this precisely22 matches the egregiously erroneous (hitherto-unexplained) Ancient Star Catalog longitude for Regulus (119°5/6; §E1).

E6 Two curious historical notes in passing: [a] Regulus’ λ was not used as a ref-star for astrolabe-placing the other Catalog stars of Leo, whose mean error at epoch was merely

6The irony here is that the ease of a nuke getting into the US is greatly enhanced by the machinations of the rulership itself (Obama & other white Republicans) to cheat Labor out of decent wages by mega-immigration and "free trade".

7 Note analogy to equally outrageous-but-underinformed-truth line at DIO 4.2 p.55.

8 Try recalling the last US war opposed at its start by the press or any major church. It helps to be over 100′ old. When Pope JP2 (DIO 4.2 §19 §§HK&HS) discouraged invading Iraq, he didn’t excommunicate Catholic soldiers. Being busy ejecting saintly J.Gramick for her work with homosexuals.

9 The “Mainstream” (§G7) media & the justice system can find zero time to publically debate their own hypocrisy on the present point, instead meticulously devoting eons of their obscenely expensive time to arguing the fine points of even the most obvious evidential pseudo-controversies, and whether an endless succession of rapists, torturers, murderers (staring in its exhaustive examinations of the proscons of case after case) did or didn’t know-right-from-wrong. (See DIO 4.2 §19 §F, which obviously was itself insufficiently cynical as to how nutty courtrooms had become). A neat way to put shrinks on the public dole. And to guarantee maximum recycling of career criminals back onto the streets in everyone’s neighborhoods except the posh ones, where reside those who’re milking the system by such theatre, so they’ll be back in court as soon as possible, for more court profit.

10 See DIO 4.2 19 §D: “Split-Second: Life’s Start as the Most Murderous Moment” on god as the ultimate mass-snuffer of “unborn” humans. (Précis below at fn 29.)

11 But notice §E15.

12 So readers won’t miss DR’s take: if crime trials are part of a war on crime, then the ocasional courtroom justice-miscarry is parallel to collateral damage in conventional wars. (And far more justifiable, since so numerically miniscule by comparison.) The aim here is to emphasize that the establishment’s concern-for-life is but a mask for enriching the clan of defense lawyers so aptly called "criminal". See fn 9 & DIO 6 14 §C7.

19 Almajest 6.5 just computes the time of mid-eclipse as 22:10 by finding when the Hipparchos-Ptolemy lunisolar tables have the true geocentric lunar longitude (125°08′ by the Almajest calculation) 180° different from the true solar longitude. But mid-eclipse accurately calculated (by one of Hipparchos’ computers) from these tables would be nearer 22°. The difference (about −10°) is due to the c.5° tilt of the lunar motion vs the ecliptic in this partial eclipse, a factor that is even more trivial (−16° & −20°, resp) for the total −145 and −134 eclipses, where Hipparchos also likely rounded the tabular-predicted Lindos Apparent Times (to 23°/23 and 3°, resp). If Hipparchos’ presumed −140 use of 22° was not just a rounded value but due to accounting for tilt, then he made a tiny slip, since at the tabular mid-eclipse moment (22°) the Moon’s longitude was about 5′ short of being opposite the Sun. (If Hipparchos actually used 22°/16 Lindos App. Time: −5° would still be the likely recorded Δλ.)


21 Hipparchos’ −140 Regulus longitude would’ve been listed at 119°23′. Though 8′ is something less than 1°6, the Ancient Star Catalog’s longitudes are almost exclusively expressed in units of 1°6, so that precession to the Catalog’s epoch yields 119°4/5, this would still end up being listed as 119°5/6, since nothing in the Catalog is expressed in degree-fifths.

22 DIO 8 §11 1/1 found that astrolabe lunisolar observations showed ±0°.1 consistency. Ancient Star Catalog longitudes’ mean error is 22° (R.Newton 1977 p.216), but stars brighter than 3rd magnitude used by pre-Ptolemy ancient astronomers for observations aren’t in the Almajest (dropping too-south Sco, and 2-quarter-degree ending stars: DIO 2.3 [ff 18 & 20], are β Tau, α Gen, β Gen, γ Vir, α Vir, α Lib, δ Cap, whose longitudes’ deviations from a zodiacal error-wave (mpled from Rawlins 1991H §§F1-F2) of −9°−15′ sin(λ) [96°], show scatter ±0°, 1°, around an average of ±0°.1.

[Note added 2017. For −145, −140, −134, −1987 eclipses, J.HA’s view insists on longitude errors of, resp, −3°−33′, −35°, −33°−40′. Removing parallax-mis-signs, & defects in Hipparchos’ PH solar theory: errors in outdoor-observed star-Moon gaps were, resp, −2°, −4°, +1°, +2°. (All 6 ancient results share in common the same trivial error, due to ΔT uncertainty, of ordmag 0°.1 for that era.)]
E7 Late Ptolemy's work's use of Regulus as a foundation-point suggests that Hipparchos held Regulus as a pivotal star in his astronomy, which could explain why his 140 measurement of its position was retained inviolate to the end of his career. And Ptolemy's.

F Evaluating Hipparchos & the Sign-Slip Theory

F1 We conclude that our parallax-sign-error theory has survived the §2 fruitfulness test: 4 hits for 4 at-bats. But the traditional image of Hipparchos as among the greatest of ancient scholars survives less robustly. (See also Rawlins 1991W § N16&S.)

F2 A temperate conclusion is that Hipparchos was a vital promoter of astronomy in antiquity, if not quite the critical scientific figure he was once thought to have been. (Indeed, some of his attempts at improving basic astronomical parameters may have degraded them. See, e.g., Rawlins 1991W §S1.) He is today most famous for discovering precession, yet Rawlins 1999 shows that it was known to Aristarchos of Samos about 1 1/2 centuries earlier.

F3 But this doesn’t dim our gratitude for his merits, e.g., [a] Grounded in empiricism.

[b] Developed nested calendar (Rawlins 2002A fn 14&17) and durable lunisolar theory.


[e] Took accurate solstices [DIO 20 §2 Table 3]. [f] Oversaw creation of his ever-remembered Ancient Star Catalog, the oldest extant detailed compendium of the starry heavens.

[g] [Draconitic ratio good to ordmag 0.1 via – 1244/11/13 eclipse: www.dio.org/jb13.pdf.]

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Gerald Toomer 1984, Ed. Ptolemy’s Almajest, NYC.

References continue...

A1 How did FDR & Bush 2 differ? FDR was crippled below the neck. And heart.

A2 How do academic pols resemble Bach's 250 cantatas? Herd one, herd 'em all.

A3 By Their Fruits Ye Shall Know Them. Jimmy Cagney became immortal for real.

A5 Born Lyres' Pure Barden. Astrology was evidently born in Mesopotamia. Obviously under the sign of Taurus. Its professional practitioners are born under the constellation Lyra. And you thought horoscopes were worthless? If astronomy is the oldest science and prostitution the oldest profession, the prostitution of astronomy is superlative-squared.

A6 How did FDR & Bush 2 differ? FDR was crippled below the neck. And heart.

A7 California is the US' prime home for astrologers, psychics, etc. The cultural insipidity help produce a state in such ghastly shape that most educated persons permanently crossing its border are leaving, not arriving? Is California what Yogi Berra actually had in mind when he said of a restaurant: “nobody goes there anymore; it's too crowded.”

A8 Making God's Point: Model-T

Model-T is the US' prime home for astrologers, psychics, etc. The cultural insipidity help produce a state in such ghastly shape that most educated persons permanently crossing its border are leaving, not arriving? Is California what Yogi Berra actually had in mind when he said of a restaurant: “nobody goes there anymore; it's too crowded.”


A10 What’s the difference between a believer in god & a believer in SantaClaus? Answer: One is 365 times crazier than the other.
it in-full here at Table 1. (Bolstered by Table 2 & Fig.1, as well as by \( L = 0^\circ \), & eq.3’s fresh discovery of the Meres‘ klima’s actual ancient value.) The long-overdue detailed \( DIO-counter-attack against Mufosi’s 75\textdegree shun-trashing of Diller is also appropriately in-full.

K4 It is notable that all three of the scholars who have gone into print to repel Diller’s discovery are historians who have served time at the Princetitute. As we asked at \( DIO 4.3 \) \( \S 14 \) regarding the now-mercifully-dead Ancient Star Catalog controversy: “To yet continue stubbornly flying in the face of [in the present instance a long-accumulating multiplicity of consistent evidences: Diller 1934 → \( DIO 5 \) \( \S 53 [1] -[7] \) → Table 2 here] is to carry unfalsifiability to kooks dimension — and to raise the question of whether it is worth discussing historical issues at all. (Of course, one may easily understand why certain maguls might wish to render reason and competence irrelevant to the evolution of ideas in [the hist.astron] field.) For, if even the most logically & evidently one-sided controversies are to be decreed [see NCS at \( DIO 2.3 \S 18 \%C20\&C25 \) as indefinitely irresolvable, then — why investigate anything?"

When this quote was applied to a few Velikovskian at \( DIO 7.1 \) \( \S 40 \), no objection was made by historians. But, can they show equanimity when the same principle is found applicable to eminent personages of their own profession? K5 Final thought: if MuJHA represents the best that the Mufia-defense team can muster against Diller’s truth (and, pathetically enough, it is), then the issue is no longer a legitimate controversy (even for those afflicted by the numeracy-gaung delusion that it ever was) — and Aubrey Diller’s ghost can rest content on his honestly & creatively earned laurels.

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2 Pytheas’ Solstice Observation Locates Him:
Cape Croisette

Pytheas’ Solstice: Oldest Vertical-Instrument Transit Observation

Why Has No Historian Taken Pytheas’ Precision Seriously?

Or Bothered Consulting a Map of Marseilles?

Summary

The earliest person known as a scientist-explorer is Pytheas, native & citizen of the Hellenistic colony of Massalia: modernly Marseilles, still the main city of south-coastal France. A legendary figure, Pytheas was known (\( \S 3 \S G1 \)) as an able mathematician, astronomer, and geographer. In the history of the exact sciences he is primarily remembered for his Summer Solstice observation (\( \S 3 \) eq.10) of the shadow/gnomon ratio at Massalia at Local Apparent Noon:

\[
s_s/g = \frac{41\frac{2}{120}}{\tan 19\frac{12}{120}}
\]  

It is the purpose of the present article to establish several points.

[1] The reality & historical utility of eq.1 is shown by two independent indicia: [a] The ordmag 1’ precision of his Marseilles datum is that expected of real outdoor pre-telescopic measurement.

[b] Said precision narrows Pytheas’ location to a coast near Marseilles (Fig.1) which turns out to be the ideal Marseilles-region location for an astronomical observatory — far better than Marseilles proper.

[2] Pytheas’ Summer Solstice observation was presumably based upon the average of repeated sightings (perhaps in annual banches) at his long-term home-town observatory, which would yield a precise result constituting the oldest extant raw astronomical transit observation.\(^1\)

[3] The exact location of his observatory is recoverable to a precision of ordmag 1 mile — in both latitude and longitude — at Cape Croisette (a few miles south of Marseilles), a vantage-point having an astronomer’s ideal southern view over the Mediterranean.

A Having a Fortuitous Ball

A1 We have elsewhere (e.g., \( \S 1 \) fn 15 & \( \S 3 \)) dispensed with a 2002 Mufia-descended last-gasp attack upon one of the glores of rational scientific history — specifically: upon Aubrey Diller’s immortal priority in proving Hipparchos’ use of spherical trig and an accurate obliquity in the 2rd century BC. But we happily have a positive outcome from the Mufia’s 75\textdegree “hubbub” on the Diller issue (to borrow MuJHA p.15’s flip snert at the firmness of Diller-DR’s diamond-clear discovery): we will respond to the offending paper’s mis-adducement of the famous S.Solstice gnomon observation of Pytheas of Marseilles (which alleges it was just a calculated non-observation), by running with the ball fortuitously lobbed our way, recognizing the datum as that of a patently high-precision observation — and thereby locating the Mediterranean spot near Marseilles where this legendary astronomer-navigator-explorer did his astronomy: Cape Croisette (Fig.2), 0\textdegree 1 south of Marseilles-harbor proper (Fig.1).

