Ancient Spherical Trig:

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

Diller Verified on Klimata After 75° 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 falsiability: Crock of Ages Clefted But JHA-Xerxesed

DIO’s rise in prominence (e.g., NYTimes 2009/9/8 Science)1 has only fanned the coivering 
DIO for its 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 classicist 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°/3. How have history-of-astronomy’s “Muffia” & JHA cults thanked Diller for one of the great contributions to the history of math? Muffia found Neugebauer abusively attacked Diller for 40%. ON’s competing pseudo-solution was long sought though (Table 3) it fits only 6 of the 14 Hipparchos klimata, while the proscribed Diller-DR scheme has always fit virtually (now exactly) ALL 14 data at issue; yet M’kteers for decades (1934-2002) kept certifying ON’s seemingly ageless crock. 
Finally, in 2002, just as Isis honored Diller-DR’s proof with publication ([3 fn 23), the 68’ Muffia-damm broke: A.Jones’ JHA paper dumped Neugebauer’s folly. But the same paper (oft called “MuJHA”4 within) Xeresially insta-replaces it with a new crop (3 §6E here), trying to weasel (DIO 11.3 §6 p.70) out of crediting Diller’s ever-more-obvious success. 
The minilob to the totality of Diller’s victory collapsed in 2009 (on April 1, aptly) when the only日渐 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/ct.htm#jqsk) of all previously unsolved ancient lunar speeds use attested, normal ancient methodology & produce precisely all 6 attested 4-digit integers: 24 digits (no other theory does either: www.dioi.org/thr.htm#zxxp), assuming Greeks-Seleukid use of now-lost 13th century BC eclipses. Finding no new math or alternate eclipses to complain of, Muffiosi just scoff at data-remoteness. But neutral experts’ dates for Babylon observations’ start dovetailed5 with DIO’s theory: Isis 83:474 (1992): c.1350 BC.

1 The NYTimes article (link to full version: www.dioi.org/ct.htm#qzq) notes that DIO opponents display a lamentably common mental impenetrability. Cooperatively proving the point: during our 1997 of existence, the publishers of the JHA & DIO (top US & UK research astronomy journals: 3 §4 fn 56 here) have never communicated, despite urgings at, e.g., DIO 13 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 JH-A-Muffia output. Latter: ¶1 head, §3 §E7&fnm 55&56, DIO 6 §3 fn 78. Shirt-unstiffings are entirely reactive to truth-wars by usual establishment anti-rebel plows: money, shunning, money, censorship, money, kept “experts”, money, goons, money, threats, money.
3 E.g., 1991&1994 (see DIO 4.2 p.55 & fn 2), as well as J.Britton by phone (c.2000). No Muffioso has yet faced the Diller-DR theory’s subtle-as-a-ton-of-hits preferablety. History of astronomy forums (by contrast to Isis printing Thurston 2002S) [vs now! DIO 22 §1 2018] won’t even cite the perfect fit Diller-DR have achieved. Britton&Jones just rank anchor-authority above statistics! Muffihink lives.
4 We usually call this unrefereed paper “MuJHA” to stress the Muffa-output here, which has never been any one scholar’s responsibility. Extra weirdness: MuJHA 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. [ii] Hipparchos used sph trig; but the JHA then pretended that Diller didn’t prove this important contribution to math history, instead acting as if the Muffia-JHA gang was doing so itself! — this, after 68y of Muffia denial (e.g., Neugebauer 1975 p.337; DIO 7.1 §2) of the same proposition. (Similar side-switch—to claim-juump: www.dioi.org/lff.htm#ctw.)
5 Which parallels our 2001 star-dating the Great Pyramid to c.2600 BC (Nature 412:699). This we’d known was a conventional figure; but DIO was unaware of the 1350 BC date when researching ancient lunar theory, so our 13th century BC results constitute an entirely independent multiple verication.
an outdoor topocentric observation to find geocentric longitude. Thus the sign mixup would naturally cause an error of about $-2^{2}/3$ or $-40^\circ$ — and the laughably impossible “observational” longitude he reports is indeed (Evans 1987 p.275 n.50): “too small by about 40°”. Typically, Evans has had no comment since, despite DR [South Bend, IN, 1997 June, face-to-face] and Hugh Thurston [by letter] gently bringing the matter to his attention.\textsuperscript{3} After correcting for this Muff, we can verify the admirable smallness of the 1981 observational error of Evans (a dedicated student of ancient instrument and possession of a steady hand, since the cross-staff requires it): merely one or two arcmin — just the sort of accuracy DR has consistently\textsuperscript{4} ascribed to the best ancient-eyed observations.

