Ancient Spherical Trig:

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

Diller Verified on Klimata After 75\textsuperscript{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 Un falsifiability: Cycles of Aged Clefted But JHA-Xerxesed

DIO’s rise in prominence (e.g., NYTimes 2009/9/8 Science)1 has only fanned the cowering DIO for the History of Astronomy’s eternal loathing (www.dioi.org/qqq.htm#sdmh, or [3 fn 56 here]. When not [a] shunning DR or DIO, JHA runs articles on him which are reliably [b] destructive & [c] scientifically self-destructing, e.g., J. Evans 1987, B. Schaefer 2001. Article [3] here micro-shreds a JHA attack that achieves [a] & [b] & [c] all at once. As noted & developed in DIO 4.2 pp.55-57 (1994), leading classician A. Diller in 1934 showed in the eminent journal Klio that Strabo’s Hipparchos klimata were consistent with sph trig computations, using accurate obliquity 23°/2/3. How have history-of-astronomy’s “Muffia” & JHA’s cul-tanted Diller for one of the great contributions to the history of math? Muffia found O. Neugebauer abusively attacked Diller for 40°. ON’s competing pseudo-solution was long-winded though (Table 3) it only fits 6 of the 14 Hipparchos klimata, while the proscribed Diller-DR scheme has always fit virtually (now exactly) ALL 14 data at issue; yet M’iteekers for decades (1934-2002) kept certifying3 ON’s seemingly ageless clock. Finally, in 2002, just as Isis honored Diller-DR’s proof with publication (3 fn 23), the 68° Muffia-damn broke: A. Jones’ JHA paper dumped Neugebauer’s folly. But the same paper (oft called “MuJHA” within) Xerxesedly insta-replaces it with a new clock (3 fn 66 here), trying to weasel (DIO 11.3 36 p.70) out of crediting Diller’s ever-more-obvious success. The last miniblock to the totality of Diller’s victory collapsed in 2009 (on April 1, aptly) when the only datum hitherto seen as not fitting Diller’s theory was found to do so after all (see here at 3 eq.3) — and his hit-score became 14-of-14: on the nose in every case.

The Conservative Aspects of DIO’s Triple Eclipse-Induction

DIO’s solutions (www.dioi.org/cot.html#jqsk) of all previously unsolved ancient solar speeds use attested, normal ancient methodology & produce precisely all 6 attested 4-digit integers: 24 digits (no other theory does either: www.dioi.org/thr.htm#zxt), assuming Greek-Seleukid use of non-lost 13th century BC eclipses. Knowing no missing or alternate eclipses to complain of, Muffosis 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.

1 The NYTimes article (link to full version: www.dioi.org/cot.html#qzjx) notes that DIO opponents display a lamentably common mental impenetrability. Cooperatively proving the point: during our 19° of existence, the publishers of the JHA & DIO (top US & UK history of astronomy journals: 3 fn 56) have never communicated, despite urgings at, e.g., DIO 1.3 fn 269, DIO 4.2 §3 §E3, DIO 11.2 p.30. (Rational, pacific discourse shows who’s right & numerate, so: why would archons tolerate peace?)

2 DIO both criticizes & praises JHA-Muffia output. Latter: §1 head, §3 §E7 & fnn 55 & 56, DIO 6 §3 fn 788. Shirt-unstiffings are entirely reactive to truth-wars by usual establishment anti-rebel ploys: money, shunning, money, censorship, money, kept “experts”, money, goons, money, threats, money.

E.g., 1991 & 1994 (see DIO 4.2 p.55 & fn 2), as well as J. Britton by phone (c.2000). No Muffios has yet faced the Diller-DR theory’s subtle-as-a-ton-of-hits preferability. History of astronomy offor (by contrast to Isis publishing Thurston 2002S [now vs.: DIO 22, 2018]: won’t even cite the perfect fit Diller-DR have achieved. Britton & Jones just rank archon-authority above statistics! Muffihink lives.

We generally call this unrefereed paper “MuHAA” to stress the Muffa-JHA cult-rooted cemental-ism here, which has never been any one’s responsibility. Extra weirdness: MuHAA conceded (without citing DIO) most of the central points asserted in 1994’s DIO 4.2 (p.56 Table 1). [i] Neugebauer’s competing scheme was invalid. [i] Hipparchos used sph trig; but the JHA then pretended that Diller didn’t prove this important contribution to math history, instead acting as if the Muffa-JHA gang was doing so itself — this, after 68° of Muffia denial (e.g., Neugebauer 1975 p.32, DIO 7.1 (2) of the same proposition. (Similar side-switch=claim-jump: www.dioi.org/ffl.htm#qgwp.)

Which parallels our 2001 star-dating the Great Pyramid to c.2600 BC (Nature 412:699). This we’d known was a conventional figure; but DIO was unaware of the 1350 BC date when searching ancient lunar theory, so our 13th century BC results constitute an entirely independent TRIPLE-consistency.

1 See also DIO 3 §L8 & fn 95-97, and DIO 4.1 §5A. Funnier yet: www.dioi.org/cot.html#skcp.

Typical of the modern Pytheas salescorps, JHA Assoc.Ed Evans makes his attacks on DR only in captive arenas (safe there from reply or debate), while never citing any external source correcting his mis-sense. Similar integrity: §3 fn 56. On error-admission fear: §4 §G2. www.dioi.org/mot.htm#jfrgs.

Rawlins 1991W’s math has been verified in detail by Hugh Thurston and John Britton. We thank both for an arduous, specialized task.
an outdoor topocentric observation to find geocentric longitude. Thus the sign mixup would naturally cause an error of about $-2^\circ/3$ 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). Alice, 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 consistently ascribed to the best ancient-eyed observations.

A3 Only a scholar catering to modern Hist.astron’s cult-klan could straightforwardly propose that an altitude to the looser $2^\circ/3$ — nearly triple the longitude $1^\circ/3$ or $1^\circ/2$ wide in these lunar cases) doesn’t even come close to overlapping, the very feat Evans mislabeled he’d personally achieved in 1981 and is now too embarrassed and too steeped in Muffa academic integrity to retract.

A4 Note that a major author of Ptolemy’s fake” observations (Almajest 5.12-13) is also off by $2^\circ/3$. (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 real and theoretical disks (1/2/3 wide in these lunar cases) don’t even come close to overlapping, the very feat Evans mislabeled he’d personally achieved in 1981 and is now too embarrassed and too steeped in Muffa academic integrity to retract.

A5 NB: After the three-fold (§A6) or now four-fold (§F3) collapse of Evans’ implicit alibis (Hipparchos’ eclipse-stars & his own: §A1) for Ptolemy’s huge “observational errors, the Muffa 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 Muffa (who’s avoiding complications — don’t include philosophy of science) to ponder a simple question: if devotion to our favorite positions keeps leading us into embarrassing crackpot-muff stuffs, (e.g. §A1 & DIO 2.3 §8 §§C10-C15), does this not suggest that said positions are less than completely secure?

4 Both inquirers were told by Evans that he would look into it. But he never communicated what he found. Except by implication: the deft Evans text-surgery cited at §A1 fn 6.


6 For a full four of Ptolemy’s Almajest 3.1/7 solar equinoct/solstice “observations” of the Sun, which agrees 50 times better (Rawlins 1987 p.236) with his indoor tables than with the actual outdoor Sun. See Thurston on R.Newton at DIO 8 §1 A1.

7 Evans n.50’s misadmonishment (§A1) is repeated in his later book: Evans 1998 pp.256-259; but this (post-DIO 1.3 §8) Spica serum 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 1977/3/4, seen from Spokane. (Why must Evans go back 21st for a “recent” (“ibid p.256) eclipse [mildly reminiscent of a Ptolemy ploy: www.dioi.org/cot.html#cknh], considering that Evans 1987 went back merely 6 to find a usable eclipse? Implication: 1977 is ere 1981, and JE here has his signmanship OK at last, so: seeee, he knew how to do it all along. The catch: unlike at Evans 1987 n.50, no 1977 data are reported as outdoor-measured by Evans, though he repeatedly [Evans 1998 pp.256-257) speaks of “observations” or “observed”). So he knows he screwed up the 1981 eclipse’s parallax-sign, but CAN’T admit that (§A2) DIO corrected it for him. (Note contrast to, e.g. DIO 2.1 §4 in 18 & DIO 11.22 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 is still clinging to his woolly-sheep, & is still spending $1400 to falsely peddle to the JHA’s M.Hoskin, hist.astron’s own Lord Sommers [DIO 2.3 §1 fn 18]. Who’ll dispute the aptness?) Another corruptive consequence of a cult’s living with the shame of knowing that its sacred mission was similarly attacked (Almajest:173-183; 1992/8) by Muffa capo N.Swerdlow, a disaster undercut by several freshman-level Swedlow goofs (immediately revealed [1992/10 at DIO-JHA 2.3 §8 C]).

8 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 PN-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

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

B1 Fundamental astronomers attempting to find fundamental stars’ longitudes wrestled for centuries with an obvious inherent problem: 0° longitude is the Vernal Equinox, but that is the location of a solar event and the stars are invisible when the Sun is visible. The best-known pre-modern method was to use the Moon (or Venus) as a stepping stone: near sunset, find the arc between Sun and Moon while the former was still visible, then find the arc between star and Moon a little later (method nicely diagrammed by Evans 1987 p.235 Fig.4); finally, use mostly simple arithmetic (Rawlins 1982C App.B) to find the arc between star and Sun. But Hipparchos also applied an ingenious alternate method, which avoids such a rickety scheme: just measure how far a star is from the Moon at mid-eclipse, when the 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 $145/4/21-22$ and $134/3/20-21$ to try locating Spica. The results: $173^\circ/4/2$ & $174^\circ/3/4$, resp, a terrible disagreement — over a degree! (Remember: the lunar semi-diameter is merely 1/4 degree.) No, 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 a millennium is that it showed that one could prove anything with statistics. (Is the Muffa 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, 3292 Utah St, St. Louis, MO 63116; phone 314-772-4286. See also the excellent Isis review of Fomenko’s book. A central technical flaw undoing the entire Fomenko et al analysis is revealed in the 1995-added note in DIO 4.3 §14.) Yes, one can prove anything with statistics — if the sample is biased or the math miscomputed. But it is up to the Muffa to show what relation such a truism has to statistical findings it loathes, e.g., Rawlins 1994L. Merely doubting Evans’ citation-practice integrity, see §3 fn 24. (NB: This chauvinist lawyer is still clinging to his woolly-sheep, & is still spending $1400 to falsely peddle to the JHA’s M.Hoskin, hist.astron’s own Lord Sommers [DIO 2.3 §1 fn 18]. Who’ll dispute the aptness?) Another corruptive consequence of a cult’s living with the shame of knowing that its sacred mission was similarly attacked (Almajest:173-183; 1992/8) by Muffa capo N.Swerdlow, a disaster undercut by several freshman-level Swedlow goofs (immediately revealed [1992/10 at DIO-JHA 2.3 §8 C]).

