Ancient Spherical Trig:

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

Diller Verified on Klimata After 75\(^\text{y}\) Shunning & Mob Hits

JHA’s Subtraction from the Sum of Human Knowledge

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

Pytheas Observatory Located
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History vs Unfalsifiability: Crock of Ages Clefted But
Unquestionably Proven

In recent years, studies of ancient astronomy have yielded several significant findings. One of the most notable developments is the work of Mufa-Toomer, who has shown that Hipparchus' klimata were consistent with spherical trigonometry, using accurate values for the obliquity of the ecliptic. This has led to a reevaluation of the methods used by earlier astronomers and the development of new models for ancient astronomical calculations.

Mufa-Toomer's findings have been met with skepticism, as some have argued that they are not falsifiable. However, the evidence for the correctness of Hipparchus' methods has been overwhelming, and Mufa-Toomer's work has been widely accepted by the scientific community.

The Conservative Aspects of Dio's Triple Eclipse-Induction

Dio's solutions (www.dioi.org/cot.htm/#jqsk) of all previously unsolved ancient lunar 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#szpt), assuming Greek-Selukid use of no-loss, 13th century BC eclipses. Finding no mismatches or alternate eclipses to explain the observations, Mufa-Toomer just scoff at data-remoteness. But neutral experts' dates for Babylonian observing are consistent2 with Dio's theory; Isis 83:474 (1992): c.1350 BC.

2 The NYTimes article (link to full version: www.dioi.org/cot.htm#zqosj) notes that Dio opponents display a lamentably common mental impenetrability. Cooperatively proving the point: during our 1991 & 94 (see Dio 4.2 p.55 & fn 2) 46 St John (above) 5. 56.9 & fn 788. Shirt-stuffings are entirely reactive to truth-wars by usual establishment anti-rebel ploys: money, shunning, money, censorship, money, 'experts', money, moans, threats, money.

A卦 Among the numerous gymnastic hysterical-astrology pratfalls enlivening JHA's hefty (64 pp) James Evans 1987 double-lead-paper attack2 upon (then-minority) Ptolemydouble-busters was [JHA Editor-to-be] Evans' lordly illusitration 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°, thus according to him (iden) 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 (ident & p.235) Hipparchos' two hugely disparate Spica data (explained below: §B) which disagree by over 1°. He then draws for us a Mufiose 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) 11 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 Evans' and Hipparchos' errors WERE NOT reversed. Similar integrity: see also §A2. As just noted: said irony's core was revealed1 in Rawlins 1991W fn 288. Contra Evans, neither his own nor Hipparchos' problems were observational. Instead miscomputed 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 ϕ differed virtually 1°/5. So Evans' sign-confusion created a huge net error because, whereas longitude parallax ϕ (the difference between topocentric [observer-centered: outdoor-visible] and geocentric [indoor-tabular] longitude: eq.2) is obviously supposed to be ADDED when converting the geometric lunar longitude into a topocentric (observer-centered) longitude, Evans instead SUBTRACTED it as if reducing

3 Scrfulins only a conventional figure: but Dio was unaware of the 1350 BC data. Yet seeking ancient lunar theory, so our 13th century BC results constitute an entirely independent TRIPLE-consistency.
Hipparchos’ Eclipse-Star Data 2009 December DIO-J.HA 16 ¶1

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 alibiting Ptolemy & showing up skeptic R.Newton’s supposed naïveté about observational astronomy — are fallacious.) In Rawlins 1991W fn 288, it was remarked that the theory was “bogus.” The following paper will provide (§B) for the 1st time full reconstructions of Hipparchos’ math for these two Spica-misplacing eclipses, and then will go beyond, with an exploratory application (§E) to the only other extant Hipparchos eclipse, which we discovered was used to position his hitherto-inexplicably ultra-misplaced fundamental star Regulus.

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 rigidity scheme: just measure how far a star is from the Moon at mid-eclipse, when the Moon is guaranteed to be virtually (though see fn 19) 180° from the Sun.

B2 Ptolemy tells us (Almajest 3.1) that Hipparchos used the eclipses of −154/5/21-22 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 ording it a millennium is that it showed that one could prove anything with statistics. (Is the Mufa aware 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 favorite mongoose, Leroy Ellenberger, 3929 Utah Str, 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 what relation such a truism has to statistical findings it loathes, e.g., Rawlins 1990L. Merely doubting statistical results in general is a pathetic pose. It should be added that two expert mathematicians (K.Pickering & H.Thurston) have already checked and verified in detail the math of the 1994 paper — a paper showing that Ptolemy not only stole the Catalog but clumsily attempted to hide this theft by the very method R.Newton 1977 had charged. Yet, Mufa publications — with their usual respect for academic decency & honesty — arrogantly continue to learn nothing from these results, in order that they may go right on profitably peddling their hero-plagiarist to the world as The Greatest of ancient astronomers. And such scholarship is published without a blush by centrist forums. Rarely does selectivistically-scattershot agnosticism scrape this low in the barrel. Rawlins 1982C’s simple statistical proof (Tables IV&V) that the Star Catalog was stolen from Hipparchos by Mufa-hero C.Ptolemy, was similarly attacked (JHA 23.3:173-183; 1992/8) by Mufa capo N.S., a disaster undercut by several freshman-level Swerdloff goofs (immediately revealed [1992/10] at DIO-J.HA 13 §8 C). Since that contretemps, a general Mufa discounting (even re-invention of the whole field of statistics seems not only expected but downright inevitable.

an outdoor toposcopic observation to find geocentric longitude. Thus the sign mixup would naturally cause an error of about −2°3/3 or −40′ — 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).3 After correcting for this Muff, we can verify the admirable smallness of the 1981 observational error of Evans (a dedicated student of ancient instrument and possessor of a steady hand, since the cross-staff requires it): merely one or two arcmin — just the sort of accuracy DR has consistently3 ascribed to the best ancient-eyedex observations.

A3 Only a scholar catering to modern Hist.astron’s cult-klan could straightforwardly propose the Zonal- to Zonal- the looser 2°4/3 — observational and so by implication helps excuse 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 author of Ptolemy’s faked “observations” (Almajest 5.12-13) is also off by 2°/3. (See discussion at R.Newton 1977 pp.182-191. Also DIO 8 §1 fn 13.) Scribbling a drawing will give one an idea of how ridiculous this is: mislocating a disk so grossly that the really and theoretical disks (1/2 wide in these lunar cases) don’t even come close to overlapping,4 the very feat Evans misclaimed he’d personally achieved in 1981 and is now too embarrassed and too steeped in Mufa academic integrity4 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 “observationals, the Mufa 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 Mufosi (who don’t include philosophy of science) to ponder a simple question: if devotion to our favorite positions keeps leading us into embodying crackpot-level muffs (e.g., §A1 & DIO 2.3 §8 §§C10-C15), does this not suggest that said positions are less than completely secure5?

4Both 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.


6Also true of all four of Ptolemy’s Almajest 3.1&7 solar equinox-solstice “observations” of the Sun, which agree 50 ten 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 n.50’s misadmonishment (§A1) is repeated in his later book: Evans 1998 pp.256-259; but this (post-DIO 1.3 p.288) Spica sermon quietly avoids discussion of his Seattle observation of the 1981/7/16 eclipse in this connexion (just photo at p.48, 100s of pages distant from his Hipparchos-Spica comments), shifting attention instead to the previously unadduced eclipse of 1977/4/3, seen from Spokane. (Why must Evans go back 21½ 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° to find a usable eclipse? Implication: 1977 is ere 1981, and JE here has his signumanship 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 2.1.4 fn 18 & DIO 11.2.2 cover.) Or admit the falsity of his alibi-for-silence-on-errors pretense (DIO 9.1 p.2) of not reading DIO. (Had he faced reality on Regulus at Evans 1998 pp.259f, 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’s http://www.dioi.org/cot.htm#Nsnr & http://www.dioi.org/dioi/ are justly peddling to the JHA’s M.Hoskin, hist.astron’s own Lord Sommers [DIO 2.3 §1 fn 18]. Who’ll dispute the aptness?)

Another corrugative consequence of a cult’s living with the shame of knowing that its sacred mission (hypping derivative Babylonian astronomy & Ptolemy as original genius) is unadmissibly indefensible.

Indeed, Muffa desperation to reject non-cult common-sense has now reached the point where the clique has even (presumably unknowingly) brought in Velikovskian-circle expertise to denigrate R-N-DR work. During my 1995/2/26 chat with B. van Dalen, he mentioned that the reason his (generally wonderful) paper van Dalen 1994’s n.1 had cited the 1989 Fomenko et al paper (which, with
For each eclipse, Hipparchos’ method was:

[a] Measure by armillary astrolabe9 the actual longitudinal difference $\Delta \lambda$ between the star, at longitude $\lambda_s$, and the mid-eclipse Moon at observed (topocentric) longitude $\lambda'_M$:

$$\Delta \lambda = \lambda_s - \lambda'_M \quad (1)$$

[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 $\lambda'_M$:

$$p_\lambda = \lambda'_M - \lambda_M \quad (2)$$

[c] Without applying the equation of time, find via Hipparchos’ PH theory10 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:

$$\lambda_M = \lambda_S \pm 180^\circ \quad (3)$$

[d] Adding eq. 1 to eq. 2 and subtracting eq. 3 produces an equation for the desired stellar longitude $\lambda_s$:

$$\lambda_s = \lambda_S + p_\lambda + \Delta \lambda \pm 180^\circ \quad (4)$$

B5 For the –145/4/21-22 eclipse: the outdoor longitude difference $\Delta \lambda$ (between Spica & the Moon) at the time when Hipparchos’ indoor luni-solar theory predicted mid-eclipse (23:58 Lindos Mean Time),11 was about $-33^\circ.8$, so he likely measured close to $\Delta \lambda = -33^\circ.5/6$. [b] Hipparchos’ PH solar theory12 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&5) 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 §S1&N8. To repeat the note made there at the time: we thus have no evidence of the equation of 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 $0^\circ.4$-amplitude & $0^\circ.2$-amplitude periodic errors of the Hipparchos-Ptolemy solar&lunisolar theories, resp. (Hipparchos worked by apparent time, though PH is for mean.)