\(^1\)Without certainty, one presumes Pytheas observed before Timocharis since the latter probably used a transit circle, an advance over the gnomon. Anyway, Timocharis’ star declinations are not raw data.
A2 MuHJA p.17 having claimed that the Summer Solstice datum (eq.1 or §3 eq.10) of Pytheas was not an observation, we explore (as scientists should) an alternate possibility, namely, that Pytheas’ eq.1 was a real gnomon observation. (Which is actually, a priori, much more than a possibility.) We know that many Greeks’ gnomons were vertical & asymmetric. (See, e.g., diagrammed discussions at Manitius 1912-3:1:419-420 & R.Newton 1977 pp.38-39. Also developments in, e.g., Rawlins 1982G & Rawlins 1985G pp.260f.) This produces a shadow corresponding to (eq.5) the S.Solst zenith distance \( Z \), of the top (not center) of the solar disk: the upper limb. (I.e., measured \( Z \) will be 16° [the solar semi-diameter \( \text{s.s.d} \] less than the \( Z \) of the solar center, a fact many well-known Greeks were naïve about.)7 Thus, a solar tilt \( \delta \) will reduce an instrument’s \( \delta \) accurately less than the true value. A useful 1° estimate of the uncertainty in Pytheas’ \( Z \), follows from checking its limits (via eq.1), knowing ancient rounding practices (discussed at, e.g., Rawlins 1994L §B3), which used degree halves, thirds, fourths, fifths, & sixths:

\[
\frac{41\frac{2}{5}}{120} \pm \tan 19\frac{11}{12}' \quad \& \quad \frac{41\frac{4}{5}}{120} \pm \tan 19\frac{13}{12}'
\]

(2)

Thus, crudely:

\[
Z = 19\frac{12}{12}' \pm 1'
\]

(3)

A3 But we can improve the precision here by examining4 ancient rounding even more finely than at §A2: if Pytheas’ reading (of his 120°-high gnomon) were near 41 3/4 or 41 5/6, he would not have rounded to eq.1’s 41 4/5. (Ancient unit-division was limited to quarters & sixths for celestial longitudes & latitudes but fifths of degrees were ordinary for meridian-observation based data: e.g., Hipparchos Comm [Rawlins 1994L §F4], Almajest 7.3.) So the true brackets are the half-way points in the ranges 41 3/4-to-41 4/5 (41 31/40) and 41 4/5-to-41 5/6 (41 49/60), the precise mean of which is (including plus-or-minus found from each difference):

\[
\frac{41\frac{3}{4}}{20} + \frac{41\frac{4}{5}}{20}/2 = \frac{41\frac{19}{20}}{120} = \text{arcmin} 19\frac{12}{12}' \pm 0.5
\]

(4)

\[\text{The Greeks’ propinquity for the flawed idea of using an asymmetric gnomon has never been confronted. (Perhaps partly because ancient-astronomy historians tend not to actually try using the equipment they write about.) So, here’s a go at resolving the issue: the edge of the penumbral fuzziness of a vertical stake’s shadow-tip is not vague. When all but 1° of the solar diameter is covered, the remaining sliver of the solar disk’s dazzlingly brilliant area is ordmag 1% of the whole, so that such a sliver is ordmag 10000 times brighter than the full Moon — which is why the edge of the penumbra is much sharper and thus more precisely determinable than most expect. Thus, a 1° random error is unlikely for careful use of a vertical gnomon. And the experiment is easy to render so precise that the main non-s.s.d error will be minuscule diffraction. Arrange that the gnomon’s shadow be cast into a room protected from non-direct sunlight. Use a vertically-oriented rectangular-plate gnomon (see, e.g., R.Newton 1973-4:p.373 Fig.1). Then, between it and the shadow, bring down another vertically-oriented rectangular-plate until it virtually chops off the solar beam cast upon a flat-horizontal, carefully-ruled shadow-measurer. (Due to diffraction, for 5m-high equipment, the gap between shadow-edge & first intensity maximum is c.1°; but the uncertainty in that edge’s position is smaller: ordmag 1/10°.)}

3 Subtracting \( s_{sl} = \text{16°} \) from eq.5 shows that if Pytheas knew the correct obliquity (but didn’t know of the gnomon’s \( s_{sl} \)-error), he would have thought that his observatory was at about \( L = 42°56' \). (Reprints in 2009, with Meroe resolved: DIO 5 Table 0 at www.dioi.org/woels/w50.pdf; detailed table and odd/even: www.dioi.org/jhaj/.) Don’t blame DR’s sardonic style for the field’s pathology, Princetituter & Mufa godpop Neugebauer’s possessive shunning and Babylonianist abuses of Diller 1934 had been going on since before DR was born (indeed, for 45 years before DR ever contacted Diller): prior to the Mufa deliberately, cohesively ducks owning that Diller’s 57 Original shirt-unstuffer 1994 publication [augmented 2002]: online at www.dioi.org/vols/w42.pdf.

p.2) But, then, few (if any) JHA papers have ever cited DIO except to attack it, since Editor-for-Life M.Hoskin’s rage at the sight or mention of DR’s name is well known. (This, though DIO’s board is patently more scientifically and responsibly than Hoskin’s.) Among said rage’s more transparent play-outs: DIO 6 13 [§2] (Outre? No, just everyday HJA sanity&integrity. As here at fn.24 & §2. In a careerist world, does anyone even care whether journals are honest?) DIO 11.2 [ibid] (Gratitude to Opposites), DIO 11.2, cover & above at §1 fn 18 & §2. In a careerist world, does anyone even care whether journals are honest? Except to steer clear of the danger of being associated with those that are.)

2 A firmer objection is that, if \( Z \) were 19°5/15, §3 eq.15 would not yield its (attested) sum.

K Xerxes’ Eternity-Squared — & How Purple Cows Got That Way

K1 Jones 2002E resembles a try-anything-even-if-it-contradicts-yesterday’s-dodge routine, of an anti-Occam brand already spoofed at DIO 2.3 §8 §C3. And the next Mufa's into the lists to degrade Diller will offer a different joke-defense, casting Jones’ aside (just as he dumped Neugebauer’s), but the rigidly prescribed common thread will be: Diller hasn’t established anything. Xerxes’ *Immortal Ten Thousand* army faked eternality by replacing each slain soldier with another, so we should admire the Mufa’s “Immortal Ten Thousand” degrade-brigade as granting to Xerxes eternal-life it’s own eternal life. (See perverse analogy at www.dioi.org/epi.htm#env.) And so we observe (yet again) a familiar cardiac-Xerxesure at the spectacle of anti-imperial rebellion and heresy being Taken-Seriously (Iiss: Thurston 2002S in this instance), leading to (yet again) a serial stout-wriggle attempt to rescue (yet again) an exalted sacred cow from the jaws of the mundane spring-trap of mere evidence (DIO 11.3 §6 preface). But the Diller case is (like archonal cows’ heads) too big & too visible for escape. (And is invaluably unambiguous: Table 1 can show even 8 old kids [see DIO 4.2 pp.55-57] exactly how honest the evidence-ducking & debate-averse Mufa is.) I.e., bigfat data-trapped sacred cows just can’t make or fake it as wriggle-out mink. They can only turn purple trying.

K2 Runnin-round like Chickens with — B-But, their Heads Are Enormously ON! Given frantic Browner-motion Muffa inconsistencies (§E6) vis-à-vis Strabo’s klima, we’re not trying hard to resist recalling yet again the old vaudeville-comic rape-defense routine (already vainly thrown at ineducable historians of astronomy back in 1991’s DIO 1.2 §B9): But I don’t even know the girl; & I was nowhere near Judy that night; anyway, she consented. K3 FOR Every reader should consult FOR HIMSELF the still truth of the state of the florid-visaged history-of-ancient-astronomy community’s purple-cowards, as revealed by its forums’ predictable (and predicted) totalitarian revulsion at our 1994 DIO 4.2 p.56 Table 1. The table is so devastating to said clueclever’s insistingly stolid pretense (that Diller’s finding cannot be Received by Accepted Society), that DIO is for the 3rd time publishing

Diller Vindicated: Early Sph Trig 2009 [Rev. 2015] DIO-J.HA 16 §3 37

adducing (Table 2) standard ancient 1°/12 rounding (fn.41) upperped Diller’s score; it now yields his match (§B) to all 13 klima. Caketop-cherry: check contenders’ L for [2009 ed’s] previously unremarked 14th klima, that at \( M = 12° \), the *Equator*; Jones, 100 stades; Neugebauer, 1500 stades; Diller-DR, 0 stades. Jones \( P < 10^{-16} \); Diller-DR \( P = 0.76 \). [For the 2015 edition, several earlier analyses are re-edited to include the Equator klima.]

The Greeks’ proclivity for the awed idea of using an asymmetric gnomon has never been confronted. (Perhaps partly because ancient-astronomy historians tend not to actually try using the equipment they write about.) So, here’s a go at resolving the issue: the edge of the penumbral fuzziness of a vertical stake’s shadow-tip is not vague. When all but 1° of the solar diameter is covered, the remaining sliver of the solar disk’s dazzlingly brilliant area is ordmag 1% of the whole, so that such a sliver is ordmag 10000 times brighter than the full Moon — which is why the edge of the penumbra is much sharper and thus more precisely determinable than most expect. Thus, a 1° random error is unlikely for careful use of a vertical gnomon. And the experiment is easy to render so precise that the main non-s.s.d error will be minuscule diffraction. Arrange that the gnomon’s shadow be cast into a room protected from non-direct sunlight. Use a vertically-oriented rectangular-plate gnomon (see, e.g., R.Newton 1973-4:p.373 Fig.1). Then, between it and the shadow, bring down another vertically-oriented rectangular-plate until it virtually chops off the solar beam cast upon a flat-horizontal, carefully-ruled shadow-measurer. (Due to diffraction, for 5m-high equipment, the gap between shadow-edge & first intensity maximum is c.1°; but the uncertainty in that edge’s position is smaller: ordmag 1/10°.)
Since Hipparchos changed (§F1) adopted parameters (e.g., solar&lunar elements: Rawlins 1982C pp.367f & Rawlins 1991W §§K-R) as his researches progressed, the tactic of bringing a nakedly-alone, extraneous, incoherent Hipparchos datum against a member of a coherent data-set (Table 1) is pointless except (fn 45) as a lawyeresque ploy to join & prop-up the shunning of Diller’s discovery. Since MuJHA’s theories are non-exclusive (DIO 11.1 p.26 n.1) using them (e.g., §G1) to down Diller’s coherent55 success is (informatically) gratuitous. MuJHA can’t match Table 2’s 14-fold match with anything like, & the prime datum brought against Diller’s obliquity doesn’t even fit, so (§G2) MuJHA p.17 alibis: “tiny errors in [H’s] calculation . . . might result from [trig] imprecisions”. Wouldn’t real refng note that the Diller-DR Table 1 asked for no such leniency for its then-dozen perfect H-trig-calculation fits of eqs.4&2 to Table 1? Does Occam’s Razor mean anything anymore? (Further at MuJHA p.17: for \( \epsilon = 23^\circ 51'20'' \) [sic], the resultant \( \mathcal{M} = 15^11/4 \) klima’s \( \mathcal{L} = 30100 \) stades, differing by 200 stades [not MuJHA’s 100] vs Table 1.)

J6 MuJHA doesn’t cite DIO at all. Now, since the newly-discovered and very strongest case (here at Table 1) for Diller’s matches was published at DIO 4.2 p.56 Table 1 (a table in which 15 of determined, evidently-unanimous Mufa opposition has found no errors), and since the 2002 MuJHA paper’s timing suggests that it was concocted specifically to counter omerta-breaking 2002 Hist.Sci.Soc citation (fn 23) of said table, it is inexcusable that MuJHA did not cite the ultra-tight-fit new table, or at the very least: the info that DR’s

Rawlins 1982C (MuJHA n.8) while floating the undetailed Pliny-circular confirmation of 23°2/3 at Rawlins 1982C p.368 (ignoring its eq.28’s extra evidence for 23°2/3), and [b] scoff at Pliny’s precise circular as “crude” (MuJHA n.11, thereby sneering at Longitude Zero-refereed Rawlins 1985G), then competent JHA refereeing would require citation of Rawlins 1985G pp.262-263 where the circular are found (fn 50) consistent with a cleverly&accurately derived linear fit to a klimata table computed by sph trig via 23°2/3. Cornered again (as at §I1), Authoritative-in-His-Own-Mind Jones can only effect required degradation of a lovely DR fit by decreeing as unreliable THE DATA, not his own pre-judgement. Jones 2002E’s implicit proposal: putatively-correct Strabo data (constant-shifted, wrong obliquity) just happened by accident to arrange themselves in precisely perfect accord (read Fig.1’s caption carefully to see how precise!) with: correct obliquity, correct sph trig math, standard degree-rounding & stade-rounding, and without fudging any of the long agreed-to Strabo data. Jones’ unfunnest crankprank since his Winter Equinox (Rawlins 1991W §B4).