A3 Only a scholar catering to modern Hist.astron’s cult-klan could straightforwardly propose that instead of the triple $2^{2}/3$ — nearly triple the looser $2^{2}/3$ — is observational and so by implication helps excuse the tight adherence of Ptolemy’s “observations” to indoor-calculations (i.e., frauds) while disagreeing hugely with the outdoor sky.

A4 Note that a major member of Ptolemy’s fake’ed “observations” (\textit{Almagest} 5.12-13) is also off by $2^{2}/3$. (See discussion at R.Newton 1977 pp.182-191. Also DIO 8 §1 fn.13.) Scribbling a drawing will give one an idea of how ridiculous this is: mislocating a disk so grossly that the really and theoretical disks (1/2 wide in these lunar cases) don’t even come close to overlapping,\textsuperscript{4} the very feat Evans misclaimed he’d personally achieved in 1981 and is now too embarrassed and too steeped in Muffa academic integrity\textsuperscript{5} to retract.

A5 NB: After the three-fold (§A6) new \textbf{four-fold} (§F3) collapse of Evans’ implicit alibi (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 alibi was designed for. (Which figures, since evidence has little relation to that cult’s belief-system.) It hasn’t occurred to Nottroth (who is always pointing out: we don’t include philosophy of science) to ponder a simple question: if devotion to our favorite positions keeps leading us into embarrassing crackpot-level muffs, \textit{e.g.}, §A1 & DIO 2.3 $\S$§C10-C15), does this not suggest that said positions are less than completely secure\textsuperscript{6}

\textsuperscript{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 7.

\textsuperscript{5}Ev., Rawlins 1982C p.263 & n.17, Rawlins 1983G passim, & Rawlins 1985H.

\textsuperscript{6}Almajest 3.1.3) is also off by $2^{2}/3$. (See discussion at R.Newton 1977 pp.182-191. Also DIO 8 §1 fn.13.) Scribbling a drawing will give one an idea of how ridiculous this is: mislocating a disk so close to $2^{2}/3$, is now too embarrassed and too steeped in Muffa academic integrity\textsuperscript{5} to retract.

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: $0^\circ$ 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, 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 tickety 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$^\circ$ from the Sun.

B2 Ptolemy tells us (\textit{Almagest} 3.1) that Hipparchos used the eclipses of $-145/4/21-22$ and $-134/3/20-21$ to trying spotting Spica. The results: $173^\circ 1/2 & 174^\circ 3/4, resp, a terrible disagreement — over a degree! (Remember: the lunar semi-diameter is merely 1/4 degree.) So, we now apply the parallax-sign-error theory to both eclipses.

\textsuperscript{7}Velikovskian boldness and correctness has re-dated the Ancient Star Catalog by ordmag a millennium is that it showed that one could prove anything with statistics. (Is the Muffa aware that the \textit{Almagest} is a late medieval document, and that the Nabonassar epoch [747 BC for most of us] is actually from the AD era? Full information available from the Velikovskians’ least favorite mongoose. Leroy Ellenberger, 3929 Utah Str, St.Louis, MO 63116; phone 314-772-4286. See also the excellent \textit{Isis} review of Fomenko’s book. A central technical flaw undoing the entire Fomenko \textit{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 splitting the falsity of his alibi-for-silence-on-errors pretense (Tables IV&V) that the Star Catalog was stolen from Hipparchos by Muffa-hero C.Ptolemy, to the only other extant Hipparchos eclipse, which we discovered was used to position his hitherto-inexplicably ultra-misplaced fundamental star Regulus.