11 Hipparchos’ likely location on the island of Rhodes (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) most timekeeping was by Local Apparent Time, customarily via sundial. Hipparchos’ clock-stars (Hipparchos Comm 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 12h 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.)

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

B6 For the time of –134/3/20-21 tabular mid-eclipse (just before 3°), Hipparchos’ outdoor measure of $\Delta \lambda$ would find close to $-2^\circ.3/4$. [b] Hipparchos’ PH theory gives solar $\lambda_S = 357^\circ.1/4$. [c] The Almajest geocentric lunar distance is 64°.9. [d] Thus, for latitude 36°N, Almajest 2.13 $p_\lambda = -15°$. [e] So eq. 5 yields:

$$\lambda_s = 357^\circ.1/4 - (-1°/4) + (-2^\circ.3/4) - 180^\circ = 174^\circ.3/4.$$

B7 We note that both results (eqs. 6&7) exactly equal the quite inaccurate (and even more grossly disparate) $\lambda$ values reported at Almajest 3.1. (See §B2.)

B8 These matches strongly suggest the validity of the wrong-$p_\lambda$-sign hypothesis. They also offer other historical information, which we turn to next.

C The Hipparchos Lunar Model’s Scale

C1 As we know (e.g., Rawlins 1991W eqs.23&24 and 3R), Hipparchos used several different lunar distances throughout his career. If his mean distances assumed for the present parallactic computations differed drastically from c.60°, this would affect $p_\lambda$ inverse-proportionally. The fits attained here suggest that he or his computers used conventional values during the period of the present calculations. Which is consistent with our finding at fn 14.

C2 One can argue for nonpreliminary Hipparchian mean lunar distances of from 52° to 67°. (See Rawlins 1991W eqs.23-24 & 3R1.) But use of these values instead of Ptolemy’s (59 Earth radii; Almajest 5.13 & Toomer 1984 p.251 n.49) will affect eqs. 6-8 by only a very few very tiny error. Nonetheless, though the present eclipse analyses (as well as fn 14) can work for 67°, they won’t for 52°. A reasonable conclusion is that we here have come upon indications in favor of Gerald Toomer’s finding (see, e.g., Toomer loc cit) that Ptolemy’s 59° lunar mean distance was that of Hipparchos.

D Hipparchos’ Sph Trig Reconﬁrmed by His Parallax Corrections

D1 It has long been recognized (e.g., Neugebauer 1975 p.323) that parallax tables were in use in the 2nd century BC. (This was always obvious from Almajest 5.5, but perhaps no one has previously caught the implication for the onset of spherical trigonometry. Neugebauer loc cit explicitly contradicts it.) These tables were essentially the same as...
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 (Almajest 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 complained of (§5 & §KB2) 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-signed eclipse method); however, Regulus is the zodiacal bright star with the largest negative Hipparchos λ error for his Ancient Star Catalog’s epoch — (126.28: Rawlins 1991H fn 44: — 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 about -1°/4, though that from Ptolemy’s tables was +19° for his ludicrous lunar distance of 43 Earth-radii. (The Neugebauer 1975 p.02 value [16°] is explicitly based upon Ptolemy’s simple syzygial lunar model, not his final one.) Obviously, Hipparchos did not share Claudio Indoor Ptolemy’s notorious belief that the Moon’s angular size varied by a huge factor (of up to nearly two yards). Indeed, the smallness of Hipparchos’ 12°1/2 parallax for the -16/2 observation indicates that his parallax calculations used a conventional lunar distance (as we already realized at §C1). We can check this by computing via modern theory the lunar parallax on the assumption that the geocentric lunar distance was 60 Earth-radii (vs 57 in reality): 14°; thus correcting Hipparchos’ -126/2 observation of topocentric lunar longitude 351°2/3, we have 351°26’, for which the nearest Hipparches approximation would be 351°38’, which is just the Hipparches geocentric longitude reported at Almajest 5.5 (Neugebauer 1975 p.92).

E2 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] p.33 & p.34; A.Diller’s indication before §E3 [E4, www.dio.org/geom/ehn/sheep-astros.html]: Our outstanding mystery here is Regulus’ perplexing Hipparches super-misplacement (§E1), and our so-far successful theory is that eclipse-parallax-sign-error accounts for Hipparches’ horrible stellar longitude error. If the eclipse is valid, can it also explain the only other attested (Almajest 7.2) Hipparches stellar longitude, the very worst of the lot: Regulus?

E3 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 — 140/1/27-eclipse (the only other Hipparchos-observed eclipse record we have: Almajest 6.5&§B9).

E4 For the -140/1/27-eclipse: [a] At tabular mid-eclipse (22°), actual Δλ was 5°/3, so (especially given his now-prominent proclivity for integral19 data), he likely expressed the measurement as exactly Δλ = 5°. [b] Hipparchos’ PH theory gives λ35 = 305°9/0 (Almajest 6.5 makes it 305°9/8), so he would record λ35 = 125°1/6. [c] Almajest lunar theory distance = 54°34/3. [d] So for Rhodes, Almajest parallaxes, p33 = 29° ± 1°/2 which would become -1°/2 after sign-mistake. [e] So eq. 5 yields, adding in 8° (1°.6 of Hipparchos-Ptolemy 1°100’ precession21 (from -140 to catalog epoch -126.28):

\[ \lambda = 305°1/6 - 1°/2 (+ -5°) - 180° + 1°/6 = 119°5/6. \]  

(8)

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).

Regulus longitude mystery: Shevchenko 1990 had proposed that Hipparchos’ Moon-star fundamental astronomical was in the evening, and Rawlins 1991W fn 138 had remarked on this proposal’s redemption by Rawlins 1991H §G1. The only important exception seemed possibly 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.)

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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 –15′ (20′ offset23 from Regulus’ error). [b] Hipparchos stayed with his –140 Regulus λ fixed by eclipse, even after –134 indication (via Spica) of the method’s unreliability.

E7 Late Ptolemy works’ use of Regulus as a foundation-point suggests that Hipparchos held Regulus as a pivotal star in his astronomy, which could help 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 §E2 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&K.)

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 §§1.) 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.24 [b] Developed nested calendar (Rawlins 2002A fnn 14&17) and durable luni-solar theory. [c] Likely invented the clever “circuli” scheme (§3 §11). [d] Determined accurate obliquity. [e] Took accurate solstices [DIO 20 2 Table 3]. [f] Oversaw creation of his ever-remembered Ancian Star Catalog, the oldest extant detailed compendium of the starry heavens.

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23I have elsewhere (Rawlins 1991W fn 253) suggested that Hipparchos’ stable of mathematicians sometimes operated independently of him. (Perhaps occasionally with eyes aroll.) The nonrelation of Regulus to Leo’s Catalog stars hints at an instance of this.

24All 4 observations examined here (3 ancient & 1 modern), superficially in error by ordmag 1°, fit our parallax-sign-slip theory and so turn out to have been accurate to within a very few arcmin. The accuracy of Hipparchos’ draconitic month was a pinnacle of Greek empiricism: DIO 11.1 3.

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 (§3 §G1) as an able mathematician, astronomer, and geographer. In the history of the exact sciences he is primarily remembered for his Summer Solstice observation (§3 eq.10) of the shadow/gnomon ratio at Massalia at Local Apparent Noon:

\[ \frac{g}{s} = \frac{41.2}{120} \approx \tan 19°12' \] (1)

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 telescopic measurement.

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

[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., §1 fn 15 & §3) dispensed with a 2002 Mufa-descended last-gasp attack upon one of the glories of rational scientific history — specifically: upon Aubrey Diller’s immortal priority in proving Hipparchos’ use of spherical trig and an accurate obliquity in the 2nd century BC. But we happily have a positive outcome from the Mufa’s 75° “hubbub” on the Diller issue (to borrow MuJHA p.15’s flip sneer 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°1 south of Marseilles—harbor proper (Fig.1).
A2 MuJHA p.17 having claimed that the Summer Solstice datum (eq.1 or $\frac{3}{3}$ 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 (eq.5) to the Solst zenith distance $Z_0$ of the top (not center) of the solar disk: the upper limb. (I.e., measured $Z$ will be $16'$ [the solar semi-diameter $ssd$] less than the $Z$ of the solar center, a fact many well-known Greeks were naïve about.) Thus, a solstitial $s_0/g$ with such an instrument will produce a latitude $L$ which is $16'$ less than the true value. A useful $1^{st}$ estimate of the uncertainty in Pytheas’ $Z_0$ 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:

\[
s_0/g = \frac{41\frac{2}{120}}{2} = \tan 19^\circ 11' \quad \& \quad s_1/g = \frac{41\frac{4}{120}}{2} = \tan 19^\circ 13'
\] (2)

Thus, crudely:

\[
Z = 19^\circ 12' \pm 1'
\] (3)

A3 But we can improve the precision here by examining\(^4\) ancient rounding even more finely than at §A2: if Pytheas’ reading (of his 120-unit-high gnomon) were nearer 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):

\[
\frac{41\frac{3}{4}}{120} + \frac{41\frac{1}{2}}{120}/2 \rightarrow \frac{41\frac{19}{120}}{2} \pm \tan 19^\circ 12'.2 \pm 0'.5
\] (4)

\(\frac{3}{3}\) The Greeks’ proclivity 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’s random error is unlikely for careful use of a vertical gnomon. And the experiment is easy to render so precise that the main non-ssd 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 at-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.)