Hipparchos observed a UMI NPD = 12°2/5. Jones converts to 8700 stades. But neither Hipparchos nor Strabo did so. MuJHA complains that a star’s 8700 doesn’t equal the Cinnamon klima’s 8800 (Strabo 2.57.35). (He thinks Hipparchos believed each klima had a bright star’s NPD sitting right on it!) This is what co-triggers MuJHA n.9 to urge lowering all Strabo L by 100 stades? When MuJHA appeared, DR phoned Jones to stress (§J3): no \( \epsilon \) satisfies this hypothetical new set. Unlike Diller’s 23°2/3 (fn 54) Jones’ best-fit \( \epsilon \) values lack independent confirmation & exhibit no typical ancient rounding. In R.Newton’s phrase: “a subtraction from the sum of human knowledge” (fn 44). Sad to find in the work of one who, despite earlier judgement (prior Jones Mufose mess: Rawlins 1991W), has made additions to said sum (e.g., DIO 11.1 §D1, DIO 11.2 §p.30, Rawlins 2008S fn 23 & p.58, DIO 9.1 p.2; as has JHA (§1 |E1; www.dioi.org/fr.htm#cskv); & Evans (www.dioi.org/cot.htm#gjne, ggg.htm#vppp). MuJHA yields nought but chaotic (e.g., §E6 item [c]) muddying of others’ achievements. Was this its cultish destructive aim? Unrefereed Jones 2002E deems worthless all DR refereed finds touched-on: \( L = 31^\circ 04' \) (§J2), circular (fn 54), klimata (fn 44), refereed by Mus (1982), Greenwich (1985), & Isis (2002), resp. DIO 1.2 §H2 [g]’s 1991 prediction of MuJHA DR-credit-denial tactic: “Publish a wild speculation (unattested method or inferior t) which the JHA can then pretend is a viable alternative explanation of whatever DR has solved.” DIO 11.1 p.26 n.1: Strabo’s 8800 stades precisely fits (esp.2&4 & here) Diller’s klimata scheme (Table 2), though unnoticed by Textbook fruitfulness. Yet MuJHA n.9 tries adding 8800 against the Neugebauer-Diller data-set (above & §2: 8700 \( \neq 8800 \)) while following Neugebauer 1975 pp.305 & 335 n.23 in noncing attested 8800’s exact confirmation of Diller. Though DR was 1st to publish the 8800 match to Diller, 8800’s possible relation to 12°3/4 was initially pondered by Neugebauer 1975 p.335 n.23.

Curious practice: try refuting a discovery (Diller 1934) that’s been updated with a remarkably better confirmatory 1994 hit-score (DIO 4.2 p.56 Table 1, or Table 1 here) without citing the update. Likewise, JHA Assoc.Ed Evans 1998 cited Rawlins 1982C, but not the revealing later DIO update’s new clincher-evidence at Rawlins 1994L §C. (Our comments: §L fn 2&7, & www.dioi.org/vols/w80.pdf, Figure 1: Entire Marseilles harbor (Carte Touristique 67 [Marseilles-Carpentras] Institut Géographique National (IGN) France, Paris), including Cape Croisette area (etc) south of the city. Short, narrow east-west white lines mark eq.5’s brackets for the latitude of Pytheas’ observatory. (Northern bracket’s west end is at latter “E” in “CROISETTE”; southern bracket’s east end is near southeast tip of Isle de Jarre.) The mainland capes immediately west (off map to left) of Marseilles Bay do not stretch as far south as the upper bracket and so are not potential Pytheas-observatory locations.
that and tested for $A_0$.) By contrast, Diller’s solution ($\epsilon = 23°2/3$ and $A_0 = 0$) easily falls within 1 standard deviation (sd) for both variables. (Probability $P$ exceeds 2/3.) I.e., Diller is again vindicated. Doubly. On the nose.31 But who will be the 1st Mufoso — after over 80° of bigotry, ungenerosity, & even viciousness52 — to own up to this? J4 Under the 2-dimensional elliptical-cross-section Gaussian surface representing the probability density $p_d$ of any point on the $\epsilon-A$ plane, probability53 $P$ is the integrated volume exterior to the locus of points whose $p_d$ equals that of the point in question:

$$P = e^{\frac{-D}{2\sigma^2}} = e^{-\frac{(N-2)D^2}{2\sigma^2}} = e^{-FD^2/2}$$

(19)

where $S = \text{square-residuals sum there}$; $S_m = \text{best-fit S}$; $\sigma = \text{single-datum standard deviation}$; sums’ relative difference $D = (S - S_m)/S_m = S/S_m - 1$; $N = \text{no. of data}$; $F = \text{degrees of freedom (N minus the number of unknowns, that being 2 in this case)}$. For the Princetitute 4-dimensional case (fn 7): $P = (1 + FD^2/2)e^{-FD^2/2} = 10^{-518}$. J5 A general observation: the MuJHA paper (which never remotely approaches supplanting Diller’s well-founded improvement of our knowledge of antiquity by arriving at a comparably coherent vision: fn 55) omits mentioning any of the five then-known54 published post-1934 confirmations (now seven: DIO 5 §32) of Diller’s $\epsilon$ and data-fit.

...
fourteen that Diller-DR solves all fourteen of. If we try the Eratosthenes obliquity (eq.5) of Jones 2002E’s prong [a] attack and compute via eq.4, the results disagree with about 60% of §11 prong [b]’s proposed 100 stade-shifted klimata data-set. These unavoidable items provide independent validation of the unampered original data-set of Table 1, upon which Diller-DR’s solution is founded. Conversely, if we hold at $x = 23^\circ 51'20''$ & look for the best-fitting A, it’s 158 stades (not Jones’ 100): impossibly far from the unrestricted best fit we are about to locate (eq.18) in $\epsilon$-A space. But $A = 158$ stades would anciantly round to $A = 200$ stades, which fails for 5 out of 13 matches. Probability $P'(eq.19): 10^{-4}$ for 158 stades; ordmag $10^{-6}$ for 200 stades.

J Testing MuJHA by Math (& Unnoticed Klima) Instead of Guess

J1 But these are trifling odds compared to those against adopting Jones’ 2 prongs simultaneously ($\S13$: $\epsilon = 23^\circ 51'20''$ (prong [a]) & $A = 100$ stades (prong [b]). For this remote position in $\epsilon$-A space, $P < 10^{-11}$ (eq.19), i.e., odds of tens of trillions-to-1 against.

J2 But even were Strabo’s data infected by the Jones shift, the truth would be recoverable: [1] the miss-shift could be detected by least-squares analysis [$\S13$] and corrected-for; [2] the L-vs-M curve would still be (see Fig.1 & caption) show a suspect tendency to track the sort of curve produced by sph trig with a Hipparchan & accurate obliquity. Which vindicates Diller, though this important point is (ungenerously: www.dio.org/biv.htm#ncmf) left unstated by JHA; so how is MuJHA a refutation of Diller’s essential discovery? of Hipparchan-era sph trig? That Diller has made this discovery is known to JHA, Jones, & the Muffia. But all have chosen to leave it publicly unexpressed in explicit terms. See comment [2] at $\S1$. A near-century of collective shame is just too awful to openly confess.

J3 We next carry the previous discussion to its logical conclusion. It is obvious ($\S12$) from the shape of the Hipparchos-Strabo data’s $L$-vs-$M$ curve that it was generated from sph trig calculations. Jones agrees that sph trig was known to Hipparchos and (n.7) dumbs Neugebauer’s folly because it (unlike sph trig) “failed to show how Hipparchus could have found a sequence matching so accurately the theoretically correct latitudes”. (Which Hipparchus 1934 had done, herofo to Muffia’s arbitrary non-pleasure.) We run a least-squares fit (no roundings) of the function, $\arctan[-\cos(15\times2\pi/2)] \tan x + A$, upon the $M$&$L$ data of Table 1, to check the fit of eq.4 simultaneously with Jones’ 100-stades-shift proposal, thus treating obliquity $\epsilon$ (eq.2) AND Jones fudge-factor $A$ as unknowns. Formal results:

$$\begin{align*}
\epsilon &= 23^\circ 37'6.6 \pm 3'2.2 \quad \text{and} \quad A = -28 \pm 44 \text{ stades} \\
&= 23^\circ 37'6.6 \pm 3'2.2
\end{align*}$$

Jones 2002E n.99 haggle-adduces disparate Hipparchan data (having nothing to do with Table 1’s coherent data-set) to come up with his $A = 100$ stades (which is c.9). But the foregoing best-solution equation shows that Jones’ +100 stades is statistically ruled out, since his $A$ (like even the most helpful $\epsilon$ [$\S13$] adjusted for it) is several standard deviations distant from the $A \& \epsilon$ (eq.18) which minimize the residual-sum, with probability $P = 1/700$. I.e., we can find A mathematically. 40 (A non-fictional JHA referee would have known

49 Were MuJHA’s hypothetical data-set actually in Strabo, an uncommitted explorer-scholar would test statistically and would soon find (eq.18) that removing $A = 100$ stades would produce a data-set neatly fitting $L$ values calculated via eq.4. I.e., math-analysis cures corrupt data better than guessing-around. See, e.g., the restored Pliny circuli (fn 50, which (before Rawlins 1985G) had been universally regarded as useless. (A view time-warpily echoed at Jones 2002E n.11. Neugebauer 1975 p.748 even fantastically treats the circuli as “a telling illustration for the absence of any scientific organization in antiquity.”) Yet, by minimal reconstruction, Rawlins 1985G has shown that the Pliny circuli are a clever, unexpectedly precise linear fit to a sph trig klimata table based on Diller’s $\epsilon$ (eq.2) and are thus one of more than a half-dozen post-1934 findings ($\S15$) that back up his 23$^\circ$2/3. (See DIO 5 $\S$D3.)

50 Jones 2002E n.9 credits Muffa-don Neugebauer with reconstructions actually 1st published in Diller 1934 cited in MuJHA’s previous endnote). Again, alert referring wondering to track the DR solution (fn 47) of Pliny’s “circuli” klimata can be similarly grounded in mathematical analysis rather than speculation. If one computes obliquity $\epsilon$ for each of Pliny’s firm klimata from the

Pytheas at Cape Croisette 2009 December DIO 16 $\S$2

B Finding Pytheas

B1 Now at last we are closing in on the Pytheas observatory’s latitude. Using eq.4 and eq.1 we can find the actual latitude $L$ at which Pytheas observed the Sun; the correct empirical relation is (including $s_d = 15^\circ.8$ and $r_{P}$ = $0^\circ.3$, with [for epoch $\approx 310+25^\circ$] obliquity $23^\circ 44'.0 \pm 0'.2$, error from uncertainty of Pytheas’ exact epoch):

$$L = 19^\circ 12'.2(\pm 0'.5) + 23^\circ 44'.0(\pm 0'.2) + 15^\circ.8 + 0'.3 = 43^\circ 12'.3 \pm 0'.7$$

5 We ignore rms, instead looking for the maximum additive range of errors that are not at all likely to be exceeded if the measurement was indeed carefully and repeatedly carried out. I.e., our treatment here is not based upon Gaussian statistics but only upon Greek rounding’s implied precision, as expressed in eq.4: producing a simple bracket instead of a bell-curve. Eq.5’s bracket is obviously from $43^\circ 11'.6 = 43^\circ 13'.0$ and is drawn in pale lines upon Fig.1.

B2 We are not the 1st to compute a latitude similar to (if not exactly equalling) eq.5. But previous investigators merely concluded: well, Marseilles is at $43^\circ.3$ N, so Pytheas was only 0’.1 off the mark — OK-not-bad-and-end-of-story.

B3 But let us instead pay close attention to some previously neglected points.

[eq.18]

51 $A$ Pytheas’ clear precision was $\pm 0'.5$ (eq.4), not $\pm 0'.1$ (c.10 times looser).

[eq.19]

52 Which vindicates Diller, though this important point is ($\S13$) adjusted for it is several standard deviations distant from the $\epsilon$ $\& A$ (eq.18) which minimize the residual-sum, with probability $P = 1/700$. I.e., we can find A mathematically. 40 (A non-fictional JHA referee would have known

53 As with DIO 14 $\S$3’s discovery (www.dio.org/gad.htm#bssl) that the Blest Isles were the Cape Verde Islands (not the Canaries, the longtime traditional guess), one wonders why no one previously ever just checked a map and published the obvious solution.

54 The topo-curves are at 20m intervals for each of the accompanying maps here.

55 Our thanks again to Nels Laulainen for his 2000-2001 expert advice to DIO on such matters.

56 The topo-curves are at 20m intervals for each of the accompanying maps here.
been the most easily accessible for his Marseilles students or clients. (Cape Croisette would also be an apt location for a sailor-explorer: right on the Mediterranean.)

We next check out a few other candidates.