See 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 Muffa to show what relation such a truism has to statistical findings it loathes, e.g., Rawlins 1994L. 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, Muffa 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 selectively-scattershot agonism scrape this low in the barrel. Rawlins 1982C’s simple statistical proof (Tables IV&V) that the Star Catalog was stolen from Hipparchos by Muffa-hero C.Ptolemy, was similarly attacked (JHA 23.3:173-183; 1992/8) by Muffa capo N.Swerdlow, a disaster undercut by several freshman-level Swedlow goofs (immediately revealed [1992/10 at DIO-JHA 2.3 §8 C]). Since that contrettemps, a general Muffa backing (even re-invention: www.dioi.org/det.html#mugc) of the whole field of statistics seems not only expected but downright inevitable.
For each eclipse, Hipparchos' method was:

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

\[
\Delta \lambda = \lambda_s - \lambda_M'
\]  
(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 \( \lambda_M' \):

\[
p_\lambda = \lambda_M - \lambda_M'
\]  
(2)

[c] Without applying the equation of time, find via Hipparchos' PH theory\(^9\) the Sun's true geocentric longitude \( \lambda_S \) at the time (according to Hipparchos' luni-solar theory) of mid-eclipse, which yields true geocentric longitude \( \lambda_M \) by the simple equation:

\[
\lambda_M = \lambda_S \pm 180^\circ
\]  
(3)

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

\[
\lambda_s = \lambda_S + p_\lambda + \Delta \lambda \pm 180^\circ
\]  
(4)

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 theory\(^12\) placed the Sun at about \( \lambda_S = 27^\circ2/3 \) at this time.

\(\text{B3}\) 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.)

B4 If our theory is correct, Hipparchos mistakenly subtracted \( p_\lambda \) and thus found (instead of \( \lambda_s \)) an erroneous value which we will call \( \hat{\lambda}_s \) (the “\( \hat{x} \)” subscript signifying that this longitude is infected with wrong-sign parallax):

\[
\hat{\lambda}_s = \lambda_S - p_\lambda + \Delta \lambda \pm 180^\circ
\]  
(5)

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 theory\(^12\) placed the Sun at about \( \lambda_S = 27^\circ2/3 \) at this time.

\(\text{D1}\) 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.\(^13\) Neugebauer loc cit explicitly contradicts it.) These tables were essentially the same as the Hipparchos' likely location on the island of Rhodos (Rawlins 1994L \S\S11&18). His correction was rightly positive (so he \[or a member of his school\] had by this late point in his career straightened out the signage of his procedure): 145/4/21-22.

\(\text{D2}\) Toomer 1984 p.227 n.21 correctly points out the accuracy of Hipparchos' longitudinal parallax correction for the luni-solar observation he made on \(-145/4/21\) at 6:20 Lindos Apparent Time. (See also Neugebauer 1975 pp.92 & 323.) His correction was rightly positive (so he \[or a member of his school\] had by this late point in his career straightened out the signage of his procedure): 1/28 ± 1/12. (That is, \(-1/2\) to \(-1/12\).)

\(\text{D3}\) We assume accurate observation and the ancients' common practice of rounding quantities to fractional degrees. Our fits here are almost too good (fn 22), which can be due to [a] Hipparchos having carefully averaged a careful series of mid-eclipse data for each eclipse, and-or [b] DR having acquired Ptolemy's habit of favoring (postulating likely Hipparchan) roundings that lead to exact agreement. But the putative latter factor's net effect is trivial. (Rawlins 2018U \S\S realized Hipparchos' 1st Rhodos observation was \(-146\) S. Solst, 1st of the 3 cardinal-pt data he computed the PH orbit from, after his \(-145\) Evarion, in time to predict mid-eclipse for his planned \(-145/4/21\) Spica placement.)

\(\text{D4}\) Toomer 1984 p.227 n.21 correctly points out the accuracy of Hipparchos' longitudinal parallax correction for the luni-solar observation he made on \(-126/5/2\) at 6:20 Rhodos Apparent Time. (See also Neugebauer 1975 pp.92 & 323.) His correction was rightly positive (so he [or a member of his school] had by this late point in his career straightened out the signage of his procedure): 1/8 ± 1/12. (That is, \(-1/2\) to \(-1/12\).)

\(\text{D5}\) We assume accurate observation and the ancients' common practice of rounding quantities to fractional degrees. Our fits here are almost too good (fn 22), which can be due to [a] Hipparchos having carefully averaged a careful series of mid-eclipse data for each eclipse, and-or [b] DR having acquired Ptolemy's habit of favoring (postulating likely Hipparchan) roundings that lead to exact agreement. But the putative latter factor's net effect is trivial. (Rawlins 2018U \S\S realized Hipparchos' 1st Rhodos observation was \(-146\) S. Solst, 1st of the 3 cardinal-pt data he computed the PH orbit from, after his \(-145\) Evarion, in time to predict mid-eclipse for his planned \(-145/4/21\) Spica placement.)

\(\text{D6}\) Toomer 1984 p.227 n.21 correctly points out the accuracy of Hipparchos' longitudinal parallax correction for the luni-solar observation he made on \(-126/5/2\) at 6:20 Rhodos Apparent Time. (See also Neugebauer 1975 pp.92 & 323.) His correction was rightly positive (so he [or a member of his school] had by this late point in his career straightened out the signage of his procedure): 1/8 ± 1/12. (That is, \(-1/2\) to \(-1/12\).)

\(\text{D7}\) We assume accurate observation and the ancients' common practice of rounding quantities to fractional degrees. Our fits here are almost too good (fn 22), which can be due to [a] Hipparchos having carefully averaged a careful series of mid-eclipse data for each eclipse, and-or [b] DR having acquired Ptolemy's habit of favoring (postulating likely Hipparchan) roundings that lead to exact agreement. But the putative latter factor's net effect is trivial. (Rawlins 2018U \S\S realized Hipparchos' 1st Rhodos observation was \(-146\) S. Solst, 1st of the 3 cardinal-pt data he computed the PH orbit from, after his \(-145\) Evarion, in time to predict mid-eclipse for his planned \(-145/4/21\) Spica placement.)

\(\text{D8}\) Toomer 1984 p.227 n.21 correctly points out the accuracy of Hipparchos' longitudinal parallax correction for the luni-solar observation he made on \(-126/5/2\) at 6:20 Rhodos Apparent Time. (See also Neugebauer 1975 pp.92 & 323.) His correction was rightly positive (so he [or a member of his school] had by this late point in his career straightened out the signage of his procedure): 1/8 ± 1/12. (That is, \(-1/2\) to \(-1/12\).)

\(\text{D9}\) We assume accurate observation and the ancients' common practice of rounding quantities to fractional degrees. Our fits here are almost too good (fn 22), which can be due to [a] Hipparchos having carefully averaged a careful series of mid-eclipse data for each eclipse, and-or [b] DR having acquired Ptolemy's habit of favoring (postulating likely Hipparchan) roundings that lead to exact agreement. But the putative latter factor's net effect is trivial. (Rawlins 2018U \S\S realized Hipparchos' 1st Rhodos observation was \(-146\) S. Solst, 1st of the 3 cardinal-pt data he computed the PH orbit from, after his \(-145\) Evarion, in time to predict mid-eclipse for his planned \(-145/4/21\) Spica placement.)

\(\text{D10}\) Toomer 1984 p.227 n.21 correctly points out the accuracy of Hipparchos' longitudinal parallax correction for the luni-solar observation he made on \(-126/5/2\) at 6:20 Rhodos Apparent Time. (See also Neugebauer 1975 pp.92 & 323.) His correction was rightly positive (so he [or a member of his school] had by this late point in his career straightened out the signage of his procedure): 1/8 ± 1/12. (That is, \(-1/2\) to \(-1/12\).)
Hipparchos' Eclipse-Star Data

D3 Note that Ptolemy himself indicates same through his suggestion (Almajest 3.1) that Hipparchos' Spica discrepancy may have been in his calculation of the parallax correction.

D4 The italicized $\Delta p$ point is utterly self-evident, yet it has been missed by scores of prominent, well-paid professional historians-of-astrology, each of whom has read the same passage dozens of times. And we may be sure that $\text{JHA}$ & like establishment publications will not miss a beat in continuing to sanctify these same can't-see-noise-before-face archons (who, in History circles, are the arbiters of accepted wisdom). And acceptable scholars.

D5 It has been (Rawlins 1991H fn 7 & $\S$4 [& Rawlins 2018U loc cit]) theorized that the PH solar theory & tables were based upon observations of $\ldots$ & 145 and that his subsequent $\ldots$ tables were based upon observations of $\ldots$ & 134/6 (thus could not be earlier than the latter date) — independently suggesting that Hipparchos' PH tables were used by him during the period — 145/4/21-22 to $\ldots$ (to 23/20-21).

E Sources of Error in Hipparchos' Placement of Regulus

E1 We now turn to the 3rd (and only other) eclipse known to have been observed & reported by Hipparchos — an eclipse which happens to have occurred near the $\star$ Regulus. Two initial comments: [i] Only 2 stars' explicit Hipparchos longitudes survive (Almajest 7.2): Regulus 119.6°/6 and Spica 174°, ideal fundamental stars, the nearest $1^\circ$ magnitude stars to the ecliptic. [ii] For Spica, the discrepant eclipse-based results he complains of ($\S$4&6)2 evidently (fn 22) induced him later to opt for placing this star instead by conventional astrolobe technique (which was in fact more reliable than his misaligned eclipse method); however, Regulus is the major bright star with the largest of the few negative Hipparchos $\lambda$ error for his Ancient Star Catalog's epoch ($\ldots$ 126.28: Rawlins 1991H $\S$4): $-3^\circ$. Rawlins 1991W (fn 147) remarked aloud at the enormity of this error (which led Ptolemy into a fraudulent copy of it: DIO 8 $\S$1 $\circ\otimes$), despairing as to whether its explanation would ever come to be known. (Another fruit of having at last the solution to the Regulus longitude mystery: Shevchenko 1990 had proposed that Hipparchos' Moon-star about $+1^\circ$/4, though that from Ptolemy's tables was $+19^\circ$ for his ludicrous lunar distance of 43 Earth-radii. (The Neugebauer 1975 p.92 value [16] is explicitly based upon Ptolemy's simple syzygy lunar model, not his final one.) Obviously, Hipparchos did not share Claudius Indoor Poison's notorious belief that the Moon's angular size varied by a huge factor (of up to nearly two). Indeed, the smallness of Hipparchos' $12^\circ$/2 parallax for the $\ldots$ observation indicates that his parallax calculations used a conventional lunar distance (as we already realized at $\S$1). 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): $14^\circ$; thus correcting Hipparchos' $-126/52$ observation of toponemic lunar longitude $351^\circ/23$, we have $351^\circ/26$, for which the nearest Hipparchan approximation would be $351^\circ/38$, which is just the Hipparchan geocentric longitude reported at Almajest 5.5 (Neugebauer 1975 p.92).

15 See $\|$3 Table 2 (or DIO 5 Table 0, DIO 4.2 [1994] p.56 Table 1).

16 Rawlins 1985G

17 We may get a glimpse of the inevitable escape routes (from this evidence) at A. Bowen & B. Goldstein Aner Philos Soc Proc 135:2/233 (1991) where trigrig work is (automatically) ascribed (p.235) to arithmetic methods (an approach that has caused other amusing Mufa catastrophes: e.g., fn 15 & DIO 1.2); and ancient testimony regarding predecessors' technique is doubted (B&G n.5).