\(\frac{4}{4}\) Subtracting ssd = 16’ from eq.5 shows that if Pytheas knew the correct obliquity (but didn’t know of the gnomon’s ssd-error), he would have thought that his observatory was at about $L = 42^\circ 56'$.

\(\frac{5}{5}\) We are here taking it for granted that 41 4/5 was Pytheas’ original raw datum. (And the original reading would probably have been in shadow/gnomon terms.) Yet we may test the faint possibility that whatever the original reading was, it came to later antiquity as 19° 1/5, and only subsequently (in a trig era) was its tangent calculated as a fraction of 120. (But such an assumption itself assumes ancient tangent tables [none have survived] and that these were based upon unit-120, though division of a tabular sine by its complement’s sine would cancel their 120-denominators.) However, [a] It seems rather a stretch to suppose that a later ancient would go to such trouble, to turn around the data-reduction process in order to “reconstruct” a lone pseudo-raw datum. Why would such be preserved as special? [b] A firmer objection is that, if $Z$ were 19°1/5, §3 eq.15 would not yield its (attested) sum.

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.
**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 \( ssd = 15.8 \) and \( r\&p = 0.3 \), with [for epoch \(-310\pm25\)] obliquity \( 23^\circ 44'.0 \pm 0'.2 \), error from uncertainty of Pytheas’ exact epoch):

\[
L = 19^\circ 12'.2(\pm0'.5) + 23^\circ 44'.0(\pm0'.2) + 15'.8 + 0'.3 = 43^\circ 12'.3 \pm 0'.7
\]  

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 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 \( 43^\circ 11'.6 \) to \( 43^\circ 13'.0 \) 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 \( 43^\circ 3.3 \) 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.
[a] Pytheas’ clear precision was \( \pm0'.5 \) (eq.4), not \( \pm0'.1 \) (c.10 times looser).
[b] The actual possible accuracy for a plain meridian observation has a similar error-bracket. On these bases, DR proposes accepting the theory that the measurement (with the error indicated in eq.5) was as accurate as its precision — and then investigating whether there is independent confirmation that it has provided virtually the exact latitude of Pytheas’ observatory.

**B4** Obvious next step: we check modern maps\(^5\) of the Marseilles (Massalia) region: Figs.1&2. And we thus find that the best spot an ancient astronomer could have picked near Marseilles is a few miles south of it (Fig.1), the southern part of a peninsula now called Cape Croisette. Its southern coast offers an observatory-dream unobstructed southern vista over water. (Like Tycho’s equally well-chosen observatory at Hvin; similarly, Eudoxos’ at Knidos and [DIO 4.1 \( \S3 \] [E] Hipparchos’ at Cape Prassonesi [the southern tip of Rhodos] for his southern stars.) Central novel realization here: the southern part of the Cape Croisette peninsula is a far better location for an astronomical observatory than Marseilles itself, which (Fig.1) faces westward on the water. And what is Cape Croisette’s location? It is at latitude \( 43^\circ 2.2 \) N (longitude \( 5^\circ 3.3 \) E) which neatly matches that found via eq.5 from Massalian Pytheas’ S.Solst observation.

**C Exploring for As-Yet Impossible Exactitude**

**C1** We can enjoy further speculation by asking what an astronomer would be looking for in this region. Note (Fig.2) that the easy coastal road, over pretty at terrain (today called Boulevard Alexandre Delabre), runs into un-negotiably steep coast and mountains about where the Cape Croisette coast turns the corner and starts trending eastward instead of southward. An attractive prospect for the Pytheas observatory’s location is on the tiny spit of land that is the extreme west extension of Cape Croisette: a wide hill, about 50m high\(^6\) — almost exactly the height of Tycho’s observatory — just high enough to not-infrequently be above the nocturnal aerosol layer. \(^7\) It is marked on Fig.2 as having been the site of “Anc. Batt.” (old battlements). Despite its modest height, the hill has a flat water horizon to the south and of all the likely prospects considered here for Pytheas’ location, this would have

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\(^5\)As with DIO 14 \( \S3 \) F’s discovery (www.dioi.org/gad.htm#blsl) 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.

\(^6\)The topo-curves are at 20m intervals for each of the accompanying maps here.

\(^7\)Our thanks again to Nels Laulainen for his 2000-2001 expert advice to DIO on such matters.
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°21'21.0'' 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 likely location considered here. If Pytheas’ 120-unit-high gnomon was 120 Greek feet (a Greek foot being 12’0''1/7 in modern measure), the high, steep cliffs of Maire (Fig.3) might allow a mostly natural gnomon of such height (which would ensure negligible imprecision from diffraction): the gnomon’s verticality 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: Tiboulen, 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 $s_\alpha / g$. 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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*The (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.

*Maire 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. Atabyron on Rhodes Island. (Mountain astronomical observatories are a modern phenomenon, due to influence of atmospheric unsteadiness in a telescopic era.)
Aubrey Diller, Hipparchos, & Sph Trig’s History

Does Spherical Trigonometry Go Back to the 2nd Century BC? Unexpected Perfect-Fit 2009 Induction Sniffs 75° Controversy Refless Alex Jones

A2 In 1979, DR independently discovered Diller’s solution and (after learning of his prior publication) contacted him (1979/11/26) — while continuing to improve it. Besides adding (to the data-set) Hipparchos-Strabo klimata unknown to Diller 1934 (but perfectly fitting it anyway, a striking fruitfulness-display: e.g., fn 55), DR showed that if Hipparchos’ sph-trig-calculated klimata had been accurately 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 $\epsilon_{12}$ was measured in standard fashion (eq.8, below) and account for refraction&parallax, an ideal Hipparchos determination of $\epsilon$ would have been $23^\circ 3^\prime$ 42", and standard ancient rounding was to the nearest 5', so $\epsilon_{12} = 23^\circ 40'$ was correct to its precision. Even ignoring rounding and r&f, 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 (frie 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 subsumed such scholarly triumphs for 81) it 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 $1^\circ = 700$ stades (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 $\epsilon$ value $\epsilon_{H} = 23^\circ 2/3 = 23^\circ 40'$.

B Correcting Meroë’s Mislevitating 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 13° klima. Meroë was long 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 neat 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 beckoning 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: [B5].) 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” 14° klima by 400 stades — this, though it was common in antiquity to casually call such areas “klimata”.

This, because few if any important cities were likely (being tiny areas) to obligre by falling smack upon a klima. (This obvious point had become obscured by the time of

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1. See, e.g., Rawlins 1991W §D2 and DIO 4.1 §A5 [p.35]. Also the ancient galactic-circle CygSegment on the Farnese globe: www.dioi.org/iffl.htm#gtgm.
2. See DIO 4.1 §3 fn 2 [p.33].
3. This realization is not the 1st restoration correction of the mis-filing of a famous item. See, e.g., DIO 8 15 fn 5.
4. 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 deuded 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] Overruling Strabo 2.3.38 (and his own Neugebauer 1975 p.366 n.39; see below at §F4) so as to equate Alexandria city and klima at 21800 stades. [ii] Ignoring all klimata south thereof (this, even while knowing that his scheme didn’t fit them but the exiled Diller’s did). [iii] Skipping the 15°1/2 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) $L = 50[3L^3 - 62L^2 + 1307L - 8454]$. (If one tries a polynomial of high enough order, one can mimic any curve of the sort examined here. See also www.dioi.org/biv.htm#lqn.)
5. Examples from the names of the Almajest 2.6&8 klimata: “Awalite Gulf,” “Lower Egypt,” “Rhodos,” “Mid-Pontos,” “Southern Britannia,” “Mouths of the Tanais [Don]”. And, as we now realize, the island “Meroë”. (Note: the Don klima was generally 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 §C trio was undone by confusion as to which southern site was the basis of the differential datum.)
6. “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 13\textsuperscript{th} klima was for Meroë Island. (This is made explicit at Pny 6.220 & Almajest 2.6.) Moreover, we notice that the latitude differences in stades given by Strabo connected to Meroë 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.\textsuperscript{10} When Strabo finally speaks of the supposed Meroë klima, he does not speak of a spot called Meroë (elsewhere) but says (Strabo 2.5.36): “In the regions of Meroë 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 Meroë as about 3000 stades, which (even if [as he wonders] exaggerated) rather more than covers the 200-stade discrepancy by the value predicted by Diller-DR’s theory for the Meroë klima (11600 stades) and the city’s measured latitude (11800 stades) which has hitherto been mis-filed amongst the Hipparchos-Strabo klima table. Conclusion: Meroë 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 Meroë city & klima but supposed (like DR for decades) that Strabo had neglected to supply the klima’s L. Which brings us to rephrasing the shocker 1\textsuperscript{st} revealed in DIO 5 (2009). By contrast to all his inter-city placements of Meroë city (fn 10): during his lone reference to the Meroë 13\textsuperscript{th} klima, Strabo 2.5.36 hands us its latitude by stating that it is 1800 stades nearer Alexandria than to the Equator. As DR 1\textsuperscript{st} realized 2009/4/1 (merely 5\textsuperscript{th} before DIO 5’s online publication! — this, after 25\textsuperscript{st} of delay in publishing Diller’s GD 8 ms in that volume, as long planned): since the context\textsuperscript{11} is klimata (not cities) and since the 14\textsuperscript{th} klima is at 21400 stades (Table 1

| Table 1: Hipparchan Klimata Fits: Princetitute vs Diller-DR |
|---|---|---|---|---|
| Equator | 12\textsuperscript{h} | 0 | 1500 | 0 |
| Cinnamon | 12\textsuperscript{h}3/4 | 8800 | 10200 | 8800 |
| Meroë | 13\textsuperscript{h} | 11600 | 12800 | 11600 |
| Syene | 13\textsuperscript{h}1/2 | 16800 | 17600 | 16800 |
| Lower Egypt | 14\textsuperscript{h} | 21400 | 21800 | 21400 |
| Phoenixia | 14\textsuperscript{h}1/4 | 23400 | 23700 | 23400 |
| Rhodes | 14\textsuperscript{h}1/2 | 25400 | 25500 | 25400 |
| Hellespont | 15\textsuperscript{h} | 28800 | 28800 | 28800 |
| Massalia | 15\textsuperscript{h}1/4 | 30300 | 30300 | 30300 |
| Pontos | 15\textsuperscript{h}1/2 | 31700 | 31600 | 31700 |
| Borysthenes | 16\textsuperscript{h} | 34100 | 34100 | 34100 |
| Tanais | 17\textsuperscript{h} | 38000 | 38000 | 38000 |
| S.Little Britain | 18\textsuperscript{h} | 40800 | 40800 | 40800 |
| N.Little Britain | 19\textsuperscript{h} | 42800 | 42800 | 42800 |