C2 On a sharper hill to the east (just south of the town of Callelongue), there is an antique semaphore-station marked on Fig.2 (over 100m high) at 43°12′38.7″ N, 5°21′21″.1 E, just beyond the end of the extended easy (non-mountain) road from Marseilles to Cape Croisette (i.e., Delabre Boulevard).

C3 As Pytheas was a sailor, we must also consider the possibility that he (like Tycho) operated on an island. The most obvious choice would be tiny but spectacular-gradient Maire Island (whose highest peaks exceed 450′), which is literally throwing-distance from the west spit of Cape Croisette. (See Fig.2.) Maire’s southern coast, though partially quite steep (and not [now] conveniently accessible from Cape Croisette without boat), has the best viewing of any likely9 location considered here. If Pytheas’ 120-unit-high gnomon was 120 Greek feet (a Greek foot being 12″/7 in modern measure), the high, steep cliffs of Maire (Fig.3) might allow a mostly natural gnomon of such height (which would ensure negligible impression from diffraction): the gnomon’s verticity verified by plumb-line with a bob dense enough to minimize wind-influence, and the shadow-surface’s horizontality verified by use of a water-filled hose. A direct exam of Maire’s topography could determine whether this would be feasible.

C4 And there are a few other islands which might be mentioned as possibilities: Ti- boulene, de Jarron, de Jarre. All these places’ latitudes are easily consistent with the limits of §B1’s eq.5. Recall that we began investigating this region due to those very same mathematically-derived latitude limits — and only subsequently noted potential confirmation when finding (§B4) that this put us exactly at the observatory-friendly clear-southern-view coastal region that was nearest Marseilles by road.

C5 Does that striking coincidence assure us that the Cape Croisette region is where Pytheas made his observations? — including the miraculously extant Summer Solstice $s_s/g$. Hopefully, an archaeological miracle will someday discover the exact spot where stood the scientific home of legendary astronomer-explorer Pytheas of Marseilles.

Acknowledgements: for expert assistance in locating materials, etc, I thank Keith Pickering and Jim Gillispie.

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8The (over)precision here is c.10 ft. Atop the hill today, Microsoft maps show a lone building which is at least twice 10 ft across.

9Maire Island’s peak would have even better seeing than its south shore (far lower aerosols on many nights), though with the same extreme isolation-inconvenience that presumably kept Hipparchos from using Mt.Aubron on Rhodes Island. (Mountain astronomical observatories are a modern phenomenon, due to influence of atmospheric unsteadiness in a telescopic era.)

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**L2** For Hipparchos’ klimata, which are expressed by Strabo entirely in stades, Jones 2002E n.9 justifies the need for his proposed 100 stade shift via three non-stade data:

[A] The star $\alpha$ UMi is stated by Marinos (GD 1.7.4) to have been placed at north polar distance $\theta_{PD} = 12°25′$ from the pole, which (by eq.4) for Diller’s proposed Hipparchan obliquity $23°2/3$ (eq.2) corresponds to 8700 stades, not Table 1’s 8800. The Catch: Hipparchos used more than one obliquity (see discussion at §F1), the other one being (Rawlins 1982C pp.367-368 & eq.27) $\epsilon_{II} = 23°11′/12′$ (eq.6), which, if we compute with it (eq.4) for the Cinnamon klima’s $M = 12°3/4$ yields $L = 8700$ stades — thereby providing one simple and quite plausible explanation of the discrepancy. (For another, see fn 55.)

[B] Strabo 2.5.36 puts Syene at $M = 23°7$ (eq.5), which, if we compute with it (eq.4) for the Cinnamon klima’s $M = 12°3/4$ yields $L = 8700$ stades — thereby providing one simple and quite plausible explanation of the discrepancy. (For another, see fn 55.)

[C] Jones 2002E p.16.3′ 31″ Alexandria latitude, derived from Strabo 2.5.38’s $x_g = 9$ for that city, would by eq.1 equal 21700 stades, though (as just above at [B] item [ii]) this is not so stated by Strabo. Since this disagrees by 100 stades with Strabo’s 21800 stades for Alexandria (Neugebauer 1975 p.305 & 1313 and fitted by Diller 1934 & our Table 1 or DIO 1.2 p.56) are given explicitly in stades by Strabo, an obvious indication that Table 1 is based on a coherent, one-source data-set.

[D] Jones 2002E p.17.1′ Strabo 1st stade shift would “afflict the value of the obliquity best fitting the data”. (Yes, and it would produce §J3 A [a] much worse rms than Diller-DR, thereby ruling-out the proposed shift.) Now genuine & (Schafer 2002) “premier” is a journal that would carelessly-lazily publish such deliberate trashing of a precise & epochal discovery, without bothering to test said fit! And without asking Jones if he even knew how to run such a least-squares test? (JHA attitude: if an author is an archon, why referee him?) For Jones’ eq.5 & 100 stade shift, 8 of 13 klimata fail, a worse score than Neugebauer’s! Thus Jones produces no table & never tells anyone where to find DR’s. Jones repels unwelcome evidence as amusingly as the kook Doc-Cook Society (see DIO 9.3 §C7): e.g., if he rejects the data which Diller-DR have fit, then: why can’t he cite the best-fit table? If the Strabo data-set (Table 1) is altered by Jones’ mere 100 stades (less than 9′), no choice of obliquity can satisfy it. That’s how hard it is to thread the needle through these data. Yet Diller-DR’s solution produces a flawless fit to them. For Jones 2002E n.9’s 100 stade-shift klimata data-set, the best fit is for $\epsilon = 23°39′/7′/8′$ or $23°36′/7′$, a figure nowhere stated by Jones 2002E (perhaps because this pron [b] $\epsilon$ contradicts pron [a]’s eq.5&7: §E7). After all, $\epsilon = 23°36′/7′$ is: [i] unround; [ii] “has disappeared entirely from the tradition and is not attested” (selectively echoing Neugebauer 1975 p.734’s attack on Diller 1934); & [iii] has (unlike Diller’s eq.2: §J5) no independent support. And even this best-fitting $\epsilon$ value is ruled out statistically (§J3), and will (if used in eq.4) nonetheless fail for four klimata of the

scheme didn’t give $M = 12°/6$ for the Equator ($L = 0°$); so he “corrected” it by altering an integral constant: changing the 358 in fn 50 to 360. The original is restored at DR loc cit, which finds not only that the L are now in extremely close agreement with pure sph trig calculation, but that the original scheme used Diller’s Hipparchan obliquity (eq.2): see Fig.1.

Figure 3: Panoramic view of Maire Island from very nearby W tip of Cape Croisette.
I Inconsistencies’ Inconsistencies & Hipparchos’ Circuli

Thus, MuJHA p.17’s attempt to connect Hipparchos to 23°51′20ʺ fails both because eq.15 could as easily be (say) Eratosthenes’ as Hipparchos’ and because MuJHA’s eq.13 relation of $L$ & $\epsilon$ now (revised here to accord with mss-based eq.15) leads to obliquity:

$$\epsilon = \arctan \left( \frac{\cos(15\frac{1}{5}/4)(15\frac{1}{5}/1)/2}{\tan 43°04'} \right) = 23°49′25'' \quad (16)$$

which is not Eratosthenes’ obliquity. (Such inconsistencies inevitably result from bringing in scraps of disparate data from all over the place to try splatter-strafing solid work — instead of recognizing the merit of a coherent solution to an inter-related and uniformly unitized: §[2] data-pool, such as the Hipparchos-Strabo klimata.) From prong [b] (§E6): in trying to weaken the Diller achievement, MuJHA states (p.17 [bracket added])

A.Diller and D.Rawlins have derived a value for the obliquity, 23°40′, that yields a close fit to Strabo’s stade figures (which are expressed in round hundreds of stades, thus to a precision of 1°/7). Unfortunately [?], there are some inconsistencies44 in the numbers reported by Strabo, and one may well suspect that one or two modest changes in the intervals, through either scribal error or deliberate tampering, could45 have introduced systematic errors which would affect the value of the obliquity best fitting the data.

Jones’ “untamper” riffs-off Rawlins 1985G p.263’s solution to Pliny’s circuli.46 Note Jones’ implicit acceptance of Diller’s general thesis (sph trig), which is never made explicit. As for “one or two modest changes in the intervals”: any Jones alteration besides uniform shift of all data would produce a trepidation-level-hilariously choppy $M$-vs-$L$ curve. So when MuJHA gets around to specifics, all he can do is agree (MuJHA n.9) with the reliable, long-accepted Neugebauer 1975 p.1313 rendition, except for injecting an odd anti-Diller escape-ploy (n.9): “restoration” by shifting the whole set down 100 stades, to “undo” a dreamed-up ancient tamperer’s hypothetical addition of 100 stades onto the set.47

44 The Strabo Hipparchos klimata data are given mostly as intervals rather than as absolute values, which is why Diller 1934 refers to them as garbled. And there’s been some very obvious reconstruction (to which Jones 2002E n.9 agrees), but the work of decades of scholars (embodied in Neugebauer 1975 p.1313’s valuable & crystal-clear Fig.291) has succeeded in establishing these klimata beyond any reasonable doubt (outside Mero¨e: fn 40). It is thus retrograde scholarship (fn 47&55) to try tearing down one of the grander cumulative achievements of classicism.

45 Translation: if a long-archon-loathed theory has the surprise 1994-2009 effrontery to ultimately fit a set of decades-long-established data, we “have to” (§G3) now reject the offending data, instead of heaven-forbid doubting archonal judgement! When one side doesn’t want to admit it’s lost a dispute to another side, a common tactic for the former is just: do or try whatever it takes to pretend that its cult is not totally defeated, by going for a standard the-controversy-continues sham; see, e.g., DIO 4.3 p.105 n.1; DIO 7.1 §4 p.24 fn 21. In criminal court, we often see a flagrantly guilty client’s lawyer desperately scatter-arguing for all but the obvious solution to the crime, trying to blame it on anyone but the client, insisting that the police didn’t consider one or another of a retinue of red-herring suspects. It’s smart rhetoric and good theatre; but it’s not serious or unbiased investigation. (See also §K.)

46 On 2009/8/18 (25° after the Greenwich Centenary lecture resulting in Rawlins 1985G), it dawned on super-swift DR that the circuli (fn 47&50) may be Hipparchan: [a] the $\epsilon$ is his (eq.4); [b] one of the scheme’s two bases is Rhodos (fn 50); [c] the Rhodos entry is not only mis-written (restoration: www.dioi.org/cot.htm#ypsv), but its restored fraction, 77/105, should’ve been rendered as 11/15 (idem). This suggests bungling by two closely successive and-or insufficiently collaborative hands, early in the scheme’s history, similar to the Hipparchos-school slip found at Rawlins 1991W eqs.23&24.

47 Jones’ 100-stade-shift proposal suspiciously — and invalidly — mimicks (uncited) Rawlins 1985G’s valid restoration of the “circuli” of Pliny 6.39.211-218; the key distinction: while Evans 1987 & Jones 2002E (for huge JHA political advancement) replaced order with chaos (fn 55), DR’s circuli-restoration did the reverse. (As in other cases, e.g., the DIO 9.1 §3 continued-fraction decipherment of ancient yearlength mss.) The $M-L$ pairs found in Pliny were not consistent (fn 50) via eq.4 for any Hipparchan $\epsilon$. As shown at Rawlins 1985G p.263, an ancient dabbler had noted that the original

References

MuJHA 2002. JHA 33:1:15
D.Rawlins 1994L. DIO 4.1 §3.
Gerald Toomer 1984, Ed. Ptolemy’s Almagest, NYC.
A Diller’s Tri-Discovery: Early Sph Trig, Accurate Tilt, Klimata Key

A1 In 1934, the eminent classical philologist Aubrey Diller provided the 1st conclusive proof that spherical trigonometry went back to the 2nd century BC. This unexpected perfect-fit 2009 induction sniffs 75 δ controversy refless Alex Jones Banzai-Sneers at Three Refereed Discoveries Strange JHA 2002 Attack on A. Diller Finally Refereed 7

A2 In 1979, DR independently discovered Diller’s solution while learning of his prior publication (1979/11/26) — while continuing to improve it. Besides adding (to the data-set) Hipparchos-Strabo klimata unknown to Diller (but perfectly fitting it anyway, a striking fruitfulness-display: e.g., fn 55). DR showed that if Hipparchos’ sph-trig-calculated klimata had been anciently rounded to and tabulated at the 5′ (1°/12) precision of the klimata list of the canonical Geographical Directory (GD 1.23), before conversion (eq.1) to stades, then: all but one of Diller 1934’s fits became precise hits. In 2009, the one non-fit also finally became precisely satisfied: eq.3. See at Table 2 here.