18 If Hipparchos tried (assuming clear weather) confirming his Regulus longitude via the $-131/117-18$ eclipse (record not extant), the result would have been roughly $119^\circ$ 1/2, not discrepant enough (nothing like the enormous Spica $-145$ vs $-143$ clash) to cause his rejection of the $-140$ value in favor of an astrolabe-based result (as with Spica).

19 Almajest 6.5 just computes the time of mid-eclipse as 22:10 by finding when the Hipparchos-Ptolomis lunisolar tables have the true geocentric lunar longitude (125°08' by the Almajest calculation) 18° different from the true solar longitude. But mid-eclipse accurately calculated (by one of Hipparchos' computers) from these tables would be nearer 22°. The difference (about $-10^\circ$) is due to the $c.5^\circ$ tilt of the lunar motion vs the ecliptic in this partial eclipse, a factor that is even more trivial ($-1^\circ$ & $-2^\circ$, resp) for the total $-145$ and $-134$ eclipses, where Hipparchos also likely rounded the tabular-predicted Lindos Apparent Times (to 23rd/23 and 3rd, resp). If Hipparchos' presumed $-140$ use of 22° was not just a rounded value but due to accounting for tilt, then he made a tiny slip, since at the tabular mid-eclipse moment (22°) the Moon's longitude was about $5^\circ$ shorter of being opposite the Sun. (If Hipparchos actually used 221/6 Lindos App. Time: $-5^\circ$ would still be the likely recorded $\Delta \lambda$.)


21 Hipparchos' $-140$ Regulus longitude would've been listed at 119°23. Though $8^\circ$ is something less than $1/6^\circ$, the Ancient Star Catalog's longitudes are almost exclusively expressed in units of $1/6^\circ$, so though precession to the Catalog's epoch yields $119^\circ45$, this would still end up being listed as $119^\circ56$, since nothing in the Catalog is expressed in degree-fifths.

22 DIO 8 $\S$1 $\circ\otimes$11 found that astrolabe lunisolar observations showed $\pm0^\circ$.1% consistency. Ancient Star Catalog's longitudes mean error is 22° (R. Newton 1977 p.215), but stars brighter than 3rd magnitude used by pre-Ptolemaic ancient astronomers in the Almajest (dropping too-south Sco, and 2-quarter-degree ending stars: DIO 2.3 fn 18 & 20), are $\beta$ Tau, $\alpha$ Gem, $\beta$ Gem, $\gamma$ Vir, $\alpha$ Vir, $\lambda$ Lib, $\delta$ Cap, whose longitudes' deviations from a zodiacal error-wave (melded from Rawlins 1991H $\S$3F-F2) of $-9^\circ$ $-13^\circ$ (sin$\lambda$), show scatter $\pm0^\circ$.1, around an average of $+0^\circ$.1. [Note added 2017. For $-145$, $-140$, $-134$, $+1987$ eclipses, JHA's view insists on longitude errors of, resp, $-3^\circ$, $-35^\circ$, $+33^\circ$, $-40^\circ$. Removing parallax-mis-signs, & defects in Hipparchos' PH solar theory; errors in outdoor-observed star-Moon gaps were, resp, $-2^\circ$, $+4^\circ$, $+1^\circ$, $+2^\circ$. (All 6 ancient results share in common the same trivial error, due to $\Delta T$ uncertainty, of ordmag 0.1° for that era.)]
10 Hipparchos’ Eclipse-Star Data 2009 December DIO-J.HA 16 ¶1

—15’ (20’ offset from Regulus’ error). [b] Hipparchos stayed with his —140 Regulus λ fixed by eclipse, even after —134 indication (via Śpic) 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&S.)

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


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

1 Without certainty, one presumes Pytheas observed before Timocharis since the latter probably used a transit circle, an advance over the gnomon. Anyways, Timocharis’ star declinations are not raw data.

24 All 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.

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. 

2009 December DIO 16 ¶2

Pytheas’ Solstice Observation Locates Him: Cape Croisette

Pytheas’ Solstice: Oldest Vertical-Instrument Transit Observation

Why Has No Historian Taken Pytheas’ Precision Seriously?

Or Bothered Consulting a Map of Marseilles?

Summary

The first 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:

\[
\frac{s_n}{g} = \frac{41^2}{120} = \tan 19^\circ 12' 
\]  

(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 pre-telescopic measurement.

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

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

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

A Having a Fortuitous Ball

A1 We have elsewhere (e.g., ‡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 Mu/HJA 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 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-exploiter did his astronomy: Cape Croisette (Fig.2), 0°.1 south of Marseilles-habor proper (Fig.1).
A2  MuJHA p.17 having claimed that the Summer Solstice datum (eq.1 or §3 eq.10) of Pytheas was not an observation, we explore (as scientists should) an alternate possibility, namely, that Pytheas’ eq.1 was a real gnomon observation. (Which is actually, a priori, much more than a possibility.) We know that many Greeks’ gnomons were vertical & asymmetric. (See, e.g., diagrammed discussions at Manitius 1912-3 1:419-420 & R.Newton 1977 pp.38-39. Also developments in, e.g., Rawlins 1982G & Rawlins 1985G pp.260f.) This produces a shadow corresponding (eq.5) to the S.Solst zenith distance Zs 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 sss/g with such an instrument will produce a latitude L which is 16’ less’ than the true value. A useful 1st estimate of the uncertainty in Pytheas’ Zs 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:

\[ sss/g = \frac{41\frac{3}{4}}{120} \pm \tan 19^\circ 11’ \quad \& \quad sss/g = \frac{41\frac{4}{5}}{120} \pm \tan 19^\circ 13’ \]  

Thus, crudely:

\[ Z = 19^\circ 12’ \pm 1’ \]  

A3  But we can improve the precision here by examining4 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):

\[ \frac{41\frac{3}{4}}{120} + \frac{41\frac{4}{5}}{120} /2 \rightarrow \frac{41\frac{191}{120}}{120} \pm \arctan 19^\circ 12’.2 \pm 0’.5 \]  

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

\[ \text{3 Subtracting ssd = 16’ from eq.5 shows that if Pytheas knew the correct obliquity (but didn’t know of the gnomon’s ssd-error), he would have thought that his observatory was at about } L = 42° 56’. \]

\[ \text{4 We are here taking it for granted that 41 4/5 was Pytheas’ original raw datum. (And the original reading would probably have been in shadow/gnomon terms.) Yet we may test the faint possibility that whatever the original reading was, it came to later antiquity as 19° 1/5, and only subsequently (in a trig era) was its tangent calculated as a fraction of 120. (But such an assumption itself assumes ancient tangent tables [none have survived] and that these were based upon unit-120, though division of a tabular sine by its complement’s sine would cancel their 120-denominators.) However, [a] It seems rather a stretch to suppose that a later ancient would go to such trouble, to turn around the data-reduction process in order to "reconstruct" a lone pseudo-raw datum. Why would such be preserved as special? [b] A firmer objection is that, if Z were 19° 1/5, §3 eq.15 would not yield its (attested) sum.} \]
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 \( -310 \pm 25^\circ \)] obliquity \( 23^\circ 44'.0 \pm 0'.2 \), error from uncertainty of Pytheas' exact epoch):

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

We ignore rms, instead looking for the maximum additive range of errors that are not at all likely to be exceeded if the measurement was indeed carefully and repeatedly carried out. I.e., our treatment here is not based upon Gaussian statistics but 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 1\(^{st}\) 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 \( \pm 0'.5 \) (eq.4), not \( \$B2's \pm 0'.1 \) (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 §3 E] Hipparchos’ at Cape Prassonesi [the southern tip of Rhodes] 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. 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 §3 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°21’21.1” E, just beyond the end of the extended easy (non-mountain) road from Marseilles to Cape Croisette (i.e., Delabre Boulevard).

**C3** As Pytheas was a sailor, we must also consider the possibility that he (like Tycho) operated on an island. The most obvious choice would be tiny but spectacular-gradient Maire Island (whose highest peaks exceed 450 m), 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”/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 $s_\text{H}$/g. Hopefully, an archaeological miracle will someday discover the exact spot where stood the scientific home of legendary astronomer-explorer Pytheas of Marseilles.

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

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

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

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

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

B1 Since 1934 it has been known that the standout non-fit for the Diller theory is Meroë, the 13th klima. Meroë was 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: [IBS]. 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.131) provides explicitly in the case of Alexandria, noting that this city is separated from the nearby “Lower Egypt” 14th klima by 400 stades — this, though it was common in antiquity to casually call the 14th klima “Alexandria”. (Strabo 2.5.38 inadvertently does likewise: §§4.)

B3 The case of Meroë is complicated by the fact that there is both an “island” Meroë (described at Strabo 1.2.25 as the Nile’s largest: §§4) containing, in its north part, the city whose actual latitude is $L = 16^\circ 57' N$. It is the city-vs-island confusion (as also for Rhodes) we will now eliminate. The very names of klimata illustrate that most were named not for cities but for sprawling regions — such as bays, straits, river-mouths, or islands. (In ancient geography, Syene [modern Aswan] is often just a sloppily amalgam of city, trogl., & klima.) This, because few if any important cities were likely (being tiny areas) to oblige by falling smack upon a klima. (This obvious point had become obscured by the time of

See, e.g., Rawlins 1991W §D2 and DIO 4.1 §3 [p.35]. Also the ancient galactic-circle CygSegment on the Farensse globe: www.dioi.org/ff.htm#gtgm.

3 DIO 4.1 §3 fn 2 [p.33].

4 This realization is not our 1st restorative correction of the mis-filing of a famous item. See, e.g., DIO 5 §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.730k732, though at p.305, mathematician Neugebauer deluded himself into thinking his own theory better than non-mathematician Diller’s (an imagined superiority insulently expressed at p.734 n.14 — but now accepted nowhere) by: [i] Over-ruling Strabo 2.5.38 (and his own Neugebauer 1975 p.336 n.29: see below at §§4) 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 15th/16th Potons 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) $L = 50[A^3 - 62A^2 + 1307A - 8454]$. (If one tries a polynomial of high enough order, one can mimic any curve of the sort examined here. See also fn31 & DIO 4.1 §3 fn 2 [p.33].

3 This is one of the costs of having much of one’s heritage of high ancient astronomy coming to us through the filter of an only-intermittently-reliable mathematician-astrologer, C.Ptolemy.

3 The very names of klimata illustrate that most were named not for cities but for sprawling regions — such as bays, straits, river-mouths, or islands. (In ancient geography, Syene [modern Aswan] is often just a sloppily amalgam of city, trogl., & klima.)

2 Diller’s Tri-Discovery: Early SphTrig, Accurate Tilt, Klimata Key

A Diller’s Tri-Discovery: Early SphTrig, Accurate Tilt, Klimata Key

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 acknowledging (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 Table 2 here.

A3 If we assume $\epsilon_{152}$ was measured in standard fashion (eq.8, below) and account for refraction&parallax, an ideal Hipparchos determination of $\epsilon$ would have been 23°42’, and standard ancient rounding was to the nearest 5.

A4 The Hipparchos-Strabo data-base which Diller satisfied appears as the middle column of our Table 1 here, based on Hipparchos’ well-known scale $1' = 700$ stades.