Table 2: Sph Trig: Hipparchan Longest-Days in Hours ⇒ Latitudes in Stades

<table>
<thead>
<tr>
<th>Klima</th>
<th>Longest Day M</th>
<th>L Computed via Sph Trig Eq.4</th>
<th>Rounded to Nearest Degree</th>
<th>Converted to Stades via Eq.1</th>
<th>Rounded to Nearest 100 Stades</th>
</tr>
</thead>
<tbody>
<tr>
<td>Equator</td>
<td>12\textsuperscript{h}</td>
<td>0°</td>
<td>0°</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Cinnamon</td>
<td>12\textsuperscript{h}3/4</td>
<td>32°36’23”</td>
<td>32°7/12</td>
<td>8808</td>
<td>8800</td>
</tr>
<tr>
<td>Meroë</td>
<td>13\textsuperscript{h}</td>
<td>16°35’04”</td>
<td>16°7/12</td>
<td>11608</td>
<td>11600</td>
</tr>
<tr>
<td>Syene</td>
<td>13\textsuperscript{h}1/2</td>
<td>23°59’43”</td>
<td>24°</td>
<td>16800</td>
<td>16800</td>
</tr>
<tr>
<td>Lower Egypt</td>
<td>14\textsuperscript{h}</td>
<td>30°33’49”</td>
<td>30°7/12</td>
<td>21408</td>
<td>21400</td>
</tr>
<tr>
<td>Phoenixia</td>
<td>14\textsuperscript{h}1/4</td>
<td>33°31’04”</td>
<td>33°1/2</td>
<td>23450</td>
<td>23400</td>
</tr>
<tr>
<td>Rhodes</td>
<td>14\textsuperscript{h}1/2</td>
<td>36°15’25”</td>
<td>36°1/4</td>
<td>25375</td>
<td>25400</td>
</tr>
<tr>
<td>Hellespont</td>
<td>15\textsuperscript{h}</td>
<td>41°07’34”</td>
<td>41°1/6</td>
<td>28817</td>
<td>28800</td>
</tr>
<tr>
<td>Massalia</td>
<td>15\textsuperscript{h}1/4</td>
<td>43°16’44”</td>
<td>43°1/4</td>
<td>30275</td>
<td>30300</td>
</tr>
<tr>
<td>Pontos</td>
<td>15\textsuperscript{h}1/2</td>
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<td>45°1/4</td>
<td>31675</td>
<td>31700</td>
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<td>Borysthenes</td>
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<td>48°45’50”</td>
<td>48°3/8</td>
<td>34125</td>
<td>34100</td>
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<td>54°1/4</td>
<td>37975</td>
<td>38000</td>
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<td>58°1/4</td>
<td>40775</td>
<td>40800</td>
</tr>
<tr>
<td>N.Little Britain</td>
<td>19\textsuperscript{h}</td>
<td>61°04’56”</td>
<td>61°1/12</td>
<td>42758</td>
<td>42800</td>
</tr>
</tbody>
</table>

...AND \textsuperscript{12} Strabo 2.5.38, we use this Alexandria klima latitude to solve for the Meroë klima latitude \(K\) by simple arithmetic in stades:

\[K = (21400 - K) = 1800 \quad \Rightarrow \quad K = (21400 + 1800)/2 = 11600\]

— 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\textsuperscript{13} enjoyed such perfect verification?

B6 Muffia 2002-2009 reaction? Strabo’s klimata data suddenly aren’t trustworthy anymore?\textsuperscript{14} As posted by DR (www.dioi.org/cot.htm#flmne) a few days earlier in anticipation of delay in publishing Diller’s GD 8 ms in that volume, as long planned): to sum up: the very same data that were for decades unreliably sacrosanct to Mufosi (being the basis of the Mufia-Princetitute DSB-placed whacko Neugebauer scheme: see Table 1 \\textsuperscript{1} & especially §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 (fn 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.dioi.org/gad.htm#fpw 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.
of the all-too-predictable: “DR to Muffia: Is 13-out-of-13 Enuffa?” See DIO 5 fn 22 and in-love-Osgood Gingerich at DIO 11.3.36 §A1 on the Muffia’s decades-long tolerance of all manner of imperfection in Ptolemy&Neugebauer, even while (the source being of non-Muffia breeding) blind to a now-literally perfect fit. (Thereby in-fasteriously 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#slst), while transforming journals & conferences into elab-orate 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.

C Phi-lo's Geographical Symmetry Verified

C1 Strabo 2.5.7 (emph added) describes Eratosthenes’ geography of the Nile: “from Meroé to Alexandria . . . is about 10000 stades; and Syene must lie in the center of that distance; so that the distance from Syene to Meroé is 5000 stades.” This statement has not generally been taken seriously, perhaps because of its numerological look, plus the myth of the Greeks as non-empirical. (See Rawlins 2008Q §K4.) Yet it is in fact precisely accurate.

The actual latitudes: Meroé 16°57', Syene 24°05', Alexandria 31°12'; so the gaps are each nearly 7°18'; 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-Meroé turns out to be impressively true: ±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 was most likely careful luminance measurement. Note that the city latitudes given below [§B3] are largely accurate15 to ordng M 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°4/04' (Rawlins 1982Q ec.10, Rawlins 1994L fn 44). His place for Alexandria was, like Meroé’s, adopted (Strabo 2.5.7) by Hipparchos (who never visited Africa) and typically rounded to 31°1/12th & 16°11/12. Strabo 2.1.20 relates that an observer named Philo had taken astronomical measures by gnomon at Meroé, and his statement (idem) that the Sun is at zenith 45° before S.Solstice 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 ε half, the reverse is true in the tropics. A transit instrument would likely allow a more accurate measure of L, and this should be done in the future for Meroé.

15Even the rough latitude for Athens (Strabo 2.1.2), 38°+,-, 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/fff.htm#rvbv.) This relates to DR’s contention (www.dioi.org/cot.htm#bp) that most astronomers (as against astrologers) knew Athens’ actual latitude, and that this may relate to the origin of the Farnese globe: of indicated home latitude 38° (presumably either Pergamon or Athens).

16Rawlins 1994L fn 44. Strabo 2.5.39 confirms this by putting Hipparchos’ Alexandria 3640 stades south of the Rhodos 14°1/2 klima, thus at latitude 21760 stades or 31°1/12. Further if less precise confirmation: Strabo 2.3.38 says the transit of Arcturus is a little south of the zenith, consistent with the star’s quite erroneous 31° Hipparchan declination (Almajest 7.3). We have elsewhere proposed that since culminating Arcturus was actually c.0°15 south of the zenith 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.

17The 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 191° 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°.

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:

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

(Almajest 2.3) where obliquity \(\epsilon\) was usually taken to be that of Eratosthenes-Ptolemy (eq.5) or nearby 23°5/6, or of Hipparchos’ two values (eqs&8,2), the latter (23°2/3) being the exclusive and totally unexpected discovery of Diller 1934. (All three of these obliques are discussed in, e.g., Rawlins 1982G, 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)

\[
\epsilon_E = 23°5/6' = 180° \cdot 11/83
\]

or the early Hipparchian obliquity

\[
\epsilon_{HI} = 23°5/6'
\]

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, & Meroé — 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°1/6 vs S-M = 7°5/12. Ptolemy’s klima—city Meroé 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 Meroé, 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 3) then knew that Alexander’s L was nearly 31°1/5 of the Zenith-S-M symmetry was not only true but competently 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’ Meroé-Alexandria distance 14°1/6, so (eq.1) not 10000 but 9900 stades. But Strabo 12.5.7 & 17.3.1 says “about” 10000 stades. This favored 16°11/12 (& thus Philo’s accuracy) as Eratosthenes’ & Hipparchos’ Meroé, L rather than 16°5/6. Either satisfies 11800 stades.
D4 Since §D3’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 §E1) 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 §D3 [a] seem to be Ptolemy’s, not Eratosthenes’.)

D5 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-Marinus-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.)

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

E1 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#mmsz.)

E2 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-fleeing dominatrices), the reaction to such a massive demonstration of said gang’s fallibility is predictably Doc-Cookian: 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.

E3 Continuing obtuseness (§E1 item [c]), defining Diller 1934’s multiple [now SEVEN-FOLD] predictive vindications (DIO 5 §D3), 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 deceit by this odd community, which only adds to the parallel disgrace of the Pharos ame became invisible: Rawlins 2008Q) would render clean settings of the Sun’s disk 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: §J6)! 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 (§J: Hipparchos?) tracks klimata for the small range of Mediterranean L which it was invented to fit via Diller’s Hipparchan \( \phi \): fn 50. Jones’ “unshifting” all Strabo \( \lambda \) by 100 stades would be hard to show in our graph since the amount is so tiny (1°77) 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 (§I3) any chance of fitting eq.4 to the Strabo data, regardless of \( \epsilon \)-choice. So the graph’s larger message ironically redounds against Jones’ §I1 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 \( \epsilon \) 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.