A3 If we assume ε₁₂ was measured in standard fashion (eq.8, below) and account for refraction & parallax, an ideal Hipparchos determination of ε would have been 23° 42′, and ancient standard rounding was to the nearest 5′, so ε₁₂ = 23° 40′ was correct to its precision. Even ignoring rounding and r&p, it was (as it stands) accurate to about a 20″ of a degree. This long-lost value for the obliquity was probably measured using Hipparchos’ 135 BC Summer Solstice (Almajest 3.1; Rawlins 1991H), but attestation of it had not survived so (ere Diller) no one had previously suspected that the ancients ever had an accurate obliquity. In short, Diller 1934 simultaneously announced 3 major discoveries: [1] solution of Strabo’s klimata, [2] Hipparchos’ use of spherical trig, [3] his adoption of the only accurate obliquity-measure we can recover from antiquity. That a pack of possessive snobs has nearly submerged such scholarly triumphs for 81 years is itself a triumph of organized truth-warping, providing a history (see p.2 & fn 7) warmly recommended to sociologists of cult-think.

A4 The Hipparchos-Strabo data base which Diller satisfied appears as the middle column of our Table 1 here, based on Hipparchos’ well-known scale

1° = 700 stades

(1) (Strabo 2.5&34 or Neugebauer 1975 p.305 n.27). All 13 said data were computed from klimata M values via eq.4 (below), using the unattested but impressively accurate ε value

ε₁₂ = 23° 2′/3 = 23° 40′

θ = arctan[(41 4/5)/120] + 23° 51′ 20″ = 43° 04′

(15)

H New Implications of Marseilles Latitude 43° 04′

H1 We now produce new, independent, & fruitful evidence for eq.10’s 41 4/5 being anciently taken as an accurate placement of Massalia. At Almajest 2.6, the original ms reading for Massalia’s latitude is not 43° 01′ (as recently emended) and used for MuJHA p.17’s mathematical development. No, the actual reading is 43° 04′. Remember that 43° 01′ is just an indoor klima calculation (eq.13) for the 15° 1/4 klima, via sph trig, having no mathematical relation whatever to empirical eq.10.

H2 We next reveal that Massalia’s Almajest 2.6 latitude L = 43° 04′ was in truth elaborated by comparison (Eratosthenes’?) with Pytheas’ empirical outdoor transit datum 41 4/5, as we see from the simple arithmetic of standard transit-reduction, using eqs.5&10, which produces a perfect hit upon this (previously unexplained?) latitude:

H3 The fact that 43° 04′ is the correct reading is confirmed by the GD latitudes for Marseilles (GD 2.10.8) and Byzantion (GD 3.11.5): both indisputably 43° 1/12, which (in a work whose degree-angles are all Hipparchanly rounded [as also at fn 30] to the nearest 1/12) is consistent with empirical eq.15’s 43° 04′, not with calculated eq.13’s 43° 01′.

39 Though MuJHA’s author is (in non-math respects) superior to B.Schafer as a scholar of ancient astronomy, the attraction to an ultra-shaky basis for an attack on a MuJHA-upsetting DR-related achievement is similar to Schafer’s blinded attraction (Schafer 2001) to depending upon the least reliable test (low altitude atmospheric extinction) of all those available for determining the authorship of the Ancient Star Catalog. (Schafer 2001 was also published in the DR-banishing JHA.)

40 See the vertical arrows of Neugebauer 1975 p.1313 Fig.291, each of which is (except the Equator-to-Merlin arrow) based upon an explicit statement in Strabo. (Ignore the Eratosthenes arrows on the right, and keep in mind what Neugebauer chart’s Alexandria, Carthage, & Meroë aren’t klimata.) It is obvious at a glance that most of the L values of the dozen Hipparchos-Strabo klimata-latitudes are comfortably over-determined (about doubly, on average).

41 Our thanks to Toomer 1984 p.86 n.43 for fairly and helpfully pointing out the original’s δ (the Greek math symbol for 4) even while arguing against it in favor of α (Greek math for 1), since the latter explains the Almajest 2.6 shadow ratios but only if one rounds to the nearest 1/12. (This step also crucial at fn 56.) That is, both L = 43° 01′ and the ε obliquity must be so rounded: to 43° & 23° 56′/5, resp, before the Almajest 2.6 shadow data can be recovered. The original 43° 04′ is properly maintained in the Almajest editions of Heiberg, Man insults & Tallaférro.

42 Syene (eq.17) & Massalia are among the very few cities associated with klimata in Almajest 2.6; both cities are a few miles from “their” klimata. See fn 34 for further discussion.

43 Neugebauer 1975 p.336 rightly backs Honigmann in preferring the 5.7 Alexandria ratio. (The original ms’ ratio, not the Vitruvius-Ptolemy 3:5 ratio later substituted. See LCL’s Strabo 1:510, & Rawlins 1985G p.263&266 on GD Pharo’s L vs Alexandria’s.) He (iden) uses round ε = 24° (not 5°) to develop Pytheas’ L, thereby missing our eq.15 & getting accurate L = 43° 12′ only by chance cancellation of 16′ errors (ε & ssd). (Note: Almajest 2.6’s three s/g are consistent with L = 43° 01′.)
G Empirical Pytheas

G1 The precision of Pytheas’ 41 4/5 (eq.10) is about 1/600 of the gnomon’s height, which MuJHA thinks is unrealistic for early work. But this precision is (eq.2, eq.3) just a rough cut, which is suspiciously consistent with careful outdoor measurement. NB: Strabo 7.3.1 regards Pytheas as an expert. He also reports (Strabo 2.5.8) Pytheas was a Massalian native, obviously enhancing odds that eq.10 is a real 1st-hand observation, that this observation was accepted and not returned. So there is no reason to follow MuJHA’s p.17 rejection of Diller’s unquestionably-calculated fourteen perfect fits to eq.4, just on the basis of MuJHA’s in firm speculation that a reality-accordant reading (s4, in eq.10) was actually non-real and thus also calculated. The MuJHA roundness arguments are curiously high. Highly rounded se/g ratios (Alexandria & Carthage) are obviously not directly empirical (and in these cases aren’t even se/g! : §F4), because in the real world, an outdoor eq.9 measurement of se/g will probably be as round as eq.10 (Massalia). Summing up: MuJHA is simultaneously taking equinoctial se/g as directly empirical and solstitial se/g as non-empirical and thus the very reverse is true. So by 2 independent criteria, MuJHA’s 3 assessments of the Strabo s/g data’s reality are all inverted.

G2 Returning to MuJHA p.17’s fundamental Pytheas-Massalia theory, we see that Jones’ argument is two-step: Massalia L is computed via eq.4 using M = 15°1/4 and the Eratosthenes obliquity (eq.5) which MuJHA is proposing for Hipparchos.

Massalia L = arctan \( \tan(43°01'24''N) \approx 1.25 \) \( \tan(23°51'20''N) \) = 43°01'24''N (13)

Then, we subtract that same obliquity, and arrive at s.Solstice Z where supposed to explain the “calculated” Pytheas s4/g of eq.10 but doesn’t:

s4/g = \( \tan(43°01'24'' - 23°51'20'' = 41.713/120 \neq (41 / 5)/120 \) (14)

a failure which leads (§J5) to Jones 2002E’s p.17 plea — not necessary for ANY of Diller-DR’s FOURTEEN hits (Table 2) — to accept SlighT-Miscalulation (§J5) in the CENTRAL TRAM measurement of Jones 2002E’s concoction. Just one more unexplained inconsistency.

34 The experiment requires the gnomon’s verticality checked by plumb-bob, an art preceding Pytheas by thousands of years. Some gauge ancients’ solar-data accuracy by reference to their star observations. But the Sun is immensely brighter & easier to place. The S.Solstices of Kalippos (329/628 14) & Hipparchos (~134~626 14) were both accurate within 1/4 rounding precision (see Archimedes at Almajest 3.1), but such success requires 1 accuracy since it is done by equal altitudes. (Raw human ocular accuracy is to 1/3: Rawlins 1985G. As ancient scientists were aware: DIO 14 12.)

35 I.e., agreeing with math not reality, obviously the normal situation for klimata: In 34.

36 Based without justification upon claimed (§F4) significance of the eq.10 ratio’s precision. But how would Pytheas calculate Zs (thereby giving him his se/g via tangent) by subtracting obliquity e from L (MuJHA p.17 middle equation), when he didn’t know e using eq.8, which requires one already to have determined Zs, the equivalent (via arctan) of the very item supposedly being sought (s4/g)!

So MuJHA has to speculate that 41 4/5 is Hipparchos’ calculation, via eq.4 using eq.5’s obliquity. Yet Strabo (1.4.5 & 2.5.41) assigns 41 4/5 to Pytheas, not Hipparchos. MuJHA proceeds according to his beliefs that 41 4/5 is a klima calculation (for which there is no evidence) and that the klima’s L = 43°01’ — though we multiply demonstrate elsewhere (§H) here that the L upon which MuJHA bases his argument is actually L = 43°04’, which undercuts his whole case for eq.5’s involvement in the origin of Pytheas’ 41 4/5. (See, e.g., eq.16.) Note that, if using Eratosthenes’ obliquity (eq.4) for M = 15°1/4, MuJHA’s hypothetical calculator would have (via eq.4) gotten not 41 4/5 but the Almajest 2.6 value, 41 4/5 (eq.4). MuJHA p.17 realizes this; so, does MuJHA then responsive compensate, does MuJHA then try to contradict (and Strabo 1.4.4-5 contradicting) theory that 41 4/5 was calculated, and accept instead that Diller-DR’s theory fits better and lots more often? No, he concludes (MuJHA p.17) that his own hypothesized (central-to-countering-Diller) “calculator” must’ve screwed up. A stark example of the effect of cultism upon judgement. (Since there is no evidence for eq.4’s existence in Pytheas’ day, the MuJHA-hypothesized calculation must be alleged to have occurred far later, which much diminishes any excuse for imprecision.) Notably, Jones 2002E convinced not one among even his friends on the committee for the $1000 DIO van der Waerden Award.


which proved 2nd century BC use of sph trig, plus Hipparchos’ careful observation and mathematical use of the only accurate ε (eq.2) we know was adopted in antiquity: merely c.3’ off the truth (mostly rounding error).

B Correcting Meroë’s Misfiling Elevates Diller’s Score to 100%

B1 Since 1934 it has been known that the standout non-fit for the Diller theory is Meroë, the 13th klima. Meroë was the single seeming blemish in Diller’s tabulation, e.g., Table 1 of DIO 4.2 (1994) p.56, a table otherwise perfectly demonstrating the next success of the Diller-DR sph trig solution of the Hipparchos-Strabo data. But, then, this is not the first time that DR has (embarrassingly slowly in this case and others) finally followed in the tradition of Kepler and A.C. Doyle by realizing that the aggravating non-fit is precisely what can be beconking one on to new discoveries.

B2 On 2009/3/24 (30’ after independently happening upon Diller’s solution) DR at last saw the elementary reason that Meroë’s 11800-stade latitude became the sole non-fit:

Meroë at latitude 11800 stades is not a klima — it’s a city.

(Diller himself suspected this: JBS) I.e., 11800 stades for Meroë city should never have been in the Strabo-klimata tables of Diller 1934, Neugebauer 1975 p.305, or DIO 4.2 p.56 in the 1st place. The city-vs-klima distinction has been right before our eyes for years through the clue that Strabo 2.5.38 (see also chart at Neugebauer 1975 p.1313) provides explicitly in the case of Alexandria, noting that this city is separated from the nearby “Lower Egypt” 14th klima by 400 stades — this, though it was common in antiquity to casually call the 14th klima “Alexandria”. (Strabo 2.5.38 inadvertently does likewise: §F4.)

B3 The case of Meroë is complicated by the fact that there is both an “island” Meroë (described at Strabo 1.2.25 as the Nile’s largest: §B4) containing, in its north part, the city Meroë, whose actual latitude is L = 16°57'N. It is the city-vs-island confusion (as also for Rhodes) we will now eliminate. The very names of klimata illustrate that most were named not for cities but for sprawling regions — such as bays, straits, river-mouths, or islands. (In ancient geography, Syene [modern Aswan] is often just a sloppy amalgam of city, tropic, & klima.) This, because few if any important cities were likely (being tiny areas) to oblige by falling smack upon a klima. (This obvious point had become obscured by the time of

36 See, e.g., Rawlins 1991W §D2 and DIO 4.1 §3 §A5 [p.35]. Also the ancient galactic-circle Cystsegment on the Farew globe: www.dio.org/ifl.htm#gtgm.