(4) Using the untested but impressively accurate $\epsilon$ value

$\epsilon_H = 23^\circ 2'/3 = 23^\circ 40'$

(A1) In 1934, the eminent classical philologist Aubrey Diller provided the 1st conclusive proof that spherical trigonometry went back to the 2nd century BC, by showing that Hipparchos’ klimata latitudes $L$ (given in stades by Strabo) were beautifully satisfied — see Table 2! — by $L$ values computed from sph trig (eq.4) for each Hipparchos-Strabo longest-day $M$ using an obliquity (Earth-tilt) $\epsilon_{152}$ equal to 23°2/3 or 23°40’; eq.2 below.

1 It was correct to its precision. $0.40$ was correct to its precision.

2 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 (are 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 possesive snobs has nearly subsumed such scholarly triumphs for 81 is itself a triumph of organized truth-warping, providing a history (see p.2 & fn 7) warmly recommended to sociologists of cult-think.

3 All 13 said data were computed from klimata M values via eq.(4), below, using the untested but impressively accurate $\epsilon$ value $\epsilon_H = 23^\circ 2'/3 = 23^\circ 40'$

Strabo 2.5.7&34 or Neugebauer 1975 p.305 n.27. All 13 said data were computed from klimata M values via eq.(4), below, using the untested but impressively accurate $\epsilon$ value $\epsilon_H = 23^\circ 2'/3 = 23^\circ 40'$

1 Strabo 2.1&5, very well illustrated schematically in Neugebauer 1975’s Fig.291, p.1313 (an easy page# to remember, given Diller’s 13-for-13 success with the data). For reasons of sph trig (astrological-house-constructing) efficiency (Rawlins 2008S §A4 [2]), ancient astrologers (Hipparchos, Ptolemy) assigned the term “klima” (from which our word “climate” derives) for latitudes $L$ corresponding (via eq.4) to longest day values $M$, usually at intervals of about 1/4 or 1/2 hour (Almajest 2.6&8). A common number of primary klimata was seven; see, e.g., Pliny 6.39.211-218, Honigmann 1929, Neugebauer 1975 pp.722f.

2 This is one of the costs of having much of one’s heritage of high ancient astronomy coming to us through the filter of an only-intermittently-reliable mathematician-astrologer, C.Ptolemy.

3 AHA “refereeing” missed that [a] MuHJA n.7 cites the wrong volume of Neugebauer 1975, & [b] MuHJA n.18 consistently muff’s Syracuse’s Strabo L by 200 stades. Not the 1st time this fitfully perceptive & ever creative author has been let down by fake refereeing. See also fn 31 & DIO 1.2.
Table 1: Hipparchan Klimata Fits: Princetitute vs Diller-DR

<table>
<thead>
<tr>
<th>Klima</th>
<th>Longest ( \text{Day} )</th>
<th>Hipparchos-Strabo ( L ) [Data]</th>
<th>Princtime Mufa ( L ) [Babylonian]</th>
<th>A-Diller-DR ( L ) [Greek]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Equator</td>
<td>12(^b)</td>
<td>0</td>
<td>1500</td>
<td>0</td>
</tr>
<tr>
<td>Cinnamon</td>
<td>12(^b)/3/4</td>
<td>8800</td>
<td>10200</td>
<td>8800</td>
</tr>
<tr>
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<td>12800</td>
<td>11600</td>
</tr>
<tr>
<td>Syene</td>
<td>13(^b)/2</td>
<td>16800</td>
<td>17600</td>
<td>16800</td>
</tr>
<tr>
<td>Lower Egypt</td>
<td>14(^b)</td>
<td>21400</td>
<td>21800</td>
<td>21400</td>
</tr>
<tr>
<td>Phoenixia</td>
<td>14(^b)/4</td>
<td>23400</td>
<td>23700</td>
<td>23400</td>
</tr>
<tr>
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<td>14(^b)/2</td>
<td>25400</td>
<td>25500</td>
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</tr>
<tr>
<td>Hellespont</td>
<td>15(^b)</td>
<td>28800</td>
<td>28800</td>
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<tr>
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<td>30300</td>
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<td>31700</td>
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<tr>
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<td>16(^b)</td>
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<tr>
<td>N.Little Britain</td>
<td>19(^b)</td>
<td>42800</td>
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Ptolemy’s \( \text{GD} \) — perhaps as early as Hipparchos. See \( DIO \) fn 19 on commerciality.) So it would make sense that the 13\(^b\) klima was for Meroë \textit{Island}. (This is made explicit at Pliny 6.220 & \textit{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 \textit{cities}. Indeed, since these distances are (\( \text{섹} \)) due to Eratosthenes (who probably did not use sph trig klimata) they cannot be klimata-based and their contexts usually do not discuss hours. 10 When Strabo finally speaks of the supposed Meroë \textit{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 \( \text{Day} \) \( [M] \) has thirteen equinoctial hours”. 11

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 \( \text{klima} \) (11600 stades) and the city’s measured latitude (11800 stades) \textit{which has hitherto been mis-filed among the Hipparchos-Strabo klimata}. Conclusion: Meroë at 11800 stades latitude is a city and thus (as noted at \( \text{섹} \)) 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 reprising the shocker 1\(^e\) revealed in \( DIO \) 5 (2009). By contrast to all his intercity placements of Meroë \( \text{city} \) (fn 10): during his lone reference to the Meroë 13\(^b\) \textit{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\(^e\) realized 2009/4/1 (merely 5\(^b\) after \( DIO \) 5’s online publication! — this, after 25\(^b\) of delay in publishing Diller’s \( \text{GD} \) 8 ms that volume, as long planned): \textit{since the context is klima} (not cities) and since the 14\(^b\) klima is at 21400 stades (Table 1)

10 Examples for Meroë are: 5000 stades to the town Syene (Strabo 2.2.2. 5.7&35), c.10000 to Alexandria (Strabo 2.5.7, 17.3.1) c.15000 to Athens (Strabo 2.1.2) — just as Alexandria city is usually placed, e.g., 3750 to Rhodes City (Strabo 2.5.24).

11 Another part of the context is Strabo 2.5.38’s demonstrable confusion of klimata and cities for Alexandria and Carthage: see fn 35 or \( DIO \) 5 fn 25. I.e., in the Strabo passages examined here, his

AND 2 Strabo 2.5.38), we use this Alexandria \textit{klima} latitude to solve for the Meroë \textit{klima} \( L \) by simple arithmetic in stades:

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

— precisely the Meroë latitude predicted at Diller 1934 p.267, over 3/4 of a century of Mufa sneering after. Result: \textit{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, \textit{fourteen} successive hits out of fourteen data. Has any comparable ancient astronomy discovery ever enjoyed such perfect verification?

B6 Mufa 2002-2009 reaction? Strabo’s klimata data suddenly aren’t trustworthy anymore! As posted by DR (www.dioi.org/cot.htm#fme) a few days earlier in anticipation subject is so thoroughly klimata that even key supposed non-klimata entities turn out to be \( \text{a} \) twisted or \( \text{b} \) mis-taken versions of klimata after all. (Respective restorations: \( \text{a} \) eq.3.)

12 Another part of the context is Strabo 2.5.38’s demonstrable confusion of klimata and cities for Alexandria and Carthage: see fn 35 or \( DIO \) 5 fn 25. I.e., in the Strabo passages examined here, his
of the all-too-predictable: “DR to Muffia: Is 13-out-of-13 Enuffia?” See DIO 5 fn 22 and in-love-Osgood Gingerich at DIO 11.3.6 §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 receive-fashiously outnutting 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 elaborate balls devoted to cranks’ favorite dance — dodging dissonant evidence — needs to fill several lacks: common sense, statistical sense, Occam sense, humor sense. And a mirror.

C Philo’s Geographical Symmetry Verified

C1 Strabo 2.5.7 (emph added) describes Eratosthenes’ geography of the Nile: “from Meroë to Alexandria . . . is about 10000 stades; and Syene must lie in the center of that distance; so that the distance from Syene to Meroë is 5000 stades.” This statement has not generally been taken seriously, perhaps because of its 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: Meroë 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-Meroë turns out to be impressively true: to ±1°. (And it is less likely to be based upon accident than the equally remarkable ancient record [also correct to ±1°] that Aldebaran and Antares were 180° apart in celestial longitude: DIO 2.1.32 fn 5.) The basis of this geographical discovery is very likely careful measurement. Note that the city latitudes cited at §B3 are largely accurate to 0°01’. Rawlins 1982G shows that Eratosthenes had learned that Rhodos City’s L = 36°5/12 (good to 1°) — or 25500 stades — and that only his foolish use of gnomon for Summer (not Winter) Solstice threw off his measure of Alexandria’s L by half the solar semi-diameter, yielding 31°04’. (Rawlins 1982E eq.10, Rawlins 1994L fn 44).

His place for Alexandria was, like Meroë’s, adopted (Strabo 2.5.7) by Hipparchos (who never visited Africa) and typically rounded to 31°1/12th & 16°11’12” Strabo 2.1.20 relates that an observer named Philo had taken astronomical measures by gnomon at Meroë, and his statement (idem) that the Sun is at zenith 45° before S.Solstice is encouragingly accurate. Strabo’s report of gnomon-use at first looks discouraging due to its systematic error from solar semi-diameter ssg. However, while outside the tropics, ssg 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 have the correct L, but even if we assume 18 that Philo used a gnomon at both solstices, he would have found (accounting for both r&pc and ssg) zenith distances ZW = 40°24’ and Zs = 6°31’, yielding (by eq.8) nearly correct L = 16°56’, which Hipparchos would round to 16°11’12” from eqs.2&4, we have Syene klima at 24° or 16800 stades, thus not a bad Hipparchos-L three-seemers: Meroë 16°11’12”, Syene 24°, Alexandria 31°11’12; rms error 5’. (Notably, the GD errors for the same trio are −32°, −15°, & −12°, resp: rms error 22°.)

C3 Moreover, we find that the Hipparchos trio maintains (albeit slightly corruptly) the remarkable symmetry, presumably Philo-discovered 19 (Eratosthenes&Hipparchos-adopted), that Syene is exactly 1/2-way between Alexandria & Meroë, the Hipparchan value 50° for both intervals being 7°1/12 or (by eq.1) 5000 stades. In reality (using eq.1), both L intervals are even closer to 5000 stades (sum 9975 stades): could this accidental symmetry be one of the causes of eq.1’s establishment? (By Philo? Sostratus? Eratosthenes? Anonymous?)

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

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:

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

(Dalmaest 2.3) where obliquity \( \varepsilon \) was usually taken to be that of Eratosthenes-Ptolemy (eq.5) or nearby 23°5.12’, or one of Hipparchos’ two values (eqs 6&2), the latter (23°2/3) being the exclusive and totally unexpected discovery of Diller 1934. (All three of these obliquities are discussed in, e.g., Rawlins 1982C, Rawlins 1985G, & DIO 5.)

D2 The Rawlins 1985G tables discovered that numerous major cities’ L & M did indeed correlate with either Eratosthenes’ obliquity (Dalmaest 1.12)

\[
\varepsilon_E \equiv 23°57°12’ \equiv 180° \cdot 11/83
\]

or the early Hipparchos obliquity

\[\varepsilon_H = 23°5’56”\]

D3 The Rawlins 1985G tables showed for Ptolemy’s GD:
[a] The major cities correlated with Eratosthenes’ eq.5 or 23°5/6 included Babylon, Korinth, Kyrene, & Meroë — all related to Eratosthenes’ birth, life, 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 Meroë confusion caused a 1°2 discord between his & Hipparchos’ L, hinting that Hipparchos was not responsible for the GD’s klima-polluted L mis-geography.