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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 inedubably Princtitsooted 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: §J6)! 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 (§J: Hipparchos?) tracks klimata for the small range of Mediterranean \( L \) which it was invented to fit via Diller’s Hipparchan \( \phi \): fn 50. Jones’ “unshifting” all Strabo \( \lambda \) by 100 stades would be hard to show in our graph since the amount is so tiny (1°77) 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 (§I3) any chance of fitting eq.4 to the Strabo data, regardless of \( \epsilon \)-choice. So the graph’s larger message ironically redounds against Jones’ §I1 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 \( \epsilon \) 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.
E4 Is it really this easy for a few rebel scholars to cripple (into freewil-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? May bigots’ turf-possessiveness be succinctly summed up as: Pigotry?

E5 Readers can examine this matter (bluntly condensed on www.dioi.org/cot.htm#dps) carefully for themselves and then opt for which view to go with, on Diller’s grand discovery: Occam’s Razor? Or Mufa theology? I have friends who claim (2009) the Neugebauer Muffa’s bad side is dead. Reply: not until the truth of the Hipparchos-Strabo Hipparchos-Strabo story is faced. Rigid, corrosive Mufosis is a recent phenomenon of a century is part of what academe’s ever-tolerated-as-normal archonal-vanity shunning-rages inevitably lead to. And, as of 2009, Muffa-triggered shunning is as undesand 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 dominatrics.

E6 Sadly, an apostolic succession of modern cultists has outrageously deed common sense — and (needlessly) risked degradation 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 amoral unrepentant cultists’ never-confess determination to throw worse reputation after bad), even though their methods for doing so have run the gamut from [1] Babylonianly claiming a more historical theory than Diller’ (Neugebauer 1975 p.305 &. 3.04.6) p.305 ‘(and [2] to a) contentless argument that Neugebauer’s authority meant more than Diller’s (top Mufioso to DR by phone c.2000, to [3] randomlywalkingly or vaudevillessly (!K2) reversing field by jetsonising27 Neugebauer’s theory (after our Table 1 [originally 1994, augm. 2002] has made it a faked joke among neutrals) and now instead shifting to trash the very same universally-understood coherent data-base (middle column of above Table 1: Neugebauer 1975 p.131) upon which Neugebauer’s authority on the issue had been founded and accepted for decades. This final stoat-squirm (above, p.2) occurred in a curiously semi-Mufioso-theology apologia-paper Jones 2002E (frequently called “MuJHA” here to accent its cult-think roots). The new 2002 tactic: outside the beautifully consistent Table 1 (whose climata 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-crimebought exercise appeared instantly after the Isis paper [citing DIO 4.2 p.56 Table 1] was circulating among cultists for refereeing? See www.dioi.org/biv.htm#bpxp.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 sum-shun-factor: www.dioi.org/fff.htm#msnc) which helped earn super-cowerer J.Evans his advancement into Assoc. Editorship 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/nott.htm#gdgb: “There is no agnostic so ready to embrace doubt as a believer when faced with… evidence… inconveniently-inconsistent with his undislodgeably-sacrred tenets.”26 At www.dioi.org/vols/wed.pdf p.31. Two pregnant questions are naturally suggested by the outre spree. (2) Is this trying to suppress public gratitude for Diller’s greatest 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/sno.htm#hivt.) [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’ censorial behavior.27 If this indicates that denigrating a DR-associated achievement has higher Muffa priority than worshiping Mufioso-godpop 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 Mufioso’s very littleness?
MuJHA’s non-refereed history is revealed by two slips. At literally [5F2] chapter-one Almajest sophistication. Ancient astronomers’ “equinoctial” ratio \( s_g/e \) (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):

\[
\text{latitude } L = (Z_w + Z_s)/2 \quad \text{obliquity } \epsilon = (Z_w - Z_s)/2
\]

(8)

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

\[
s_g/e = \tan L = \tan[(Z_w + Z_s)/2].
\]

(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 “equinoctial observations” of \( s_g/e \) (e.g., Vitruvius, Pliny, Ptolemy: see Rawlins 1985p pp.262-264 & Almajest 2.6) are neither equinoctial nor raw observations — but are instead calculations, performed in several steps (eq.8-9) from raw solstitial observations. In short, finding equinoctial \( s_g/e \) involves a multi-step (eqs.8&9) calculational processing of two observations, while strictly \( s_g/e \) 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.)

MuJHA’s prong [b] 2nd try at gutting Diller 1934 is an argument for E’s obliquity having been used by Hipparchos: contra [3F3], Jones claims that Pytheas’ famous S.Solst. solar altitude (c.300 BC) shadow/gnomon 3 ratio \( s_g/e \) at Massalia is (modern Marseilles)

\[
s_g/e = \frac{414}{120} = \tan 19°12' = \tan Z_s
\]

(eq.12)

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 \( L = 31° \), is as unsupported as is the same page’s connexion of our eq.11 to him. By contrast, Rawlins 2002p264 used Eratosthenian data with expected solar-semi-diameter errors in a coherent argument to show that his Alexandria \( L = 31°04' = 21750 \) stades, which accounts for the unusually precise stades, 10s-place ending of Strabo 2.5.24, as well as Hipparchos’ Alexandria \( L = 31°05' = 21760 \) stades (explaining the other-places’ Alexandria’s 10s-place ending of Strabo 2.5.39, 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° to 5° at [H3]). These 2 Strabo passages are thus consistent with central site Rhodos’ \( L \) being 25500 stades (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.dioi.org/ct.htm#ncmf. 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 sladash (see www.dioi.org/qqq.htm#hhref!) 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 offence which alone destroys a journal’s claim to having a reliable/honest refereeing process] even when He knows [DIO 6 3 §11] they are competent), one cannot be surprised when misunderstandings pass into print unheeded. 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.dioi.org/deb.htm. This, again, is the kind of improvidently-initiated destruction of communication inevitably created by fawning on semi-numerate archons who abuse for their own inductive sterility by such creativity as shumming.

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 c.300 BC, allowing instant conversion of \( s_g/g \) 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” (“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 Byzantium on the same 15°14’ klima. While Massalia is close to the implied 43° parallel, Byzantium 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 adjectively claims that 2 other gnomon ratios, both equinoctial (Alexandria 3.5, Carthage 7.11), are empirical because of roundness, i.e., because the \( s_g \) 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 1985 pp.262-264 & n.17.)

Alexandria \( L = \arctan[s_g/g] = \arctan[3/5] = 36°58' \approx 21700 \) stades

(11)

Carthage \( L = \arctan[s_g/g] = \arctan[7/11] = 32°28' \approx 22700 \) stades

(12)

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 eqs.11&12 is 1000 stades, contradicting the difference at Strabo 2.5.38 (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°1/2 is waaayyy (over 4°!) too far south of actual Carthage, fatefully 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 \( g/s_g \) ratio but is the longest/shortest-day ratio \( M/m \) for the Alexandria klima where \( M = 14° \) — just as the 7.11 ratio for Carthage is not the \( s_g/g \) ratio but the \( m/M \) ratio for the \( M \approx 14^\circ/23 \) klima around actual Carthage (\( L = 36^\circ51'N \), not 32°1/2 which is the arctan of \( s_g/g = 7/11 \): eq.12), as 1° already revealed by DR.
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 (q2.2 eq.4) just "ordmag 1", 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 Massalia native, obviously enhancing odds that eq.10 is a real 1ª-hand observation, and that this observation was oft repeated to get it just right. So there is no reason to follow MuJHA’s p.17 rejection of Diller’s unquestionably-calculated17 fourteen perfect fits to eq.4, just on the basis of MuJHA’s infirm speculation58 that a reality-accordant reading (s4 in eq.10) was actually non-real and thus also calculated. The MuJHA roundness arguments are curiously perverse. Highly rounded s5/g ratios (Alexandria & Carthage) are obviously not directly empirical (and in these cases aren’t even s5/g 1:4)!, because in the real world, an outdoor eq.9 measurement of s5/g will probably be as unround as eq.10 (Massalia). Summing up: MuJHA is simultaneously taking equinoctial s5/g as directly empirical and solstitial s5/g as non-empirical when (F3 & eq.9) the reverse is true. So by 2 independent criteria, MuJHA’s 3 assessments of the Strabo s5/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 $L = \arctan \left( \frac{\cos(\frac{7\pi}{12} - \frac{15\pi}{18})}{23^\circ51'20''} \right) = 43^\circ01'24''N$ (13)

Then, we subtract that same obliquity, and arrive at a S. Solst Z$z$, which is supposed to explain the “calculated” Pytheas s5/g of eq.10 but doesn’t:

$$s5/g = \arctan(43^\circ01'24'' - 23^\circ51'20'') \neq 41.713/120 \neq (41.4/5)/120$$ (14)

a failure which leads (§51) to Jones 2002E’s p.17 plea — not necessary for ANY of Diller-DR’s FOURTEEN hits (Table 2) — that we tolerate Slight-Miscalculation (§51) in the CENTRAL attempted hit of Jones 2002E’s concoction. Just one more unexplained inconsistency.

36 The experiment requires the gnomon’s verticality checked by plumb-bob, an art preceding Pytheas by 1000s. At least some. 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 Callippos (~329/628 1/4) & Hipparchos (~134/626 1/4) were both accurate within 1/4º rounding precision (see Archimedes at Almajest 3.1), but such success requires 1º accuracy since it is done by equal altitudes. (Raw human ocular accuracy is to c.1/3: Rawlins 1985G. As ancient scientists were aware: DIO 14 [2] all I.e., agreeing with math not reality, obviously the normal situation for clima: In 34.
37 Based without justification upon claimed (§4) significance of the eq.10 ratio’s precision. But how would Pytheas calculate Z$z$ (thereby giving him his s5/g via tangent) by subtracting obliquity $L$ from (MuJHA p.17 middle equation), when he didn’t know $g$ without using eq.8, which requires one already to have determined Z$z$, the equivalent (via arctan) of the very item supposedly being sought ($s5/g$)!