37 See DIO 4.1 §3 fn 2 [p.33].

38 This realization is not our 1 restoration correc of the mis-filing of a famous item. See, e.g., DIO 8 §5 fn 5.

39 E.g., Pliny 6.212; Honigmann 1929 (The Seven Klimata and the Important Cities) pp.34, 40, 43, 52, 147; S&G p.116 n.4. Also Neugebauer 1975 pp.730&732, though at p.305, mathematician Neugebauer deluded himself into thinking his own theory better than non-mathematician Diller’s (an imagined superiority) insultingly expressed at p.734 n.14 — but now accepted nowhere) by: [i] Over-ruling Strabo 2.5.38 (and his own Neugebauer 1975 p.336 n.29: see below at §F4) so as to equate Alexandria city and klima at 21800 stades. [ii] Ignoring all klimata south thereof (this, while even knowing that his scheme didn’t fit them but the exiled Diller’s did). [iii] Skipping the 15°12 Pontos klima, where his scheme also failed. At DIO 4.2 p.55 fn 4, the Neugebauer theory (Neugebauer 1975 p.305) was reduced to a cubic polynomial (4 coefficients) \( L = 50[1^1 + 62^1 + 130^1 = 8454]. \) (If one tries a polynomial of high enough order, one can mimic any curve of the sort examined here. See www.dio.org/hiv/hlpm.)

40 Examples from the names of the Almajest 2.6&8 klimata: “Avalite Gulf”, “Lower Egypt”, “Rhodos”, “Mid-Pontos”, “Southernmost Britannia”, “Mouths of the Tanais [Don]”. And, as we now realize, the island “Meroë”. (Note: the Don klima was placed at c.54°N, e.g., Almajest 2.6&8: GD 3.5.24. The actual Don mouths are at c.47°N. Perhaps an ancient slip occurred when its distance north of one of our 3 trio was undone by confusion as to which southern site was the basis of the differential datum.)

41 Contradictory ancient definitions of Syene are touched upon below at eq.17.
Ptolemy’s GD — perhaps as early as Hipparchos. See DIO 5 fn 19 on commerciality.) So it would make sense that the 13th klima was for Meroe Island. (This is made explicit at Phny 6.620 & Almajest 2.6.) Moreover, we notice that the latitude differences in stades given by Strabo connected to Meroe are generally expressed with respect to other cities. Indeed, since these distances are (§C2) due to Eratosthenes (who probably did not use sph trig klimata) they cannot be klimata-based and their contexts usually do not discuss hours.10 When Strabo finally speaks of the supposed Meroe klima, he does not speak of a spot called Meroe (as elsewhere) but says (Strabo 2.5.36): “In the regions of Meroe and of [Ptolemy’s Hunting Lodge], the longest day [M] has thirteen equinoctial hours”.

B4 And Strabo 17.2.2 estimates the north-south extent of Meroe as about 3000 stades, which (even if [as he wonders] exaggerated) rather more than covers the 200-stade discrepancy between the value predicted by Diller-DR’s theory for the Meroe klima (11600 stades) and the city’s measured latitude (11800 stades) which has hitherto been mis-filed among the Hipparchos-Strabo klimata. Conclusion: Meroe at 11800 stades latitude is a city and thus (as noted at §B2) no more belongs in klimata Table 1 than does Alexandria city, which had thus already at the outset been eliminated by everyone but Neugebauer 1975 p.305.

B5 Diller 1934 p.267 realized the difference between the Meroe city & klima but supposed (like DR for decades) that Strabo had neglected to supply the klima’s L. Which brings us to repressing the shocker 1st revealed in DIO 5 (2009). By contrast to all his inter-city placements of Meroe city (fn 10): during his lone reference to the Meroe 13th klima, Strabo 2.5.36 hands us its latitude by stating that it is 1800 stades nearer Alexandria than to the Equator. As DR 1st realized 2009/4/1 (merely 5th before DIO 5’s online publication! — this, after 25º of delay in publishing Diller’s GD 8 ms in that volume, as long planned); since the context is klimata (not cities) and since the 14th klima is at 21400 stades (Table 1 was not an observation! (Zenith distance Z is the complement of altitude h.) Though, almost everybody else has realized it was an observation, as MuJHA n.11 creditably notes. MuJHA claims it was instead just a calculation, because (?) it was presented in such precise form. Jones adjacentally claims that 2 other gnomon ratios, both equiangular (Alexandria 3:5, Carthage 7:11), are empirical because of roundness, i.e., because the s and g are smallish integers. (Definitely an original argument.) But in truth, neither is empirical, as has been serially pointed out over several decades by (Jones-uncited) findings of Honigmann, Neugebauer, & DR. (See, e.g., Neugebauer 1975 p.336 n.29 and Rawlins 1985p pp.263-264 & n.17.)


Eq.11’s 100 stade difference vs Strabo’s 21800 stades for Alexandria (Neugebauer 1975 p.1313) is one of the three bases for Jones 2002E n.9’s proposed 100 stade shift of all the klimata. But such a shift would maintain Strabo’s L-differences, yet the difference between eq.11 is 32:28 instead contradicting the difference of 53:38 (900 stades). (i.e., why does Jones 2002E use the Alexandria discrepancy between eq.11 & Strabo’s L, while ignoring the corresponding Carthage non-discrepancy?) Carthage’s 7:11 ratio is obviously non-empirical, since 32°12 is waaaaay (over 4º!) too far south of actual Carthage, falsely distorting maps of the N.Africa coastline for the next millennium. And the explanation for this ancient disaster is the very same as for Alexandria’s true Strabo ms reading, namely 7:5. (Not 3:5, as MuJHA n.10 scrupulously notes.) Thus it is not the g/s ratio but is the longest/shortest-day ratio M=m for the Alexandria klima where 7/11 ratio for Carthage is not the M=m ratio but the 7/5. (Not 3/5, as MuJHA n.10 scrupulously notes.) Thus it is not the g=s ratio but the h/s ratio of such major cities (Meroe, Alexandria and Carthage: see fn 35 or DIO 5 fn 25. I.e., in the Strabo passages examined here, his
F3 MuJHA’s non-refered history is revealed by two slips.31 (At literally [F2] chapter-one Almajest sophistication.) Ancient astronomers’ “equinoctial” ratio $s_c/g$ (horizontal shadow length $s$, divided by vertical gnomon height $g$) isn’t “derived from the equinoctial shadow” (MuJHA p.16 emph added) but from solstitial observations (Almajest 1.12):

$$L = \frac{Z_w + Z_s}{2} \text{ obliquity } \epsilon = \frac{Z_W - Z_S}{2}$$

(8)

With $g$ standardized at 60 (Almajest 2.6; evidently 120) in Pytheas’ day: eq.10, the equinoctial equation is:

$$s_c/g = \tan L = \tan \left(\frac{Z_w + Z_s}{2}\right)$$

(9)

where $Z_w$ and $Z_s$ are the Winter Solstice & Summer Solstice local apparent noon zenith distances, resp, which are found via, e.g., transit circle (Almajest 1.12). Or perhaps by gnomon, the sort of observation analysed in eq.10 (which MuJHA p.17 centrally and inexplicably is sure is a calculation). Thus, all ancient “equinoctival observations” of $s_c/g$ (e.g., Vitruvius, Pliny, Ptolemy: see Rawlins 1985pp.262-264 & Almajest 2.6) are neither equinoctival nor raw observations — but are instead calculations, performed in several steps (eqs.8 & 9) from raw solstitial observations. In short, finding equinoctival $s_c/g$ involves a multi-step (eqs.8 & 9) calculational processing of two observations, while solstitial $s_c/g$ is just read directly off a gnomon (Pytheas) or a transit circle (Almajest 1.12). (The R.Newton discussion cited at fn 29 well clarifies similar problems.)

F4 MuJHA’s prong [b] 2nd try at gutting Diller 1934 is an argument for E’s obliquity having been used by Hipparchos: contra §F3, Jones claims that Pytheas’ famous S.Solstitial solar altitude (c.300 BC) shadow/gnomon ratio $s_c/g$ at Massalia (modern Marseilles)

$$s_c/g = \frac{41.4}{120} = \tan 19.5^\circ 12' = \tan Z_s$$

[12 eq.1]

(10)

whole-degree-rounded $L$, the chronological order of finding these data is unlikely to have been as suggested. Jones 2002E p.16’s pure speculation, that Eratosthenes’ Alexandria $L = 31^\circ$, is as unsupported as is the same page’s connexion of our eq.11 to him. By contrast, Rawlins 1982G p.264 used Eratosthenian data with expected solar-semi-diameter errors in a coherent argument to show that his Alexandria $L = 31^\circ 04'$ = 21750 stades, which accounts for the unusually precise stades’ 10s-place ending of Strabo 2.5.24, as well as Hipparchos’ Alexandria $L = 31^\circ 05'$ = 21760 stades (explaining the also-exceptional stades’ 10s-place ending of Strabo 2.35.9, as well as GD 4.5.76’s Pharos $L$, which is merely Eratosthenes’ $L$ processed through Hipparchos’ usual 5’ rounding. (See similar $E$–H rounding of 4’s to 5’ at §F3). These 2 Strabo passages are thus consistent with central site Rhodos’ $L$ being 25500 (city) for Eratosthenes & 25400 stades (klima) for Hipparchos.)

31 One key error is at p.15 line 6 [detected in 2002 by Thurston], confirmed by another at p.16 line 4. These are noted at DIO 11/1 p.26 n.1 & www.dio.org/cot.htm#ucmf. If one repeatedly chooses (though legitimate journals exist in the field) to publish in a forum which one knows perfectly well has a long record of slandering (see www.dio.org/gg.htm#hhwcl) to non-existent refereeing (and whose Editor-for-Life strikes hate-objects from his list of those scholars whom He might choose as His referees [*an offense which alone destroys a journal’s claim to having a reliable refereeing process*] even when He knows [DIO 6 §3 [1]] they are competent), one cannot be surprised when misunderstandings pass into print unapprehended. It is also disappointing to find an attack on DR in a journal from which appreciative citation of his work has been strictly barred for many years, even while DIO’s fair-debate doors are always open: www.dio.org/deb.htm. This, again, is the kind of improvidently-anticipated destruction of communication inevitably created by fawning on semi-numerate archons who alone for their own inductive sterility by such creativity as shunnings. The very same data that were for decades unreliably sacrosanct to Muffios (being the basis of the MuJHA-Prinetute Debate) turned out to be [a] twisted or [b] mis-taken versions of klimata at all. (Restorative restorations: [a] §F4 & [b]) such creativity as shunnings.

32 If Pytheas used a gnomon based upon 120 as a unit (as are the sine tables of Almajest 1.11) then did tangent tables already exist about 300 BC, allowing instant conversion of $s_c/g$ to $Z_s$, as in eq.10?

33 Question: why do classicists persist (as in the LCL version of this passage) in translating the Greek for ancient scientists’ ‘gnomon’ (gnomon) as “index”, when it is important that modern scientists analysing ancients’ work understand what instrument was being used?34 Strabo 1.4.4 & (2.1.12), 2.5.8&41. Some of the Strabo report has Hipparchos placing Massalia and Byzantion on the same 15°14’ klima. While Massalia is close to the implied 43° parallel, Byzantion

AND Strabo 2.5.38), we use this Alexandria klima latitude to solve for the Meroë klima latitude $K$ by simple arithmetic in stades:

$$K = \frac{(21400 - K)}{1800} \Rightarrow K = \frac{(21400 + 1800)}{2} = 11600$$

(3)

— precisely the Meroë latitude predicted at Diller 1934 p.267, over 3/4 of a century of Muffia sneering ago. Result: ALL FOURTEEN data fit the Diller-DR scheme. This is evident from our depictions of Diller’s triumph in Tables 1&2 and Fig.1: an astonishingly flawless record of, again, fourteen successive hits out of fourteen data. Has any comparable ancient astronomy discovery ever enjoyed such perfect verification?

B6 MuJHA 2002-2009 reaction? Strabo’s klimata data suddenly aren’t trustworthy anymore? As posted by DR (www.dio.org/cot.htm#dfme) a few days earlier in anticipation of Life strikes hate-objects from His list of those scholars whom He might choose as His referees (an offense which alone destroys a journal’s claim to having a reliable refereeing process) even when He knows (explaining the also-exceptional stades’ 10s-place ending of Strabo 2.5.39, as well as Table 1) — who typically team-permitted not a peep of doubt on the subject — are dumped just at the kind of improvidently-anticipated destruction of communication inevitably created by fawning on even while a journal from which appreciative citation of his work has been strictly barred for many years, even while DIO’s fair-debate doors are always open: www.dio.org/deb.htm. This, again, is the kind of improvidently-anticipated destruction of communication inevitably created by fawning on semi-numerate archons who alone for their own inductive sterility by such creativity as shunnings.