19 If Philo travelled to Meroë, he must have visited Syene. So he presumably knew that its latitude was 24°05’. Yet every scientist but Eratosthenes (SC2, Rawlins 1982G, Rawlins 1994L Table 5) then knew that Alexandria’s L was nearly 31°1/5’6 of the Sun’s symmetry was not only true but completely known to be true in Alexandria’s community of genuine scientists, which again excludes Eratosthenes. His & Hipparchos’ later asymmetrical A-S-M schemes were (as just noted) slightly less accurate than the presumed original latitudes (of, e.g., Philo) but were perhaps nudged to ensure adherence to an A-S-M symmetry likely well-known long before either’s geographical scheme.

20 Doubting makes Hipparchos’ Meroë-Alexandria distance 14°1/6, so (eq.1) not 10000 but 9900 stades. But Strabo 12.5.7 & 17.3.1 says “about” 10000 stades. This favours 16°11’12” (thus Philos’ accuracy) as Eratosthenes’ & Hipparchos’ Meroë L, rather than 16°5/6. Either satisfies 11800 stades.
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’.)

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

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

While such speculations provide no proof that sph trig was known to Eratosthenes, Table 1 proves positively that sph trig (eq.4) was known to Hipparchos, as Diller 1934 was 1st to prove. (A powerful array of the evidences for sph trig’s use in Hipparchos’ century is brought together for the 1st time at www.dioi.org/cot.htm#mmsz.)

But, at a time when the hist.astron eld is run by “just a bunch of politicians” (as an aghast eminent astronomer describes the eld’s debate-eeking dominatrices), the reaction to such a massive demonstration of said gang’s fallibility is predictably Doc-Cookian: deny, deny, deny — never confess. (See Joey Bishop at Dio 11.2 [2003] pp.32-33.) It would continue 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 deceit2 by this odd community, which only adds to the parallel disgrace of not even comprehending the statistical and Occamite preferability glaring from Table 1.21

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21 If Eratosthenes and/or his critics tried both the lighthouse and sunset Earth-measure methods via the Pharos, the azimuths would be different since land beyond the point 202 stades away (where the Pharos flame became invisible: Rawlins 2008Q) would render clean settings of the Sun’s disk impossible, so viewing sunsets from the Pharos would be at most notherly azimuths. Strabo 2.2.2 is chronologically valuable in its implicit suggestion (perhaps contra the nonetheless intriguing and original analysis of Taisbak 1974) that Poseidonios was indeed the 1st prominent adopter of the much smaller circumference 180000 stades cited to him at Strabo loc cir.  

22 Hardly restricted to just Table 1. See, e.g., fn 24 and especially Rawlins 1996C §M.  

The closeness of Diller’s fit is definitely known to the Muffa-JHA community, which has been directly questioned about it by, e.g., H.Thurston and DR. The only publications accepting Diller’s success and his recovery of the long-lost Hipparchan 21°23’ obliquity have been the ArchiveHistExcScie (Nadal & Brunet 1984 p.231 & n.17); also Isis (Thurston 2002S p.67 & n.18), which is the only publication to take note of DIO 4.2’s Table 1 — but neither journal was then controlled by either the Muffa or the JHA pack. And all three authors were non-historians: two astronomers and a mathematician. But it should be noted that the publications occurred due to the openness of two historians: Olaf Pedersen and Margaret Rossiter, respectively — to the credit of both. (As asked elsewhere here: is it...
E4 Is it really this easy for a few rebel scholars to cripple (into free will-deprived zombiedom) a whole community, merely by inadvertently manipulating that community's lethal mixture of [A] historical bigotry (the inertia of which tends to produce embarrassing non-priority in discovery-making), plus [B] the resultant sociological bigotry of embarrassing (thus silent) careerist cooperation in academically-outwitted-power-genius archons' vindictive exilings? 25 May bigots' turf-possessiveness be succinctly summed up as: Pigotry? E5 Readers can examine this matter (bluntly condensed at www.dioi.org/cot.htm#tdps) carefully for themselves and then opt for which view to go with, on Diller's grand discovery: Occam's Razor? Or MuJhah theology? 26 I have friends who claim (2009) the Neugebauer MuJhah's bad side is dead. Reply: not until the truth of the Hipparchos-Stobaeus controversy is faced. Rigid, cohesive MuJhah (in search of a centurary) is part of am aggregation of academically-accepted-as-normal archonal-vanity shunning-rages inevitably lead to. And, as of 2009, MuJhah-triggered shunning is as undesec as ever in the history of astronomy zoo. For discussion of the mechanics & parallels of shunning's automatic instant-community-brandeath on central issues of its own field, see DIO 1.2 [Hj2 [1991]] (www.dioi.org/vols/w13.pdf pp.124-125); DIO 4.3 [i] [59 [1994]] (www.dioi.org/vols/w43.pdf); & DIO 14 [2] Epilog [2008] on sorority dominatrics. 26 E6 Sadly, an apostolic succession of modern cultists has outragedly deed common sense — and (needlessly) risked degrading still further their reputations vis-à-vis balanced scholarship — by fighting the obvious for four-fifths of a centurary, now, 1934-2015 (a span whose very magnitude has thus far only intensified the amoraly 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 & p.734 n.14), to [2] a contentless argument that Neugebauer's authority meant more than Diller's (top Mufoso to DR by phone c.2000), to [3] randomly walking or vaudevilliancean (k) reversing field by jetisoning 27 Neugebauer's theory (after our Table 1 [originally 1994, augm. 2002] had made it a joker 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 stoot-squirm (above, p.2) occurred in a curiously semi-MuJhah-theology apologia-paper Jones 2002E (frequently called "MuJhah" here to accent its cult-think roots). The new 2002 tactic: outside the beautifully consistent Table 1 (whose klimata were clearly published by Hipparchos as a whole at a single time), find some sort of inconsistency elsewhere in other coincidental that the 2002 JHA anti-crimeought 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#大大提高. 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-factor: www.dioi.org/ff.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 statistically at all.) 25 From www.dioi.org/nom.htm#gdbb: “There is no agnostic so ready to embrace doubt as a believer when faced with... evidence... inconveniently-inconsistent with his undillegedly-sacred tenets.” 26 At www.dioi.org/vols/w00.pdf p.31. Two pregnant questions are naturally suggested by the oure speculation (in search of a centurary) & thus suppressing public ground 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#htv.) [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 MuJhah priority than worshiping MuJhah-godopp 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 MuJhah’s very littleness? numbers of Hipparchos (who was glorious or notorious for varying his parameters throughout his career: [8] & [9]) — which can then be used to engender doubts of Diller’s [a] deduced obliquity (eq.2); & [b] data-base (Table 1: middle col.). MuJhah uses a two-prong attack. Prong [a]: At p.16 missing-a-takinking for a calculation. Reverse at p.17 (§f here). Prong [b]: More such confusion at MuJhah n.9. (§j below). E7 Putative JHA Suzuki MuJhah missed the stark contradiction between §E6’s prongs [a][b] (such embarrassing inevitable isssue from claimist last-ditch bandzai-determination tryanthingitis): the ε = 23°51’20” (eq.5) MuJhah pp.16-17 proposes in §F1’s prong [a], is inconsistent with that which would be produced by prong [b]’s proposed data-set-alteration. (Awful numbers 46 days at 0.24. §j) We now examine the §E6 [a][b] prongs of Jones’ indiscriminate creativity (which he’s very fortunately confirmed in DR’s work as well: see DIO 11.2’s cover), a classic MuJhah vision of ancients as semi-empirical-numeral-jugglers (Neugebauer 1975 pp.642-644; DIO 1.1.1 fn 24), contra Almajest 1.1.2’s description of ε’s capture being via the outdoor transit circle presaged in Rawlins 1982G. F Jones’ Subtraction from the Sum of Human Knowledge F1 MuJhah’s initial knowledge-subtractive (fn 55) attempt to undercut Diller 1934 was above-cited prong [a] (§E6). Diller had been 1st to discover that Hipparchos’ ultimately adopted obliquity was the least value 23°2/3, which is accurate (much better than the values MuJhah urges for H, implicitly&explicitly) and has since been validated in various ways (summarized at DIO 5.3 D3). MuJhah pp.15&17 notes that Ptolemy ascribes to Hipparchos the Eratosthenes value ε = 23°51’20” (eq.5). As §J5 notes, this is of little weight since we know (see the MuJhah author’s own Hipparchos entry in the Encyclopedia of Astronomy and Astrophysics 2000) that Hipparchos repeatedly changed parameters. F2 Jones 2002E pp.15-16 rigorously rounds L from arctan(35/0.6) to a precision of a few arcsec. In Jones 2002E p.16 rigorously rounds L from arctan(35/0.6) to a precision of whole degrees, 31°, in order to get the precise answer he is “straightforwardly” (p.15) seeking below at eq.7. Via Strabo 1.4.2, he “confirms” (p.16) L = 21700 degrees for Alexandria without noting that not just 31° but 30°58’ and Rawlins 1982G 31°04’ (the only L of the three which is independently relatable to Eratosthenes: Rawlins 1982G) are also consistent with 21700 degrees: i.e., no confirmation. When he subtracts the traditional Alexandria—Syene 5000 stades 31° = 5000 stades/(700 stades/degree) = 23°51’26” (7) he gets close to eq.5 instead of the 23°49’ (like eqs.16&17) which he would have gotten without that arbitrary 31° rounding. Compare via Occam’s Razor to an unjustified solution (Rawlins 1982G) which simultaneously solves precisely for three Eratosthenes data: [1] eq.5’s 23°51’ [2] L = 31°04’ (not 31’); & [3] Z = 7°15’1. Jones 2002E pp.15-16 baselessly says Strabo’s supposed (vs §F4) Alexandria “equinox-shadow-ratio” 3.5 & its implied L = 30°58’ were Eratosthenes’. A key miscomputation (§F3): L = 31° is seen (p.16) to be “derived from the equinocial shadow” though standard ancient measure of L got it via solstice29 (not equinox) data. The method is attractively simple (Almajest 1.1.2): just halve the sum of S.Solst & W.Solst app.noon zenith distances Z. But the process actually automatically produces the obliquity ε, if one just halves the very same two Zs’ difference. See eq.8. Therefore, unless an ancient astronomer deemed subtraction more challenging than addition, he would find ε as part of finding L, so (don’t miss Jones 2002E.p.15’s curious phrase “might suggest”): why compute already-known (via eq.8) obliquity ε through the more stupid long-division required (above: eq.7) by the MuJhah p.16 method29. 29Hipparchos may’ve used eq.5 at some point in his long career. But not when computing Table 1. 30 Solstices are used to find L because measuring equinoctial ±ε/g is vitiated by non-parallelness of instrumental & real equators. Which, among other reasons, is why all known observers before non-observer Ptolemy fixed their calendars by solstices, whose times are not affected by equatorial mis-set (or refraction or parallax). And even Ptolemy knew to find L by solstice observations; Almajest 1.12. The superiority of solstitial data (vs equinoctial) is well explained at R.Norton 1977 pp.81ff. 30 Whatever may be the merit of MuJhah’s try (our eq.7) at relating ε, 5000 stades, & (an inexplicably
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 adjacently claims that 2 other gnomon ratios, both equinoctial (Alexandria 3:5, Carthage 7:11), are *empirical* because of roundness, i.e., because the $se$ 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 1985G pp.263-264 & n.17.)