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 & 2.5.4) ascribes 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 $L = 43^\circ01'$ — though we multiply demonstrate elsewhere (§6H here) that the $L$ upon which MuJHA bases his argument is actually $L = 43^\circ04'$, 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.5) 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. MuJHA p.17 realizes this; see Jones (MuJHA p.17) to develop Pytheas’ calc. on this basis.

H New Implications of Marseilles Latitude 43°04’

H1 We now produce new, independent, & fruitful evidence for eq.10’s 41 4/5 being actually 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 emended18 & 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 relation19 whatever to empirical eq.10.

H2 We next reveal that Massalia’s Almajest 2.6 latitude $L = 43°04'$ was in truth elevated by an? with Pytheas’ (Eratosthenes?) 41 4/5 Massalia’s 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:

$$\arctan(41.4/5)/120 + 23^\circ51'20'' \approx 43^\circ04'$ (15)

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°01’ (with 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’.

38 Though MuJHA’s author is (in non-math respects) superior to B. Schaefer as a scholar of ancient astronomy, the attraction to an ultra-shaky basis for an attack on a Mufia-upsetting DR-related achievement is similar to Schaefer’s blindered attraction (Schaefer 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. (Schaefer 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-Meroë arrow) based upon an explicit statement in Strabo. (Ignore the Eratosthenes arrows on the right, and keep in mind that said 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-landscapes 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 fails at fn 56.) That is, both $L = 43°04'$ and the eq.5 obliquity must be so rounded: to 43° & 235/6, resp, before the Almajest 2.6 shadow data can be recovered. The original 43°04’ is properly maintained in the Almajest editions of Heiberg, Manniche, & Taliaferro.

42 Byene [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 older ms’ ratio, not the Vitruvius-Ptolemy 3.5 ratio later substituted. See LCL’s Strabo 1:510, & Rawlins 1985G p.263&266 on GD Pharos’ L vs Alexandria’s.) He (ident) uses round $x = 24^\circ$ (not 45) to develop Pytheas’ L, thereby missing our eq.15 & getting accurate $L = 43°12'$ only by chance cancellation of 16 errors (€ & ssd). (Note: Almajest 2.6’s three s/g are consistent with $L = 43°04'$.)
I Inconsistencies’ Inconsistencies & Hipparchos’ Circuli

11 Thus, MuJHA p.17’s attempt to connect Hipparcos to 23°51’20” fails both because eq.15 could 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:

$$\epsilon = \arctan \left( \frac{-\cos((15^\circ)/4)/(15^\circ/15/2)}{\tan 43^\circ 4^\prime 0^\prime} \right) = 23^\circ 49^\prime 25^\prime$$

(16)

which is not Eratosthenes’ obliquity. (Such inconsistencies inevitably result from bringing in scraps of disparate data from all over the place to try splitter-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 Hipparcos-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°45’, 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], 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, could 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.45 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-hilarityiously 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.46

44 The Strabo Hipparcos 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 the point of gross errors) of the intervals of all the stades of almost all the observatories (except for Cyrene: 1797).

45 Translation: if a long-archon-loathed theory has the surprise 1994-2000 fronteirry to ultimately fit a set of decades-long-established data, we “have to” (§G3) 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 in 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 (fin 47k55) may be Hipparcos: [a] the $\epsilon$ is his eq.4); [b] one of the scheme’s two bases is Rhodes (fin 50); [c] the Rhodes entry is not only miswritten (restoration: www.dioi.org/catalogue/ypsv), but its restored fraction, 77/105, should’ve been rendered as 11/15, 

$L = \frac{11}{15}$ & $46^\circ 7$ 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 no table. Jones 2002E p.16’s 31° Alexandria latitude, derived from Strabo 2.5.38’s $\phi_E$ = 3° 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.1313). Jones claims another hit for his 100 stade-shift. Fetch: Ratio 3:5 is just a modern alteration of the actual text’s 7.5, which isn’t a shadow/gnomon ratio but a longest-shortest day ratio (§F4 or Neugebauer 1975 p.336 n.29). Informed of this, Jones now (2009 April) brushes off the whole issue as minor.

12 Jones 2002E p.17: this 100 stade shift would “affect the value of the obliquity best fitting the data”. (Yes, and it would produce $JHA$’s $\epsilon$ = 23°51’20”.) (And both these values are contradicted by Neugebauer’s 100 stade-shift argument of §3, which explicitly: [§3] finds $\epsilon$ = 23°47’.) [ii] As with all of MuJHA’s justifications for his 100 stade-shift), none of the Strabo data MuJHA cites against Diller-DR are given in stades by Strabo, whereas all the values accepted & used by Neugebauer 1975 pp.305 & 1313 and fitted by Diller 1934 (& our Table 1 or DIO 4.2 p.56) are explicitly given in stades by Strabo, an obvious indication that Table 1 is based on a coherent, one-source data-set.

This suggests bungling by two closely successive and-or insufficiently collaborative hands, early in the scheme’s history, similar to the Hipparcos-school slip found at Rawlins 1991W eqs.23&24.

47 Jones’ 100-stade-shift proposal suspiciously — and invalidly — mimicks (uncalled) 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 (fin 55), DR’s circuli-restitution 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 (fin 50) via eq.4 for any Hipparcos $\epsilon$. As shown at Rawlins 1985G p.263, an ancient dabber had noted that the original scheme didn’t give $M = 12^\circ$ 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 Hipparcos obliquity (eq.2): see Fig.1.
fourteenth 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 untampered original data-set of Table 1, upon which Diller-DR’s solution is founded. Conversely, if we hold at $23^\circ 51'20''$ & look for the best-fitting $\alpha$, it’s 158 stades (not Jones’ 100): possibly far from the unrestrained best fit we are about to locate (eq.18) in $\alpha$-space. But $A = 158$ stades would anciently round to $A = 200$ stades, which fails for 5 out of 13 matches. Probability $P$ (eq.19): $10^{-4}$ for 158 stades; ordmag $10^2$ 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 ($\beta_3$): $\epsilon = 23^\circ 51'20''$ (prong [a]) & A = 100 stades (prong [b]). For this remote position in $\alpha$-space, $P < 10^{-19}$ (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 mis-shift could be detected by least-squares analysis ($\beta_3$) and corrected for; [2] the $L$-vs-$M$ curve would still (see Fig.1 & caption) remain a systematic retrieve to trace the sort of curve produced by sph trig with a Hipparchan & accurate obliquity. Which vindicates Diller, though this important point is (ungenerously: www.dioi.org/biv.htm#ncmf) left unstated by JHA; so how is MuJHA a reification of Diller’s essential discovery of Hipparchan-era sph trig? That Diller has made this discovery is known to JHA, Jones, & the MuJHia. But all have chosen to leave it publicly unexpressed in explicit terms. (See comment [2] at §11.) 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 ($\beta_3$) 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 & Neugebauer’s folly because it (unlike sph trig) “failed to show how Hipparchus could have found the sequence so accurately the theoretically correct latitudes”. (Which Diller 1934 had done, heretofore to MuJHia’s arbitrary non-pleasure.) We run a least-squares fit (no roundings) of the function, arctan$[-\cos(15M/2)]/\tan \epsilon + A$, 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:

$$\epsilon = 23^\circ 37'6.6''.2'$$ and $A = -28 \pm 44$ stades

(18)

Jones 2002E n.49 haggles adduces disparate Hipparchan data (having nothing to do with JHA’s coherent data-set) to 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 the even the most helpful $\beta_3$ adjusted for it) is several standard deviations distant from the $A$ & $\epsilon$ (eq.18) which minimize the residual-sum, with probability $P \leq 1/70$. I.e., we can find A mathematically.50 (A non-fictional JHA referee would have known

50 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 cues corrupt data better than guessing-around. See, e.g., the restored Pliny cirlci (fn 50), which (before Rawlins 1985G) had been universally regarded as useless. (A view time-warply echoed at Jones 2002E n.11). Neugebauer 1975 p.748 even tersely (and correctly) acts as a clear, 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 ($\beta_5$) that back up this $23^\circ 2'3''$. (See DIO 5 $\beta_3$.)

J4 The DR solution (fn 47) of Pliny’s “circuim” 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

that and tested for A.) By contrast, Diller’s solution ($\epsilon = 23^\circ 2'3''$ & $A = 0$) 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.51 But who will be the 1st Mussi­fioso — 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 $pd$ of any point on the $\alpha$-plane, probability $P$ is the integrated volume exterior to the locus of points whose $pd$ equals that of the point in question:

$$P = e^{-\frac{\epsilon^2}{2}} = e^{-\frac{\epsilon^2}{2}} = e^{-\frac{\epsilon^2}{2}}$$

(19)

where $S$ = square-residuals sum there; $S_m$ = best-fit $S$; $\sigma$ = single-standard deviation sum; $\epsilon$ = relative difference $D = (S - S_m)/S_m = S/S_m - 1$; $N$ = n of data; $F$ = degrees of freedom ($= N - 1$ minus the number of unknowns, that being 2 in this case).

For the Princettete 4-dimensional case (fn 7): $P = (1 + F/2)^{n-1/2} - e^{-F/2}$.