12 §F3 [B2] & F4. If not cult-bound, Jones’ classics expertise could’ve found eq.3 long before DR.

13 Has any academic cult ever matched the MuJHA’s gift for (1934-2002) rejecting virtual perfection in favor of a theory fitting (Table 1) less than half the available data? See www.dio.org/biv.htm#kpvs.

14 A dodge which only entered the debate in 2002 when the hilarious Diller-Princetute Neugebauer contrast of Table 1 finally caused Isis’ citation of the devastating display of it at DIO 4.2 p.56. To sum up: the very same data that were for decades unreliably sacrosanct to Muffios (being the basis of the MuJHA-Prinetute Debate) turned out to be [a] twisted or [b] mis-taken versions of klimata at all. (Restorative restorations: [a] §F4 & [b] such creativity as shunnings.

15 §J1 here — who typically team-permitted not a peep of doubt on the subject — are dumped just at the moment the cult is Isis-faced with the fact that these data much more convincingly back Diller, not his slanderer (in 58), Muffia-guru Neugebauer. One is reminded of the notorious BS 2001 JHA attack on Hipparchos’ authorship of the Ancient Star Catalog, based on assuming a high atmospheric density. When BS told DR of this plan (1999/10/1), DR immediately suggested the reverse: use Hipparchos’ established authorship to gauge ancient atmospheric opacity. (See www.dio.org/gad.htm#hnw for this and similar cases of mis-weighing evidences’ relative strengths.) In the present instance, it would have been wiser to realize that the steadiness of the fit of Diller’s math tells us that Strabo’s data (if not always his interpretations) are more trustworthy than some of us had previously thought — and that is yet another enlightenment owed to the original intellect of Aubrey Diller.
C Philo’s Geographical Systemmetry Verified

C1 Strabo 2.5.7 (emph added) describes Eratosthenes’ geography of the Nile: “from Meroë to Alexandria . . . is about 10000 stades; and Syene must lie in the center of that distance; so that the distance from Syene to Meroë is 5000 stades.” This statement has not generally been taken seriously, perhaps because of its numerical look, plus the myth of the Greeks as non-empirical. (See Rawlins 2008b §K4.) Yet it is in fact precisely accurate. The actual latitudes: Meroë 16°57′, Syene 24°05′, Alexandria 31°12′; so the gaps are each nearly 7°1/8; or, using eq.1 and rounding as usual to the nearest 100 stades: 5000 stades. C2 So the ancient finding of the equality of Alex-to-Syene and Syene-to-Meroë turns out to be impressively true: to ±1°. (And it is less likely to be based upon accident than the equally remarkable ancient record [also correct to ±1°] that Aldebaran and Antares were 180° apart in celestial longitude: DIO 2.1 §2 fn 5.) The basis of this geographical discovery: the carefully measured heights of the sun at noon. The heights are largely accurate3 to ordnmg 0°1. Rawlins 1982G shows that Eratosthenes had learned that Rhodos City’s L = 36°5/12 (good to 1′) — or 25500 stades — and that only his foolish use of gnomon for Summer (not Winter) Solstice threw off his measure of Alexandria’s L by half the solar semi-diameter, yielding 31°04′ (Rawlins 1982C eq.10, Rawlins 1994L fn 44). His place for Alexandria was, like Meroë’s, adopted (Strabo 2.5.7) by Hipparchos (who never visited Africa) and typically rounded to 31°11/12 & 16°11/12. Strabo 2.1.20 relates that an observer named Philo had taken astronomical measurements by gnomon at Meroë, and his statement (idem) that the Sun is at zenith 45° before S.Solst is encouragingly accurate.17 Strabo’s report of gnomon-use at first looks discouraging due to its systematic error from solar semi-diameter ssd. However, while outside the tropics, ssd will foul up the L half of eq.8 instead of the e half, the reverse is true in the tropics. A transit instrument would 16 Even the rough latitude for Athens (Strabo 2.1.2), 38°+1, is much better than Hipparchos’ later false value of c.37° (Hipparchos Comm 1.11.3&11), which became adopted in astrology manuals for centuries after, e.g., GD 3.15.22 & (see DIO 5) 8.12.18. (For speculative explanation of his error: www.dioi.org/fft.htm#rbbv.) This relates to DR’s contention (www.dioi.org/fft.htm#bpsj) that most astronomers (as against astrologers) knew Athens’ actual latitude, and that this may relate to the origin of the Farnean globe: of indicated home latitude 38° (presumably either Pergamon or Athens). 17 Rawlins 1994L fn 44. Strabo 2.5.39 confirms this by putting Hipparchos’ Alexandria 3640 stades south of the Rhodes 14°1/2 klima, thus at latitude 21760 stades or 31°11/12. Further if less precise confirmation: Strabo 2.5.38 says the transit of Arcturus is a little south of the zenith, consistent with the star’s quite erroneous 31° Hipparchian declination (Almajest 7.5). We have elsewhere proposed that since Arcturus was actually c.0°1 north of the zenith at Alexandria in Hipparchos’ era, he (again: §1) made a sign or translation error and subtracted c.0°1 from his 31°1/12 Eratosthenian Alexandria latitude to find the awful figure 31° (error −0°3) for the declination of Arcturus. 18 The actual interval would have been 46d. But we find that his figure is accurate to its precision, if we inquire as to how Philo determined the time of Summer Solstice: he would use equal-altitudes, so why not choose the two zeniths’ dates, for L = 16°57′ (assuming epoch c.270BC, though there is little time-sensitivity here) just under 91° apart? Philo would then find the S.Solst 1/2 way between those two dates and report the semi-arc as half of 91°—or: 45°.

of the all-too-predictable: “DR to Mufa: Is 13-out-of-13 Enufa?” See DIO 5 fn 22 and in-love-Osgood Gingerich at DIO 11.3 §6 ¶A1 on the Muffa’s decades-long tolerance of all manner of imperfection in Ptolemy&Neugebauer, even while (the source being of non-Muffa breeding) blind to a now-literally perfect fit. (Thereby inverse-fastidiously outnuttting Some Like It Hot’s original indefatigable old master Osgood, even while Diller-DR provides an exception to his Nobody’s-Perfect capper: idem.) As observed in ibid (e.g., fn 12): a cult which systematically, pseudo-effetely labels&treats others as cranks (www.dioi.org/cot.htm#stsl), while transforming journals & conferences into elaborate balls devoted to cranks’ favorite dance — dodging dissonant evidence — needs to fill several lacks: common sense, statistical sense, Occam sense, humor sense. And a mirror.

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numbers of Hipparchos who was glorious or notorious for varying his parameters throughout-out his career: §F1 — which can then be used to engender doubts of Diller’s [a] deduced obliquity (eq.2); & [b] data-base (Table 1: middle col.). MuJHA uses a two-prong attack. Prong [a]: At p.16 mis-taking a calculation for an observation. Reverse at p.17 (§F here). Prong [b]: More such confusion at MuJHA n.9. (§F below). E7 Putative JHA refe for MUHA missed the stark contradiction between §E5’s prongs [a][b] (such embarrassments inevitably issue from chauvinist lastidacht banzai-determination tryanythingitis): the = 23°51′20″ (eq.5) MuJHA pp.16-17 proposes in §F1’s prong [a], is inconsistent with that which would be produced by prong [b]’s proposed data-set-alteration. (Awful number details at §F1&F2.) We now examine the §E6 [a][b] prongs of Jones’ indiscriminate creativity (which he’s very fortunately corrected in DR’s work as well: see DIO 11.2’s cover), a classic Muffia vision of ancients as semi-empirical-number-jugglers (Neugebauer 1975 pp.642-644; DIO 1.1 §1 fn 24), contra Almajest 1.12’s description of e’s capture being via the outdoor transit circle presumed in Rawlins 1982G.

F Jones’ Subtraction from the Sum of Human Knowledge

F1 MuJHA’s initial knowledge-subtractive (fn 55) attempt to undertug Diller 1934 was above-cited prong [a] (§E6). Diller had been 1st to discover that Hipparchos’ ultimately adopted obliquity was the lost value 23°2°3, which is accurate (much better than the values MuJHA urges for H, implicitly&explicitly) and has since been validated in various ways (summarized at DIO 5 §D3). MuJHA pp.15&17 notes that Ptolemy ascribes to Hipparchos the Eratosthenes value4 = 23°51′20″ (eq.5). As §J5 notes, this is of little weight since we know (see the MuJHA author’s own Hipparchos entry in the Encyclopedia of Astronomy and Astrophysics 2000) that Hipparchos repeatedly changed parameters. F2 Putative JHA’s origin, Jones 2002E p.16 rigorously rounds L from arctanz(35°) = 30°58′ to a precision of whole degrees, 31°, in order to get the precise answer he is “straightforthly” (p.15) seeking below at eq.7. Via Strabo 1.4.2, he “confirms” (p.16) L = 21700 stades for Alexandria without noting that not just 31° but 30°58′ (and Rawlins 1982G) 31°04′ (the only L of the three which is independently relatatable to Eratosthenes: Rawlins 1982G) are also consistent with 21700 stades: i.e., no confirmation. When he subtracts the traditional Alexandria—Syene 5000 stades 31°−5000 stades/(700 stades/degree) = 23°51′26″ (7) he gets close to eq.5 instead of the 23°49′ (like eqs.16&17) which he would have gotten without that arbitrary 31° rounding. Compare via Occam’s Razor to an unjuggled solution (Rawlins 1982G) which simultaneously solves for precisely three for Eratosthenian data: [1] eq.5’s 23°51′, [2] L = 31°04′ (not 31°1), & [3] Z = 1/2 klima. Jones 2002E pp.15-16 basically says Strabo’s supposed (vs §F4) Alexandria “equinox-shadow-ratio” 3/5 & its implications. A key misimpression (§F3): L = 31° is said (p.16) to be “derived from the equinoctial shadow” though standard ancient measure of L got it via solstice29 (not equinox) data. The method is attractively simple (Almajest 1.12): just halve the sum of S.Solst & W.Solst app.noon zenith distances Z. But the process also automatically produces the obliquity e, if one just halves the very same two Zs’ difference. See eq.8. Therefore, unless an ancient astronomer deemed subtraction more challenging than addition, he would find e as part of finding L, so (don’t miss Jones 2002E p.15’s curious phrase “might suggest”): why compute already-known (via eq.8) obliquity e through the more laborious long-division required (above: eq.7) by the MuJHA p.16 method29.
E4 Is it really this easy for a few rebel scholars to cripple (into free-will-deprived zombiedom) a whole community, merely by inadvertently manipulating that community’s lethal mixture of [A] historical bigotry (the inertia of which tends to produce embarrassing non-priority in discovery-making), plus [B] the resultant sociological bigotry of embarrassing (thus silent) careerist cooperation in academically-outwitted power-genius archons’ vindictive exilings?24 May bigots’ turf-possessiveness be succinctly summed up as: Pigotry?