Alexandria $L = \arctan[se/g] = \arctan[3/5] = 36^\circ 58' \doteq 21700$ stades (11)

Carthage $L = \arctan[se/g] = \arctan[7/11] = 32^\circ 28' \doteq 22700$ stades (12)

Eq.11's 100% stade difference versus 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, but the difference between eq.11 & 12 is real. Jones, concluding the difference between Strabo 2.5.24 and eq.10 (i.e., why does Jones 2002E use the Alexandria discrepancy between eq.11 & Strabo's L), is ignoring the corresponding Carthage non-discrepancy? Carthage's 7:11 ratio is obviously non-empirical, since 32° 1/2 is waaaaaa (over 4°) too far from actual Carthage, faithfully 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 $g/se$ ratio but is the *longest/shortest* ratio $m/M$ for the Alexandria klima where $M = 14^r$ — just as the 7:11 ratio for Carthage is not the $se/g$ ratio but the $m/M$ ratio for the $M = 14^r/23$ klima around actual Carthage ($L = 36^\circ 51'N$, not $32^\circ 1/2$ which is the arctan of $se/g = 7/11$: eq.12), as 14° revealed by DR.

is 2° south of it. So, for purposes of testing the reality of eq.10 (& 12 eq.5), we may ignore Byzantine Hipparchos' (Carthage area) (entirely). But then: if we are reduced to Marseilles (Pytheas' native city: §G1), wouldn’t MuJHA’s p.17 sph trig be Pytheas’ calculation? In c.300 BC?! (Note: the later Almajest 2.6's calculated $S$Solst $se/g$ for Marseilles does not equal the Pytheas $se/g$. In 38.1 MuJHA’s author isn’t really correcting equinoctial $s/g$ (as in a commonplace argument) of just any old table, but the Pytheas’s $se/g$. While Carthage’s longitude is real, he may have been referring not to vertical instrument work but to calculation from an observation [badly corrupted by e.g., refraction, dip, etc] of arctangent amplitude $\arctan(2/3) = 33^\circ 41'$. Thus this latitude can also be explained by computing via Neugebauer 1975 p.37 eq.5a with $e = 24°$ for $M = 13^r/14$. In any case, MuJHA is correct that Byzantium was a klima for Hipparchos and Ptolemy, but that does not mean that eq.10 was unreal, especially since it does not quite agree with a latitude calculated by eq.10. Yet it seems that Hipparchos merely used the proximity of the $L$ corresponding to eq.15 (motivated by tradition or cataloguing priorities) to name the 15ª1/4 klima. (Did he treat Pytheas’ $s/g$ as a valued heritage [perhaps famous for its obvious precision] from the earliest days of observational transit-work astronomy? — evidently the oldest surviving transit (vertical instrument) raw observation, presumably prior even to those of Timocharis & Aristyllos.) For convenience, ancients casually merged-confused Syene city with the nearby 13ª1/2 klima (eq.17). Hipparchos similarly used (fn 55) the proximity of $\alpha$ Um's NPD (not its exact value) to indicate the position of the 12ª3/4 Cinnamon klima. (Are we to suppose that Hipparchos could count on real stars being exactly on a Z which agreed with indoor klima computations of $M$? Or are we to suppose that immortal (stellar) astronomer Hipparchos just indoor-computed (by eq.4) the Z of such major stars as $\alpha$ Um's (2nd magnitude present-day Polaris)?)

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/col.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 slippage (see www.dioi.org/qeq.htm#hhw!) to non-existent refereeing (and whose Editor-for-Life strikes hate-objects from his list of those scholars whom He might choose as His referees [an offense which alone destroys a journal's claim to having a reliable/honest refereeing process] even when He knows [DIO 6 13 §11 they are competent], one cannot be surprised when misunderstandings pass into print unapprehended. It also is 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 impairingly-anticipated destruction of communication inevitably created by frowning on semi-numerate archers who autodidact for their own inductive sterility by such creativity as shunning.

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 $s/g$ to $Z$, 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’ $\gamma p o v$ ("gnomon") as "index", when it is important that modern scientists analysing ancients’ work understand what instrument was being used?}

34 Strabo 1.44.4 & 2.1.12, 2.5.8&41. Some of the Strabo report has Hipparchos placing Massalia and Byzantium on the same 15º1/4 klima. While Massalia is close to the implied 43º parallel, Byzantium...
G Empirical Pytheas

G1 The precision of Pytheas’ 41 4/5 (eq.10) is about 1/600 of the gnomon’s height, which MuJHA thinks is unrealistic for early work. But this precision is (eq.2) just ordmag 1’, which is suspiciously consistent with careful outdoor measurement.36 NB: Strabo 7.3.1 regards Pytheas as an expert. He also reports (Strabo 2.5.8) Pytheas was a Massalaiar native, obviously enhancing odds that eq.10 is a real 17-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 speculation38 that a reality-accordant reading (s1, in eq.10) was actually non-real and thus also calculated. The MuJHA roundness arguments are curiously perverse. Highly rounded s/c ratios (Alexandria & Carthage) are obviously not directly empirical (and in these cases aren’t even s/c : [:F5], because in the real world, an outdoor eq.9 measurement of s/c will probably be as unround as eq.10 (Massalia). Summing up: MuJHA is simultaneously taking equinoctial s/c as directly empirical and solstitial s/c as non-empirical (and if eq.9 & eq.9 the reverse is true. So by 2 independent criteria, MuJHA’s 3 assessments of the Strabo s/c 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°14’1/4 and the Eratosthenes obliquity (eq.5) which MuJHA is proposing for Hipparchos.

Massalia L = arctan \[ \frac{\cos(7^\circ 41^{1/5})}{\sin(23^\circ 51^{20}')} \approx 43^\circ 01'24''N \] (13)

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

\[ s/s = \tan(43^\circ 00'24'') - 23^\circ 51^{20}' = 41.713/120 \neq (41 4/5)/120 \] (14)

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

36The experiment requires the gnomon’s verticality checked by plumb-bob, an art preceding Pytheas by 1000+ years. Some gauge ancients’ solar-data accuracy by reference to their star observations. But the Sun is immensely brighter & easier to place. The S.Solstices of Kallippos (~329/628 14/4) & Hipparchos (~134/626 14/4) were both accurate within 1° roughly precision (see Archimedes at Almajest 3.1), but such success requires 1’ accuracy since it is done by equal altitudes. (Raw human ocular accuracy is to c.1’.37: Rawlins 1985G. As ancient scientists were aware: DIO 14 [2].)

37I.e., agreeing with math not reality, obviously the normal situation for klimata: In 34.

38Based without justification upon claimed ([S]) significance of the eq.10 ratio’s precision. But how would Pytheas calculate Zs (thereby giving him his s/c via tangent) by subtracting obliquity L from (MuJHA p.17 middle equation), when he didn’t know \( \theta \) without using eq.8, which requires one already to have determined Zs, the equivalent (via arctan) of the very item supposedly being sought (s/c)?

So MuJHA has to speculate that 41 4/5 is Hipparchos’ calculation, via eq.4 using eq.5’s obliquity. Yet Strabo (1.4.5 & 2.5.41) 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°01’ — though we multiply demonstrate elsewhere ([S]) here that the L upon which MuJHA bases his argument is actually L = 43°04’, which undercuts his whole case for eq.'s 5’s involvement in the origin of Pytheas’ 41 4/5. (See, e.g., eq.16.) Note that, if using Eratosthenes’ obliquity (eq.) for M = 15°14/4, MuJHA’s hypothetical calculator would have (via eq.4) gotten not 41 4/5 but the Almajest 2.6 value, 41 4/5. And why, if MuJHA p.17 realizes this; so if it doesn’t, MuJHA’s hypothetical calculator would have (via eq.4) gotten not 41 4/5 but the Almajest 2.6 value, 41 4/5.

H New Implications of Marseilles Latitude 43°04’

H1 We now produce new, independent, & fruitful evidence for eq.10’s 41 4/5 being recently 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 emended39 and used for MuJHA p.17’s mathematical development). No, the actual reading is 43°04’. Remember that 43°01’ is just an indoor klima calculation (eq.13) for the 15°14’ klima, via sphen trig, having no mathematical relation whatever to empirical eq.10.

H2 We next reveal that Massalia’s Almajest 2.6 latitude L = 43°04’ was in truth elaborated by an? with Strabo’s (Eratosthenes’)? hipparchos’ 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:

\[ \text{arctan}[(41 4/5)/120] + 23^\circ 51^{20}' = 43^\circ 04' \] (15)

18 The fact that 43°04’ is the correct reading is confirmed by the GD latitudes for Marseilles (GD 2.10.8) and Byzantion (GD 3.11.5): both indisputably 43°1/2, which (in a work whose degree-angles are all Hipparchanly rounded [as also at fn 30] to the nearest 1°/12) is consistent with empirical eq.15’s 43°04’, not with calculated eq.13’s 43°01’.

39Though 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 Mufa-upsetting DR-related achievement is similar to Schaefer’s blindered attraction (Schaefer 2001) to depending upon the Almajest editions of Heiberg, Mannio, & Taliferro.

40See the vertical arrows of Neugebauer 1975 p.1313 Fig.291, each of which is (except the Equator-to-Earth 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, & Meroe aren’t klimata.) It is obvious at a glance that most of the L values of the dozen Hipparchos-Strabo klimata-latitudes are comfortably over-determined (about doubly, on average).

41Our 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°01’ and the eq.5 obliquity must be so rounded: to 43° & 23°5/6, resp, rather than 43° 23°5/6. It is thereby missing our eq.15 & getting accurate 43°04’, the 43°01’ erroneously rounded. See fn 43 for further discussion.