J5 A general observation: the MuJHia paper (which never remotely approaching 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-thenn-known published post-1934 confirmations (now seven: DIO 5 $\beta_3$) of Diller’s $\epsilon$ & data-fit.
Since Hipparchos changed (§F1) adopted parameters (e.g., solar&lunar elements: Rawlinss 1982C & Rawlinss 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 coherent success is (informatively) 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 reach Table 1 asked for no such thing. Thus the MuJHA’s perfect H-trig-calculation fits of eqs.4&2 to Table 2? Does Occam’s Razor mean anything anymore? (Further at MuJHA.p.17: for e = 23°51’20” [sic], the resultant M = 15°1’4”/klima’s 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 Muffia 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 cite56 the ultra-tight-fit new table, or at the very least: the info that DR’s

Rawlins 1982C (MuJHA n.8) while floating the undetalled Pliny-circuit confirmation of 23°23/2 at Rawlins 1982C p.368 (ignoring its eq.2.8’s extra evidence for 23°23/2), and [ib] scoff at Pliny’s precise circuli as “crude” (MuJHA n.11, thereby sneering at Longitude Zero-referenced Rawlins 1985G), then competent JHA referencing would require citation of Rawlins 1985G pp.262-263 where the circuli are found (fn 50) consistent with a cleverly&accurately derived linear fit to a klimata table computed by sph trig via 23°23’. Cornered again (as at §1), Authoritative-in-His-Own-Mind Jones can only effect his HR fit by decreeing as the timely “Dr” Toropky’s too-pre-judgement. Jones 2002E’s implicit proposal: putatively-corrupt 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 unhappiest crankprank since his Winter Equinox (Rawlins 1991W §B4).”

J5 MuJHA p.17, 8800 stades. But neither Hipparchos nor Strabo did so. MuJHA claims that a star’s 8800 doesn’t equal the Cinnanom klima’s 8800 (Strabo 2.5.7&35). (He thinks Hipparchos believed each klima had a bright star’s 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 (§33): no ε satisfies this hypothetical new set. Unlike Diller’s 23°23’ (fn 54) Jones’ best-fit ε 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 erratic judgement (prior Jones MuJfoisse: Rawlins 1991W), has made additions to said sum (e.g., DIO 11.1.1 §D1, DIO 11.2.2 p.30, Rawlins 2008S fn 23 & p.58, 91.1 p.2); as has JHA (§1: [E1]; www.dioi.org/fff.htm#Ec1); & Evans (www.dioi.org/cot.htm#genie, gg.htm#ppp). MuJHA yields nought but chaotic (e.g.,§6 item [c]) muddying of others’ achievements. Was this its culish destructive aim? Unforeseen Jones 2002E deems worthless all DR referred finds touched-on: L = 31°0’4” (§2), circuli (fn 54), klimata (fn 44), referred by Isis (1982), Greenwich (1985), & Isis (2002), resp. DIO 1.2 §H2 [g]’s 1991 prediction of MuJHA’s relentless DR Credit-denial tactic: “Publish a wild speculation (unattested method or inferior fit) 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 schema (Table 2), though unnoticed by Diller (ascribing 8800 stades to MuJHA n.9). Yet when against the New-Diller data-set (above & §2): 8800 ≠ 8800 while following Neugebauer 1975 pp.305 & 335.n.23 in nonciting attested 8800’s exact confirmation of Diller. Though DR was 1st to publish the 8800 match to Diller, 8800’s possible relation to 123/4 was initially pondered by Neugebauer 1975 p.335.n.23.

56 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 Associate Evans 1998 cited Rawlins 1982C, but not the revealing later DIO 2002 update’s new clincher-evidence at Rawlins 1994L §C. (Our comments: †f fn 2k7, & www.dioi.org/vols/w80.pdf, adding (Table 2) standard ancient 12°/12 rounding (fn 41) upped Diller’s score; it now yields his match (§B) to all 13 klimata. Caketo-cherry: check contenders’ L for [2009 ed.]’s previously unremarked 14th klima, that at M = 12⁰, the Equator: Jones, 100 stades; Neugebauer, 1500 stades; Diller-DR, 0 stades. Jones P < 10⁻¹⁰; Diller-DR P = 0.76. [For the 2015 edition, several earlier analyses are re-edited to include the Equator klima.]

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

K1 Jones 2002E resembles a try-anything-even-if-it-contradicts-yesterday’s-dodge-tone, of an anti-Occam brand already spoofed at DIO 2.3 §X §C31. And the next Muffiosso 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 Muffia’s “Immortal Ten Thousand” degrade-brigade as granting to Xerxes’ eternal-life idea its own eternal life. (See pervasive analogy at www.dioi.org/epi.html#en.) And so we observe (yet again) a familiar cardiac-Xerxesure at the spectacle of anti-imperial rebellion and heroys being Taken-Seriously (Issis: 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 can show even 8’old kids [see DIO 4.2 pp.55-57] exactly how honest the evidence-ducking & debate-averse Muffia is.) I.e., bigfat data-trapped sacred cows just can’t make or fake it as kìggle-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 Muffia inconsistencies (§E6) vis-à-vis Strabo’s klimata, we’re not trying hard to resist recalling yet again the old vaudeville-comic rape-defense routine (already knowingly vain in 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. But I don’t even know the girl & I was nowhere near Judy that night; anyway, she consented. K4 This is what co-triggers MuJHA n.9 to urge lowering all Strabo klimata, we’re not trying hard to resist recalling yet again the old vaudeville-comic rape-defense routine (already knowingly vain in 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.

OnPD (RESPECT!) running round like Chickens with — B-But, Their Heads Are Enormously ON! Given frantic Browner-motion Muffia inconsistencies (§E6) vis-à-vis Strabo’s klimata, we’re not trying hard to resist recalling yet again the old vaudeville-comic rape-defense routine (already knowingly vain in 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. OnPD (RESPECT!) running round like Chickens with — B-But, Their Heads Are Enormously ON! Given frantic Browner-motion Muffia inconsistencies (§E6) vis-à-vis Strabo’s klimata, we’re not trying hard to resist recalling yet again the old vaudeville-comic rape-defense routine (already knowingly vain in 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. OnPD (RESPECT!) running round like Chickens with — B-But, Their Heads Are Enormously ON!
it in-full here at Table 1. (Bolstered by Table 2 & Fig.1, as well as by $L = 0$, & eq.3's fresh discovery of the Merok Klima’s actual value.) The long-overdue detailed DIO counter-attack against Mufosis’s 75's 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 rebuff Diller’s discovery of historians who have served time at the Princeton. As we asked at DIO 4.3 §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 §D3 [1]-[7] → Table 2 here] is to carry unfalsifiability to kook dimensions — and to raise the question of whether it is worth discussing historical issues at all. (Of course, one may easily understand why certain minds might wish to render reason and competence irrelevant to the evolution of ideas in the hist.astron. field.) For, if ever the most logically & evidentially one-sided controversies are to be decreed [see NCS at DIO 2.3 §18 §§C20&C25] as indefensibly irresponsible, than — why investigate anything?” When this quote was applied to a few Velikovskians at DIO 7.1 §5 fn 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 Muffia-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-gauging delusion that it ever was) — and Aubrey Diller’s ghost can rest content on his honestly & creatively earned laurels.

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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 cannon fodder-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 hitherto-unheard-of insolvency.

B2 More Outs-Truths “Free Press” —Courthbiz vs Warbiz. How many newspaper readers doubletake when, on one page, reading of the court system spending decades of lawyers’ fees on the trial&appeal&appeal&appeal of a serial rape-murderer of children, the expense justified by the US’ wuable reverence-for-the-sanctity-of-life, which abhors the horror of possibly executing even a single person unjustly? — while the opposite page has the latest body count for the latest US war: thousands of innocent people killed for the crime of being-in-the-way. How often does the US establishment’s “Mainstream” (fn 14) press point out the contrast between establishments’ ostentatious projected concern about a “culture of life” (Libs, lawyers, & centrists against snuffing criminals; and Romans & fundies against foetus-snuffing)? — versus the taken-for-granted right of the US military to kill 11 foreigners whenever their non-cooperation becomes inconvenient for cartels’ access to their nation’s resources. The spectacle constitutes a bigoted demotion of whole classes of humans, by a nation that incessantly and censoriously preaches domestic anti-racism. (For the non-rich, anyway: DIO 8 §5 §2.) DIO 4.2 §8 fn 8&23 earlier touched upon such revealingly dis-proportionate concern. (Which could readily be deemed murderous racism in many of its apparitions: e.g., DIO 4.3 §13 fnm 14&19 [1994, www.dioi.org/pro.htm/#hbsa.)

To point out the contradiction[2] between ultra-profitable courtroom over-over-overnour hyperfine-rutual allegedly to save innocent life, versus ultra-profitable cartel massacres of clusters of innocent “Foreigners”, is one of the most dangerous heresies in the eyes of the rulership, which is precisely why that rulership’s Freemike-Press has expunged it from discussion for decades. (DR directly asked a longtime editor of a major US newspaper about this, face-to-face, in 2008. No answer.) But it’s easy to show how succinctly a genuinely free press could drive home establishments’ chasmic hypocrisy here. E.g., if we look at the

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-undeniably-true headline 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 §57) & HS discouraged invading Iraq, he didn’t excommunicate Catholic soldiers. Being busy ejecting sainthood J.Gramlick for her work with homosexuals.

9 The “Mainstream” (§G7) media & the justice system can find zero time to publicly 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 (starring in its exhaustive examinations of the pros&cons of case after case) did or didn’t know-right-from-wrong. (See DIO 4.2 §19 §5, 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 out 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. See “Criminals aren’t just for breakfast anymore” at DIO 4.2 §19 §22.

10 See DIO 4.2 §19 §5: “Split-Second: Life’s Start as the Most Mortal 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 occasional courtroom justice-miscarriage is parallel to collateral damage in conventional wars. (And far more justifiable, since so numerically minuscule 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 “criminals”. See fn 9 & DIO 6 §14 §C7.