E5 Readers can examine this matter (bluntly condensed at www.dioi.org/cot.htm#tdps) carefully for themselves and then opt for which view to go with, on Diller’s grand discovery: Occam’s Razor? Or Muffia theology?25 I have friends who claim (2009) the Neugebauer Muffia’s bad side is dead. Reply: not until the truth of the Hipparchos-Sph. model is faced. Rigid, suffusive Muffia’s sum 9000 stades of a century is part of what academe’s ever- tolerated-as-normal archonality-shunning-rages inevitably lead to. And, as of 2009, Muffia-triggered shunning is as undue as ever in the history of astronomy zoo. For discussion of the mechanics & parallels of shunning’s automatic instant-community-braindeath on central issues of its own field, see DIO 1.2 §H2 [1991] (www.dioi.org/vols/w13.pdf pp.124-125); DIO 4.3 §15 §G9 [1994] (www.dioi.org/vols/w43.pdf); & DIO 14 §2’s Epilog [2008] on sorority dominatrices.26

E6 Sadly, an apostolic succession of modern cultists has outrageously defied common sense — and (needlessly) risked degrading still further their reputations vis-a-vis balanced scholarship — by fighting the obvious for four-fifths of a century, now, 1934-2015 (a span whose very magnitude has thus far only intensified the amorally unrepentant cultists’ never-confess determination to throw worse reputation after bad), even though their methods for doing so have run the gamut from [1] Babylonionly claiming a more historical theory than Diller’s (Neugebauer 1975 p.305 & p.735 n.14), to [2] a contentless argument that Neugebauer’s authority meant more than Diller’s (top Muffiose to DR by phone c.2000), to [3] randomly-walking or vaudevillianly ([K2] reversing field by jettisoning27 Neugebauer’s theory (after our Table 1 [originally 1994, augm. 2002] had made it a failed joke among neutrals) and now instead shifting to the coherently universally-understood coherent data-base (middle column of above Table 1: Neugebauer 1975 p.1313) upon which Neugebauer’s authority on the issue had been founded and accepted for decades. This final stuat-squirm (above, p.2) occurred in a curiously semi-Muffiose-theology apologia-paper Joneson2002E (frequently called “Muhuja” here to accent its cult-think roots). The new 2002 tactic: outside the beautifully consistent Table 1 (whose klimata were clearly published by Hipparchos as a whole at a single time), find some sort of inconsistency elsewhere in other coincidental that the 2002 JHA anti-crime-thought exercise appeared instantly after the Isis paper [citing DIO 4.2 p.56 Table 1] was circulating among cultists for referencing? See www.dioi.org/biv.htm#bouv.24 For one of the most flagrant deadbrain-kissing non-citation performances ever accomplished in the Ptolemy controversy, see at DIO 8 p.2 [1998] the details of the deliberate, systematic behavior (where the osculation factor trumped even the shun-factor: www.dioi.org/ffl.htm#msnc) which helped earn super-cowerer J.Evans his advancement into Assoc. Editor of the Journal for the History of Astronomy. (Evans has recently tried silkpursing a handy establishment goon by publishing his output at JHA 38:199-206 [2007], without realizing that the paper’s proposal lacks statistical significance [www.dioi.org/aeg.htm] or, indeed, perceiving that the paper is statistical at all.)25 From www.dioi.org/mot.htm#gdbb: “There is no agnostic so ready to embrace doubt as a believer when faced with . . . evidence . . . inconveniently-inconsistent with his undislodgeably-sacred tenets.”26

At www.dioi.org/vols/web/pdf.p3.1. Two pregnant questions are naturally suggested by the outré speculations of [K2] (and thus trying to forestall Diller’s great discovery: [i] Why do observers so rarely note that shunners are customarily less brilliant and by-definition less brave than shunners? (See, e.g., p.2 in 1; or www.dioi.org/snb.htm#htvi.) [ii] And why, in such situations, can one count on all but the best of the “science press” to undeviatingly, fawningly trust & promote the former, not the latter? — while censoring all mention of their gurus’ conceptual behavior. 27If this indicates that denigrating a DR-associated achievement has higher Muffia priority than whipping Muffia-godop Neugebauer, that’s some progress. But such little-steps (see 1998 note at DIO 1.2) haven’t taken us far in the last decade. Except backwards, via Muffiosei’s very littleness?

D Diller Vindicated: Early Sph Trig 2009 [Rev. 2015] DIO-J.HA 16 §3
get the correct L, but even if we assume18 that Philo used a gnomon at both solstices, he would have found (accounting for both rhp and sse) zenith distances ZW = 40°24’ — and ZS = 6°31’, yielding (by eq.8) nearly correct L = 16°56’, which Hipparchos would round to 16°11/2. From eqs.2&4, we have Syene klima at 24° or 16800 stades, thus not a bad Hipparchos L-threesome: Mer贫困村 16°11/2, Syene 24°, Alexandria 31°11/2: rms error 5°. (Notably, the GD errors for the same trio are —32°, -15°, & -12°, resp: rms error 22°.)

C3 Moreover, we find that the Hipparchos trio maintains (albeit slightly corruptly) the remarkable symmetry, presumably Philo-discovered19 (Eratosthenes&Hipparchos-adopted), that Syene is exactly 1/2-way between Alexandria & Meroë, the Hipparchan value20 for both intervals being 7°11/12 or (by eq.1) 5000 stades. In reality (using eq.1), both L intervals are even closer to 5000 stades (sun?975 stades); could this accidental symmetry be one of the causes of eq.1’s establishment? (By Philo? Sostratos? Eratosthenes? Anonymous?)

C4 Most revealing conclusion here: Eratosthenes’ outdoor-determined African city-latitudes (which non-peripatetic Hipparchos adopted: §C2) were from an era before latitudes were twisted ([B3 & fn 18) to conform to indoor-computed klima.

D The Birth of Spherical Trig

D1 The variables in Table 1, longest-day M (hours) and latitude L (degrees), are related by a spherical trig equation:

\[
\cos(15M/2) = -\tan L \tan \epsilon \quad \text{thus} \quad L = \arctan[-\cos(15M/2)/\tan \epsilon] \quad (4)
\]

(Almajest 2.3) where obliquity \(\epsilon\) was usually taken to be that of Eratosthenes-Ptolemy (eq.5) or nearby 23\°5/6, or one of Hipparchos’ two values (eqs &2, &2), the latter (23°2/3) being the exclusive and totally unavailable discovery of Diller 1934. (All three of these obliquities are discussed in, e.g., Rawlins 1982C, Rawlins 1985G, & DIO 5.)

D2 The Rawlins 1985G tables discovered that numerous major cities’ \(L / M\) did indeed correlate with either Eratosthenes’ obliquity (Almajest 1.12) or the early Hipparchos obliquity

\[
\epsilon_H \approx 23°51'20'' = 180° \cdot 11/83 \quad (5)
\]

or the early Hipparchos obliquity

\[
\epsilon_H \approx 23°57' \quad (6)
\]

D3 The Rawlins 1985G tables showed for Ptolemy’s GD:

[a] The major cities correlated with Eratosthenes’ eq.5 or 23°5/6 included Babylon, Korinth, Kyrene, & Meroë — all related to Eratosthenes’ birth or writings.

[b] The major cities correlated with Hipparchos’ eq.6 included Arbela, Athens, Carthage, Nicaea, & Rhodos — all related to Hipparchos’ birth, life, or writings.

18 Possibly Strabo made no distinction between asymmetric gnomon, symmetric gnomon, and transit instrument. Regardless, it appears that Philo was discoverer of the later-canonical A-S-M symmetry, which was abandoned by the time of Ptolemy, whose intervals were: A-S = 7°11/12 or 5000 stades. Ptolemy’s klima—city Meroë confusion caused a 1°/2 discord between his & Hipparchos’ L, hinting that Hipparchos was not responsible for the GD’s klima-polluted L mis-geography.

19 If Philo travelled to Meroë, he must have visited Syene. So he presumably knew that its latitude was 24°05’. And every scientist but Eratosthenes ([C2, Rawlins 1982G, Rawlins 1994L Table 5]) then knew that Alexandria’s L was nearly 31°15’/5, so the GD’s L was 24°, clearly the better symmetry. Thus Ptolemy’s 7°11/12, as a tentative guess of public GDP symmetry was not only true but completely known to be true in Alexandria’s community of genuine scientists, which again excludes Eratosthenes. His & Hipparchos’ later symmetrical A-S-M schemes were (as just noted) slightly less accurate than the presumed original latitudes (of, e.g., Philo) but were perhaps nudged to ensure adherence to an A-S-M symmetry likely well-known long before either’s geographical scheme.

20 Doubting makes Hipparchos’ Meroë-Alexandria distance 14°11/12, so (eq.1) not 10000 but 9900 stades. But Strabo (1.2.7 & 17.3.1) says “about” 8000 stades. This favors only 11/12 (and thus Philo’s accuracy) as Eratosthenes’ & Hipparchos’ Meroë L, rather than 16°5/6. Either satisfies 11800 stades.
Diller Vindicated: Early Sph Trig

Since §3’s correlation [a] was found via the sph trig of eq.4, we have here (also Rawlins 1982N n.11) a shaky suggestion that (contra §1) sph trig was known in the 3rd century BC. Indeed, there is even a hint (§2 fn 32) that trig may go back to c.300 BC. Trig’s absence from surviving mathematical texts (e.g., Rawlins 2008Q fn 32) has been taken to indicate its late appearance; but another possible explanation is that trig was long scorned (by academically powerful pedantic pure-math geometers) as a mere engineers’ tool which should not foul mathematical treatises. (The potential analogy with Isaac Newton’s presentations in his Principia is obvious.) Powerfully against this theory, however: Eratosthenes’ important geographical parallels (e.g., Meroë, Athens, Hellespont, Borysthenes) appear to be unrelated to klimata calculations. (The eq.2 calculations via eq.5 in §3 [a] seem to be Ptolemy’s, not Eratosthenes’.)

If known to Eratosthenes, the simple double-sunset Earth-measure method (requiring sph trig) would have faced him with the large disagreement between the lighthouse method’s 256000-stades (likely known before him: Rawlins 1982N p.215 & Rawlins 2008Q §1) vs the sunset method’s 180000-stades. (The latter being the Poseidonios-Marinos-Ptolemy value which eventually became dominant. Conversion discussed in Rawlins 2008Q & Rawlins 2008S.) Did he face the disjunct? (See detailed discussion at DIO 5 fn 18).

Figure 1: Latitude L graphed in dark (filled) dots as function of longest day M, sph-trig-computed via Diller’s eqs.1, 2, & 4, at all 14 M for which Strabo gives Hipparchan L: M in hours; L in stades, rounded (like all Strabo klimata) to 100s. Each Strabo datum is marked by the same dark dot, since Diller’s sph trig theory matches perfectly for all 14 cases. Asterisks mark the corresponding L for the arithmetical folly (fn 7) which Neugebauer inceduclly Princetooted his horn for. North of Rhodos, the dots&asterisks nearly merge in most cases (former smaller, so superposition won’t prevent seeing both), but the Neugebauer theory’s failure at lower L (most amusingly at Equator: §6!?) is lethally blatant. Hollow dots mark the 7 klimata of Rawlins 1985G p.263’s reconstruction of the pre-tempered original of the scheme underlying Pliny’s “circuli” (fn 48), showing how neatly the ingenious device of the ancient creator (§1: Hipparchos?) tracks klimata for the slim range of Mediterranean L which it was invented to fit via Diller’s Hipparchan ϵ: fn 50. Jones’ “unshifting” all Strabo L by 100 stades would be hard to show in our graph since the amount is so tiny (11/7) that the shifted points would be inside the dark dots marking Strabo’s data. The fit is so fragile that such a minuscule shift destroys (§3) any chance of fitting eq.4 to the Strabo data, regardless of ϵ-choice. So the graph’s larger message ironically redounds against Jones’ §1 prong [b] wetdream that “one or two modest changes in the intervals” could best Diller: the above curve is too super-precisely characteristic of sph trig eq.4 & Hipparchan ϵ to allow explicit or even implicit denial of credit to Diller as discoverer of the true basis of Strabo’s Hipparchan klimata, which lay secret for 2 millennia.

E Cripples, Bigotry, & Pigotry: the Grovels of Academe

While such speculations provide no proof that sph trig was known to Eratosthenes, Table 1 proves positively that sph trig (eq.4) was known to Hipparchos, as Diller 1934 was 1st to prove. (A powerful array of the evidences for sph trig’s use in Hipparchos’ century is brought together for the 1st time at www.dioi.org/cot.htm#mmmzs.) But, at a time when the hist.astron field is run by “just a bunch of politicians” (as an aghast eminent astronomer describes the field’s debate-fee ling dominatrixes), the reaction to such a massive demonstration of said gang’s fallibility is predictably Doc-Cookian: deny, deny — never confess. (See Joey Bishop at DIO 11.2 [2003] pp.32-33.) It would be merely pathetically funny if it weren’t so damaging to the balance of communal micro-judgement here and macro understanding of the entire subject of ancient astronomy.

Continuing obfuscness (§E1 item [c]), defining Diller 1934’s multiple [now SEVEN-FOLD] predictive vindications (DIO 5 §3D, is noted at DIO 11.1 p.26 fn 1 item [iv] — as well as the even more revealing fact that the near (now total) perfection of DIO 4.2 Table 1’s fit is not mathematically challenged (or challengeable) nor is this literally PERFECT fit even mentioned by the history of ancient astronomy community: As of 2015, that’s 21st of cultist bibliographical decei 25 by this odd community, which only adds to the parallel disgrace of not even comprehending the statistical and Occamite preferability glaring from Table 1.

If Eratosthenes and-or his critics tried both the lighthouse and sunset Earth-measure methods via the Pharos, the azimuths would be different since land beyond the point 202 stades away (where the Pharos flame became invisible: Rawlins 2008Q) would render clean settings of the Sun’s disk impossible, so viewing sunsets from the Pharos would be at more northerly azimuths. Strabo 2.2.2 is chronologically valuable in its implicit suggestion (perhaps contra the nonetheless intriguing and original analysis of Taisbak 1974) that Poseidonios was indeed the 1st prominent adopter of the much smaller circumference 180000 stades cited to him at Strabo. But it should be noted that the publications occurred due to the openness of two historians: Olaf Pedersen and Margaret Rossiter, respectively — to the credit of both. (As asked elsewhere here: is it