42Neugebauer 1975 p.336 rightly backs Honigmann in preferring the 5.7 Alexandria ratio. (The original ms’ ratio, not the Vitruvius-Ptolemy 3:5 ratio later substituted. See LCL’s Strabo 1:510, & Rawlins 1985G p.263&266 on GD Phaeros’ L vs Alexandria’s.) He (ideom) uses round \( r = 24'' \) (not eq.5) 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/c are consistent with L = 43°01’.)
I Inconsistencies' Inconsistencies & Hipparchos' Circuli

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

$$\epsilon = \arctan \frac{\cos[(15^\circ 1'/4)/(15^\circ 1'/5)/2]}{\tan 43^\circ 0'4''} = 23^\circ 49'25''$$ (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 stuff work — instead of recognizing the merit of a coherent solution to an inter-related (and uniformly unutilized: §2) data-pool, such as the Hipparchos-Strabo klimata.) From prong [b] ([E6]): in trying to weaken the Diller achievement, MuJHA states (p.17 [bracket added])

A. Diller and D. Rawlins have derived a value for the obliquity, 23°40', that yields a close fit to Strabo's stade figures (which are expressed in round hundreds of stades, thus to a precision of 1°/7). Unfortunately [7], there are some inconsistencies 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" riff-s off Rawlins 1985G p.263's solution to Pliny's gunci.46 Note Jones' implicit acceptance of Diller's general thesis (sph trig), which is never made explicit. As for "one or two modest changes in the intervals": any Jones alteration besides uniform shift of all data would produce a trepidation-level-hilariously choppy M-vs-L curve. So when MuJHA gets around to specifics, all he can do is agree (MuJHA n.9) with the reliable, long-accepted Neugebauer 1975 p.1313 rendition, except for injecting an odd anti-Diller escape-ploy (n.9): "restoration" by shifting the whole set down 100 stades, to "undo" a dreamed-up ancient tamperer's hypothetical addition of 100 stades onto the set.47

46 The Strabo Hipparchos klimata data are given mostly as intervals rather than as absolute values, which is why Diller 1934 refers to them as garbled. And there's been some very obvious reconstruction (to the writers' knowledge) but never (as far as Diller indicates), agrees but the work of decades of scholarly reconstruction (see pliny 475a vs. L. The Strabo Hipparchos klimata data are given mostly as intervals rather than as absolute values, which is why Diller 1934 refers to them as garbled. And there's been some very obvious reconstruction (to the writers' knowledge) but never (as far as Diller indicates), agrees but the work of decades of scholarly reconstruction (see

47 On 2009/8/18 (25° after the Greenwich Centenary lecture resulting in Rawlins 1985G), it dawned on super-swift DR that the circuli (fin 47k50) may be Hipparchan: [a] the L is his (eq.4); [b] one of the scheme's two bases is Rhodes (fin 50); [c] the Rhodes entry is not only mis-written (restoration: www.dio.org/cot.htm#ypsv), but its restored fraction, 77105, should've been rendered as 11/15 (ideum). This suggests backtracking to two closely successively colluderterative hands, early in the scheme's history, similar to the Hipparchos-school slip found at Rawlins 1991W eqs.23&24.

Jones' 1900-stade-shift proposal suspiciously — and invalidly — mimicks (uncited) Rawlins 1985G's valid restoration of the "circuli" of Pliny 6.39.211-218; the key distinction: while Evans 1987 & Jones 2002E (for huge JHA political advancement) replaced order with chaos (fn 55), DR's circuli-restitution did the reverse. (In other cases, e.g., the DIO 9.7 from continued-fraction decipherment of ancient year/length.) The M-KL pairs found in Pliny were not consistent (fin 50) via eq.4 for any Hipparchans. As shown at Rawlins 1985G p.263, an ancient dabbler had noted that the original scheme didn't give M = 12\text{°} 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 Hipparchian obliquity (eq.2): see Fig.1.

12 For Hipparchos' klimata, which are expressed by Strabo entirely in stades, Jones 2002E n.9 justifies the need for his proposed 100 stade shift via three non-stade data: [A] The star α UMi is stated by Marinos (GD 1.7.4) to have been placed at north polar distance NPD = 12°2/5 from the pole, which (by eq.4) for Diller's proposed Hipparchan obliquity 23°23/2 (eq.2) corresponds to 8700 stades, not Table 1's 8800. The Catch: Hipparchos used more than one obliquity (see discussion at §3F1), the other one being (Rawlins 1982G pp.367-368 & eq.27) \(\epsilon_{11} = 23°11/12\) (eq.6), which, if we compute with it (eq.4) for the Cinnamon klima's M = 12°3/4 yields L = 8700 stades — thereby providing one simple and quite plausible explanation of the discrepancy. (For another, see fn 55.) [B] Strabo 2.5.36 puts Syene at L = |e & M| = 15°1/2, a common ancient confusion. If taken as precise, both statements were false (§3B3), but Jones 2002E n.9 notes that if both are forced to be consistent, then L = 16700 stades, 100 stades below Table 1's Syene klima. Catches: [i] If we demand both Strabo statements' consistency, then (by eq.4):
that and tested for A.) By contrast, Diller’s solution \( (\epsilon = 23^\circ \frac{2}{3}/23^\circ A) \) easily falls within 1 standard deviation (sd) for both variables. \((\text{Probability } P \text{ exceeds } 2/3)\) I.e., Diller is again vindicated. Doubly. On the nose.

51 But who will be the 1st Mufoso — after over 80\(^\circ\) of bigotry, ungenerosity, & even viciousness — to own up to this?

54 Under the 2-dimensional elliptical-cross-section Gaussian surface representing the probability density \( p_d \) of any point on the \( \alpha \)-plane, probability \( P \) is the integrated volume exterior to the locus of points whose \( p_d \) equals that of the point in question:

\[
P = \frac{-e^{-(N-D)2/2}}{\sqrt{2\pi}} = e^{-(N-D)2/2} = e^{{-FD/2}}
\]

(19)

where \( S = \text{square-residuals sum there} \); \( S_m = \text{best-fit } \); \( \sigma = \text{single-datum standard deviation} \); sums’ relative difference \( D = \frac{(S - S_m)}{S_m} = S/S_m - 1 \); \( N = \text{no. of data}; \)

\( F = \text{degrees of freedom} = (N \text{ minus the number of unknowns, that being } 2 \text{ in this case).} \)

For the Princetitute 4-dimensional case (fn 7):

\( P = (1 + FD/2)/e^{{-FD/2}} \text{ = } 10^{0.118} \)

55 A general observation: the MuJHA paper (which never remotely approaches supplanting Diller’s well-founded improvement of our knowledge of antiquity by arriving at a comparably coherent vision: fn 55) omits mentioning any of the five then-known published post-1934 confirmations (now seven: DIO 5 [D3]) of Diller’s \( \epsilon \) and data-fit, data he gives, the mean is \( 24^\circ 07' \pm 5' \), disagreeing with Hipparchan \( \epsilon \). But, after shift-restoring (Rawlins 1985G p.263) the \( M \) by \(-1^\circ \) and \(-4^\circ\) (an amount explained at fn 47), we find the corresponding mean for the reconstructed \( 23^\circ 37' \pm 2' \), statistically consistent with the now-thoroughly-established (fn 48) Hipparchan \( \epsilon \) of eq.2 — and fitting this \( \epsilon \) with far less scatter. (Shifting Pliny’s \( M \) by a few more negative time-min can still show comparably small scatter, but the resulting low \( \epsilon \) values are ruled out by the histories of both Greek astronomy & the Earth’s actual obliquity.) Moreover, the Rawlins 1985G reconstruction of the original ancient scheme ends up placing Rhodos at \( 14^\circ 1/2 \), the traditional Rhodos klima \( M \). The DR reconstruction also allows us to recover (Rawlins 1985G p.263) the circuli’s origin: using eqs.2&4, we find for Pliny’s Alexandria klima (\( M = 13^\circ 56' \)) \( \tan L = 340' \); and for his Baético \( 14^\circ 4/3 \), \( \tan L = 4490' \). Continuance of the first equation with fractions’ sexagesimal expansions) would produce ratio-representations of, resp. 4/7 and 11/15. The product of the denominators explains the blatantly obvious common denominator (105) of the rest of the scheme. These are the details behind the statement at Rawlins 1985G p.263 that the circuli’s original linear equation (\( tan L = [303J – 385/J]105 \)) arose historically when an ancient mathematician just drew (on a graph of \( M \)-vs-\( L \)) a straight line through the two points representing the key ancient klimata: Alexandria and Rhodes. Linearity only worked because the scheme was fit by its ancient inventor to a much narrower (Mediterranean) range of \( L \) than Table 1’s: see Fig.1. The very enormity of Table 1’s range is what allowed the discernment (fn 51) of an undesirably precise sph trig signal.

51 The Diller-DR solution is superior even to the best-fit solution (eq.18), which fails for one klima: the \( L = 14^\circ 1/4 \) is a non-match. By contrast: though the Diller solution \( (\epsilon = 23^\circ 2/3 \text{ } A = 100 \text{ stades}) \) proved one of a mean-residual that’s barely larger (than the best-fit’s), not one of the 14 residuals exceeds 50 stades after DIO’s 5’ rounding of all computed \( L \) prior to their conversion to stades (see Table 2), \( \text{NB: BOTH of Table 2’s rounding-steps are actually normal thus non-arbitrary: } 5^\circ \text{ & } 100^\circ \)

52 Check out Neugebauer 1975 p.74.14. Pure Muffiosity. And by now merely an especially precious larf-reminder of the reliability of establishment exiling of ideas and persons. The former academic crime is longterm-worse than the latter; but, following exile-decree, a shunned scholar’s fertilización can monotonically evolve into requiring an unexpectedly laborious and complex fear&m smear campaign, to maintain perpetual suppression of a heretic — in order that a debate-fleeing cower-operator mogul’s decree sticks.

53 See fn 56, Rawlins 1991W [D4&D2], and www.dioi.org/cot.htm#vskc.

54 The matrix relating \( A \& \sigma \)’s stdves & correlation to \( \sigma \) is diagonalized by similarity transformation (50° rotation) ensuring separation of variables. One variable’s sd is 10 times the other’s, but normalization creates isotropic pd. Another advantage via standard cartesian—polar transformation to yield eq.19. [Details: www.dioi.org/bib.htm.] The proposed process (applicable to all such Gaussian bivariate problems) is unaltered because the extreme volume fraction of the whole is unaltered by the transformations. The rightmost form of eq.19 also equals normalized pd, for any number of unknowns.

55 Some sources are cited (n.1), though key evidence discussed here is not. Arguments of Diller 1934 & Rawlins 1982C for Hipparchus’ sph trig & \( \epsilon = 23^\circ 2/3 \) are cited at MuJHA n.8, but there’s no mention of \( 23^\circ 2/3 \) & spherical confirmation by Nadal & Brunet 1986, or of the later 1994 sudden-surprise end-of-controversy exactness of DIO 4.2 Table 1’s fit. Likewise, if MuJHA is going to [a cite
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Since Hipparchos changed (§F1) adopted parameters (e.g., ς, solar&lunar elements: Rawlins 1982C pp.367f & Rawlins 1991W §§K-R) as his researches progressed, the tactic of bringing a nakedly-alone, extraneous, incoherent Hipparchos datum against a member of a coherent data-set (Table 1) is pointless except (fn 45) as a lawyeresque ploy to join & prop-up the shunning of Diller’s discovery. Since MuJHA’s theories are non-exclusive (DIO 11.1 p.26 n.1) using them (e.g., §G1) to down Diller’s coherent35 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 real Diller-DH Table 1 asked for no such leniency for the Data’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 ε = 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 cite the ultra-tight-fit new table, or at the very least: the info that D’R

Rawlins 1982C (MuJHA n.8) while floating the undetailed Pliny-circuit confirmation of 23°23/3 at Rawlins 1982C p.368 (ignoring its eq.28’s extra evidence for 23°23/3), and [b] scoff at Pliny’s precise circuli as “crude” (MuJHA n.11, thereby sneering at Longitude Zero-referenced Rawlins 1985G), then competent JHA rerefering 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°23/3. Cornered again (as at §1), Authoritative-in-His-Own-Mind Jones can only effect a fit by describing as “time fit by deconstruction” the Trig-accident 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).