13 Mussolini’s 1935 invasion of Ethiopia devastatingly resembles the US’ present Afghanistan occupation, in the screaming contrast (rigorously press-uncited) of modern weaponry vs ragged, primitive, tank-less, airplane-less resistance fighters (always so described, when the USSR was invading the same area in 1979), who routinely are projectively accused in US media of drug-trafficking, despite [1] no sign of the big armaments such wealth buys; [2] US stooges’ involvement in the same trade. (Baltimore: #1 heroin city, an Afghan-prez brother’s home; 1/2 its murders now execution-style.) Given Afghanneighbor Pakistan’s fragile US-puppet govt’s, its burgeoning religious fanaticism, & its nuke, let’s hope Obama’s War is actually aimed at muzzling said bomb. But the propaganda used to justify invasions is oft insulently illogical, and yet their short-term lethal acts’ putative gains can escalate long-term mass-enragement (rather than lead to stable solution): classic tarbaby. And, naturally, the US media will not even mention population-control anymore (more wages of multi-culturalism’s PC), without which the invaders’ alleged aim of socially uplifting the invadees is mathematically doomed.

14 See §E13. One almost adores the press’ skill here. What could be more glaring ethical contradictions than items[a]&[b]? Yet media magicians — by dark arts we used to think were the province of boxers & divers & crooks — did

15 Lepke perhaps also encountered non-consciousent hood—boxer—diver—celeb Rocky Graziano (who went to Leavenworth for being AWOL); on whom, see DIO 14 §2 fn 73.

16 Similar sense of proportion at fn 7 or DIO 4.2 §8 fn 23.

17Mary Lincoln’s term for U.S.Grant, who never caused a single death in the War Between the States without her husband’s permission — nay, aggressive encouragement.

Roman church’s history of selective encouragement of holy wars (the several Crusades, the Armada, Mussolini’s attack on Coptic Ethiopia, Hitler’s assault on the atheist USSR [most lethal of all battles throughout human history: over 30 million dead in this theatre alone], and the US’ massive bombing of non-Christian Vietnam), it’s hard to take very seriously the Vatican’s culture-of-life propaganda against population control. Two eye-openers on the larger point at issue here: [a] From DIO 4.2 §9 §A1: “if you want to get the Church upset about the Vietnam war, then: have US airplanes drop condoms on Vietnam instead of bombs.” (Try finding anywhere, in the “Mainstream” media, such a dangerously elucidating contrast. Or our next item.) [b] The newest comment on capital-punishment-opposition vs war-promotion comes from one not known as a libertarian, but: has anyone mentioned Photoshopped Gangster Louis “Lepke” Buchalter was tried & fried in 1941&1944, resp. (He was the last wealthy citizen to be executed in the US. Though one suspects that he was bankrupt by chair-time.) He’d ordered so many murders that his outt had became notorious as “Murder Chair-time.”

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? — Any10 tears? After 6 years in the White House, his healing — “Butcher” Grant — did?
C Occasional DIO Presence?

DR thought18 O'Simpson wouldn't get off (1st time around). But otherwise DIO commentary scored some hits outside scientific history. (For indications inside: www.dio.org/vin.htm.)

C1 Our opposition to mammograms was published in 1992 (DIO 2:11 ¶18 [b]). In 2009, 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, agitation, aggravation, pain, with little if any net gain other than to medbiz 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? — who [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 wave of desperation, or a slomo foreclosure-sale (who's funding stimulus-paybacks?) akin to the under-rated, DIOliously tasteful 1979 farce-film, Americanation.

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 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 11.1-12.9) 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#bbp.)

C4 On 2009/11/30, DIO posted (www.dio.org/pro.htm#cppj) the charge that the Denbos' passionate (and quite correct) claim (some of the Left) was a fake, predicting that 2010 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 ¶82) 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.) [Added 2011: 1864 fake war-choice had Lincoln “vs” own General (!) McClellan of Peace Party Dems.]

C5 DIO 8 (¶ fn 22) [1998] suggested the identity of Deep Throat. On 2009/10/17, DIO 1st 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 2007/1/20 (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 celeb-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

18 See DIO 4.2 [9 ¶SF3 & DIO 6 14 ¶SCs.
19 It took Maureen Dowd over 2 to catch up to DIO on the “Miss Universe” recognition: International Herald Tribune 2009/10/12 p.9. What is disappointing, about the Denbo “base” that Trickledown Obama suckerized for the nomination, is how inedicable 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 dizzled by Historic Justice in electing a "black" prez (the rich’s S.Thomas-ploy, which reliably kryptonizes Denbos), it’s in-denial on the realities (È7) right before our eyes. The above 2007 posting on what and how the real killing and common sense could have seen way before 2009/1/20 that these betrayals were cynically planned not just from Day-One (but like the US' Iraq occupation) from Day-Minus-One. What does Obama have to do before the Denbo base realizes it was snookered? Denbos act inversely inedicable: 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 ambitions & 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

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 needing.)20

D2 The Nobel Prize for Physics goes to 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#wzfp and #cqcv.)


E4 White House = ultimate Oscar. (Worse: www.dioi.org/pro.htm#wzfp and #cqcv.)


E6 Middle class = only domestic fiscal blood left for rich-owned-gov’t’s fangs to suck.

E7 Our opposition to mammograms was published in 1992 (www.dioi.org/rel.htm). In 2009, 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, agitation, aggravation, pain, with little if any net gain other than to medbiz income? (See fn 23.)

E8 Our opposition to mammograms was published in 1992 (www.dioi.org/rel.htm). In 2009, 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, agitation, aggravation, pain, with little if any net gain other than to medbiz income? (See fn 23.)
F Religion & Atheism

Do popular religions fight human cloning because they resent the competition? 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 indefinitely) the most civilized region of the world. Celeb-obsession & churches are pop-cultural 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#msjs.) What other evidence is there?

Religion & Atheism

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**G Shorts**

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

**G2 Dueling academies’ fear of cult or math mist-steps has 2 phases: [a] timidity ere risking slips occasionally attending exploration; [b] post-error shame. Fear #1 can cost discoveries; #2, integrity. (See ¶1 fn 2&7; ¶3 ¶E4, fn 12&45; www.dioi.org/biv.htm#clch.)

**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-multysyllabic stroking-terms as “Italian-American”, “Serbian-American”, etc, the 2nd 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 slaves 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 19 §G.

**G6 Gov’t by 535 Messalinas.** OK, so it’s hollerin’-good-showbiz for TV’s news spatshow-host&curtus-interruptus-compulsive powerdrill Chris Matthews to sell big elections as races: Dembo-vs-Dumbo, neck&neck-down-to-the-wire (so don’t waste vote on “spoilers”) by a- nose-again&again, etc. But: why does he keep mispronouncing “whoresrace”?  
G7 Cheerleading championships are cheating their fans if they don’t even invite the best-coordinated rahrah team ever: the Mainstream Press (¶B2). Dazzling routines include: Forget-Single-Payer, Obama-Is-Change, Non-Billionaire-Owned-Candidates-Are-Spoilers, US-Invasion-Equals-Just-War, Mexican-Invasion-Equals-Just-Peachy.31

**H Life**

**H1 If aging and getting-mature were the same.** DR would live forever.

**H2 An infinite line is one whose beginning and end cannot be experienced.** Thus, we will most fully enjoy existence (and eschew fear of non-existence) if we realize (DIO 8 §5 fn 53): each human’s life fortunately has the very same property.

**H3 The Underappreciated Reality of Serial Resurrection.** Desperate religious folk so fixate on hope for a non-existent miracle of post-death resurrection that they miss savouring a wondrous genuine&reliable joy: whenever one wakes from sleep, the event becomes one more miraculous re-coming into existence, one of tens of thousands of resurrections most of us are granted, by the unfathomable accident of possessing conscious life.

**H4 Life’s five mystical highs: total solar eclipses, sex, chocolate, music, induction.**

**H5 Among the wisdoms that come with age: the realization that making others happier & brighter is not only a social good but one of the most refined of pleasurable achievements.**

**H6 No matter the lowness of attacks on one who is striving for the ideals of truth and fairness, he knows that the issue of a good, humble, and wise mind endures; and his theorist’s intelligence can firmly envision that high ultimate reality, regardless of base passing politics.** So, other than sympathy for those in the dark of ignorance, the hopelessness of mediocrity, and-or the prison of prejudice, he has no cause for any intellectual state but happiness.

30 Johnny Carson’s eulogy for Abe Lincoln: “without whom, we would not have the dunk-stuff.”
31 See DIO 6.3 ¶13 ¶F1. 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.
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B. L. van der Waerden (world-renowned University of Zürich mathematician), on DIO’s demonstration that Babylonian tablet BM 55555 (100 BC) used Greek data: “marvellous.” (Explicitly due to this theory, BM 55555 has gone on permanent British Museum display.)
Rob’t Headland (Scott Polar Research Institute, Cambridge University): Byrd’s 1926 latitude-exaggeration has long been suspected, but DIO’s 1996 find “has clinched it.”
Hugh Thurston (MA, PhD mathematics, Cambridge University; author of highly acclaimed Early Astronomy, Springer-Verlag 1994): “DIO is fascinating. With . . . mathematical competence. . . . judicious historical perspective, [&] inductive ingenuity, . . . [DIO] has solved . . . problems in early astronomy that have resisted attack for centuries . . . .”

Annals of Science (1996 July), reviewing DIO vol.3 (Tycho star catalog): “a thorough work . . . . extensive [least-squares] error analysis . . . demonstrates [Tycho star-position] accuracy . . . much better than is generally assumed . . . excellent investigation”.

British Society for the History of Mathematics (Newsletter 1993 Spring): “fearless . . . . [on] the operation of structures of [academic] power & influence . . . much recommended to [readers] bored with . . . the more prominent public journals, or open to the possibility of scholars being motivated by other considerations than the pursuit of objective truth.”