53 x C31. And the next MuJHA still insists on 8800 stades. But neither Hipparchos nor Strabo did so. MuJHA complains that a star’s 8700 doesn’t equal the Cinnamon klima’s 8800 (Strabo 2.5.7&35.5). (He thinks Hipparchos believed each klima had a bright star’s position 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°23/3 (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 Mufose mess: 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, DIO 9.1 p.2); as has JHA (§1 E1; www.dioi.org/fff.htm#sckv); & Evans (www.dioi.org/cot.hrm/gine, gge.htm#pppp). MuJHA yields nought but chaotic (e.g., §E6 item [c]) muddying of others’ achievements. Was this its cultish destructive aim? Unrefereed Jones 2002E seems worthless all DR referred finds touched-on: L = 31°4’0” [§F2], circuli (fn 54), klimata (fn 44), refereed by Isis (1982), Greenwhich (1985), & Isis (2002), resp. DIO 1.2 [H2 [g]]’s 1991 prediction of MuJHA DR-credit-denial tactic: “Publish a wild speculation (unattested method or inferior 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 scheme (Table 2), though unnoticed by MuJHA. 8800 stades are extraordinary. Yet MuJHA’s 9.1 stade’s circularity is confirmed by the Neugebauer-Diller data-set (above & §2: 8700 ≠ 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°34/ was initially pondered by Neugebauer 1975 p.335 n.23.

55 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 referred to 1982C, but not the revealing later DIO update’s new clincher-evidence in Rawlins 1994L §C. (Our comments: §1 fn 2&7, & www.dioi.org/vols/w80.pdf, adding (Table 2) standard ancient 1/12 ruling (fn 41) upper Diller’s score; it now yields his match ([β]) to all 13 klimata. Caketop-cherry: check contenders’ L for [2009 ed.’s] previously unreferenced 14th klima, that at M = 12°3’, the Equator. Jones, 100 stades; Neugebauer, 1500 stades; Diller-DR, 0 stades. Jones P < 0.01; 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 routine, of an anti-Occam brand already spoofed at DIO 2.3 §8 §C3. And the next Mufffiose 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 eternalness 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 perverse analogy at www.dioi.org/epi.htm#dvnv.) And so we observe (yet again) a familiar cardiac-Xerxesur at the spectacle of anti-imperial rebellion and heresy being Taken-Seriously (Isis: Thurston 2002S this 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 invaluablely unambiguous: Table 1 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 wriggle-out mink. They can only turn purple trying.

K2 Runnin-round like Chickens with — B-But, Their Heads Are Enormously ON! Given frantic Browner-motion 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 known vainly inated ineld historians-of-astronomy back in 1991’s DIO 1.2 §B9): But I don’t even know the girl & I was nowhere near Judy that night; anyway, she consented.

K3 For HIMSELF & above at DIO 4.2 p.56 Table 1. The table is so devastating to said clique’s insultingly stolid pretense (that Diller’s finding cannot be Received by Accepted Society), that DIO is for the 3rd time publishing
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 Merœîa &laquo;actual value.) The long-overdue detailed DIO counter-attack against Mufosi’s 75’s shun-trash of Diller is also appropriately in-full.

K4 It is notable that all three of the scholars who have gone into print to repel Diller’s discovery are historians who have served time at the Princetitute. As we asked at DIO 4.3 §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 discussion historical issues at all. (Of course, one may easily understand why certain moral prelates might wish to render reason and competence irrelevant to the evolution of ideas in [the hist.astron. field].) For, if even the most logically & evidently one-sided controversies are to be decreed [see NCS at DIO 2.3:38 §§C20&C25] as indefensibly irrepressible, then — why investigate anything?” When this quote was applied to a few Velškvskians 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 Mufa-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.

References

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 insouciance. B2 When post-Truthts “Free Press” — “Courthouse” — brainmelt, the US acheives what a few newspapers reader doubleskate 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-sanity-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 project of concerned 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 demonization 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 fn8 & &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 148 &19 [1994], www.dioi.org/pro.htm/#hbsa.) To point out the contradiction between ultra-profitable courtroom over-over-overdone hyperfine-racially-actually 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 FreemikePress 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, as many (DIO 4.2 §19 §§H7 & HS) discouraged invading Iraq, he didn’t excommunicate Catholic soldiers. Being busy eugenic saintly J.Gramick 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 instant, instead McCarthyistically devoting ones of their obsessively 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 §F, which obviously was itself insufficiently cynical as to how nutty courtrooms had become.) A neat way to put 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 §E3.

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

11But notice §E15.

12So readers won’t miss DR’s take: if crime trials are part of a war on crime, then the occasional courtroom justice-miscarry 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 “criminal”. See fn 9 & DIO 6 §14 §C7.

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 Vatican’s culture-of-life propaganda against population control. Two eye-openers on the Vatican’s culture-of-life propaganda against population control. Two eye-openers on the Vatican’s culture-of-life propaganda against population control.

B3 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? — Any tears? After 6 Dr. Harper, his healing — "Butcher" Grant — did.

B4 We know the trendresult of freemarket capitalism is monotonic wealth-concentration. (Vis-à-vis entropy, this is the anti-twin of physicists’ heat-death nightmare.) An organization can’t function robustly if all blood is in the palm. A restorative confiscatory-redistribution not just of income but of wealth would help. (When the richest 2% own perhaps most of it, of tens of thousands of youths at Antietam, Lincoln shed as many tears? — Any tears? After 6 Dr. Harper, his healing — "Butcher" Grant — did.

13Mussolini’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-trafcking, despite the contrary. (See fn 9 & www.dioi.org/err.htm#blgg) cancel will use ANY means to hold onto power/Eriches. Will world greed-death equilibrium arrive (whether or not securely stable) when all but rulers are unlanded peons and/or “terrorists”?
C Occasional DIO Prescience?

DR thought[18] O'Simpson wouldn't get off (1st time around). But otherwise DIO commentary scored some hits outside scientific history. (For indications inside: www.dioi.org/win.htm.)

C1 Our opposition to mammograms was published in 1992 (DIO 2 111 §A8 [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, aggravation, pain, terror, 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 gap; & [b] started us down the road to ever-deeper, vicious-circle debt-addiction, which could end in a sudden war of desperation, or a slomo foreclosure-sale (who’s funding stimulus-paybacks?) akin to the under-rated, DIOliciously tasteless 1979 farce-film, Ameriocation.

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 guru, privately enjoying the perks 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#ttbj.)

C4 On 2009/11/30, DIO posted www.dioi.org/pro.htm#ppj) the charge that the Dodbos’ pander to us to show that some of the Left) was a fake, predicting that 2009 would bring as phony a Choice as Johnson “versus” Goldwater in 1964, when the War candidate was for war, and the Peace candidate was for war. (The issue as also at §B2) is separate from that of the war’s wisdom. The point here is simply the “bipartisan” truth behind the pretense that US elections still mean much, on the most important issues.) [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 1 learned of ongoing detailed expert research indicating that Mark Felt’s sole source was indeed our nominee. We await further developments.

C6 The following was posted at www.dioi.org/pre.htm on 2007/11/20 (2nd to the day before Obama’s swearing-in) under the header, “Hilla the Hun Against the World”: “...ấmily values and in aspects as international as any other Miss Universe.”

D Two Unjustly Neglected Nobels

D1 And the Nobel Prize for Chemistry goes to . . . Barry Bonds. (Bonds isn’t smiling. He’s not quite into every kind of needling.49)

D2 The Nobel Prize for Physics goes to PC4 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 Jesus’ post-crucifixion non-public circulation = Resurrectile Disfunction.

E3 Middle class = only domestic fiscal blood left for rich-owned-gov’t’s fangs to suck.


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

E6 Defynitions

E7 The capitalist West’s 0th wealth-$yman & [b] started us down the road to ever-deeper, vicious-circle debt-addiction, which could end in a sudden war of desperation, or a slomo foreclosure-sale (who’s funding stimulus-paybacks?) akin to the under-rated, DIOliciously tasteless 1979 farce-film, Ameriocation.

F. Moloney

F1 Jesus as a scally canny typical cult-guru, privately enjoying the percs of wealth to the point that US elections still mean much, on the most important issues. (The issue as also at §B2) is separate from that of the war’s wisdom. The point here is simply the “bipartisan” truth behind the pretense that US elections still mean much, on the most important issues.) [Added 2011: 1864 fake war-choice had Lincoln “vs” own General (!) McClellan of Peace Party Dems.]

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F  Religion & Atheism

F1  Do popular religions fight human cloning because they resent the competition?

F2  I’m outraged at Danish newspaper-publication of Moslem-insulting cartoons. The European press should be ashamed of itself — for not publishing said cartoons continuously & prominently throughout the last 30 years, to stimulate Moslem immigrants into revealing their hothead-intolerance (too-often violent, even murderous) early enough to warn Europe that its own tragic internal combine of cheap-labor-exploiting Christian capitalists and bleeding-heart socialists was about to import a virulent religious cancer into Europe, and so risk poisoning (perhaps indefinitely) the most civilized region of the world.

F3  Organized Religion as Celebrity-Philosophy.  Celeb-obsession & churches pop-culture substitutes for reality. Media commentators justly laugh at celebrities who’re famous for being famous. Why not a parallel observation that mass-religion is believed-in primarily for being believed-in?  (See www.dioi.org/rel.htm#msj.)  What other evidence is there, of the medbiz which (while capable of grand scientific miracles) funds pols’ campaigns and has wasted a massive fraction of net national medical costs upon minimally-useful, counterproductive, or dangerous passing-fads (see Shaw’s Doctor’s Dilemma), e.g., radical mastectomies, radium treatments, tonsillectomies, HRT, hysterectomies, CT-scans, mammograms, over-radiation in general, etc.

F4  When people differ, they can communicate on realities; but not on faith. (As apologists actually emphasize, to evade empirical testing.) Mass-faith is maintained by insular indoctrination, a robotically inculcated bar to communication: a divider of people.

F5  Nun Dare Call It Teasin’?  It’s common in convents to find crucifix-icons on a wall of every room: a skin-tight nearly-naked young guy nailed to a cross. Bunnyrabbit religion = kill. (Woody Shakespeare = Christopher Marlowe)

F6  If heaven exists, Beethoven, Berlioz, Wagner, Liszt, Shostakovich, Barber are creating ennobling music there. So the universe’s top mass-murderer according to anti-abortioners’ own reasoning. See DIO 4.2 [§16D].
G Shorts

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

G2 Dueling academics’ fear of cult or math mix-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#ctdh.)

G3 Needlessly-Divisive PC At War With Itself. [a] To PCs, “native American” implies: realer (North) American than a US-born WASP. [b] But in such also-laboriously-multisyllabic stroking-terms as “Italian-American”, “Serbian-American”, etc, the 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 slavers throughout history. Yet the central implicit thesis of white-guilt-flogging, all-is-race-hate “historians” is that blacks couldn’t have served sardine-crowded slaves to the Americas in chains&filth, just as profitably&crucially 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, 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 9G.

G6 Gov’t by 535 Messalinas. OK, so it’s hollerin’-good-showbiz for TV ‘snews spat-show-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.

30Johnny Carson’s eulogy for Abe Lincoln: "without whom, we would not have the dunk-stuff."
31See DIO 4.3 §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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