JHA 23.3 frontispiece:173­183; 1992/8) by Muffa capo N.Swerdlow, a disaster undercut the clique has even (presumably unknowingly) brought in Velikovskian-circle expertise to denigrate the 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

\textsuperscript{3}Indeed, Muffa desperation to reject non-cult common-sense has now reached the point where the clique even (presumably unknowingly) brought in Velikovskian-circle expertise to denigrate RN-DR work. During my 1995/2/26 chat with B. van Dalen, he mentioned that the reason his
B3 For each eclipse, Hipparchos’ method was:

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

$$\Delta \lambda = \lambda_s - \lambda'_M \tag{1}$$

[b] Compute from his tables the longitudinal lunar parallax $p_s$, which is the difference between $\lambda_M$ and the Moon’s true (geocentric) longitude $\lambda'_M$:

$$p_s = \lambda'_M - \lambda_M \tag{2}$$

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

$$\lambda_M = \lambda_S \pm 180^\circ \tag{3}$$

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

$$\lambda_s = \lambda_S + p_s + \Delta \lambda \pm 180^\circ \tag{4}$$

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

$$\lambda_x = \lambda_S - p_s + \Delta \lambda \pm 180^\circ \tag{5}$$

B5 For the −145/4/21-22 eclipse: the outdoor longitude difference $\Delta \lambda$ (between Spica & the Moon) at the time when Hipparchos’ indoor lunisolar theory predicted mid-eclipse (23:38 Lindos Mean Time),11 was about $-33^\circ.8$, so he likely measured close to $\Delta \lambda = -33^\circ.5/6$. [b] Hipparchos’ PH solar theory12 placed the Sun at about $\lambda_S = 27^\circ.2/3$ at this time. (So geocentric $\lambda_M = 207^\circ.2/3$.) [c] The Almajest syzygial lunar theory puts the eclipsed Moon at $58^\circ.3$. (We define 1° as one Earth-radius.) [d] For this distance at the Rhodes klima $36^\circ$N, the Almajest 2.13 parallax tables give $p_s = +20^\circ$. [e] So eq. 5 (which, recall, proposes using the wrong sign for $p_s$) yields:

$$\lambda_x = 27^\circ.2/3 - 1^\circ.3/1 - (-33^\circ.5/6) + 180^\circ = 173^\circ.1/2. \tag{6}$$

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

$$\lambda_x = 357^\circ.1/4 - (-1^\circ.4) + (-2^\circ.3/4) - 180^\circ = 174^\circ.3/4. \tag{7}$$

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

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

C The Hipparchos Lunar Model’s Scale

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

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

D Hipparchos’ Sph Trig Reconconfirmed by His Parallax Corrections

D1 It has long been recognized (e.g., Neugebauer 1975 p.323) that parallax tables were in use in the 2nd century BC. (This was always obvious from Almajest 5.5, but perhaps no one has previously caught the implication for the onset of spherical trigonometry.14 Neugebauer loc cit explicitly contradicts it.) These tables were essentially the same as

13 We assume accurate observation and the ancients’ common practice of rounding quantities to fractional degrees. Our fits here are almost too good (in 22), which can be due to [a] Hipparchos having averaged a careful series of mid-eclipse data for each eclipse, and-or [b] DR having acquired Ptolemy’s habit of favoring (postulating likely Hipparchian) roundings that lead to exact agreement. But the putative latter factor’s net effect is trifling. [Rawlins 2018U] & [Toomer 1984 p.251 n.49] corrected Hipparchos’ 14 Rhodes observation was the −146 S.Solst, 14° of the 3 cardinal-pt data he computed the PH orbit from, after his −145 Vernal Equinox, in time to predict mideclipse for his planned −145/4/21 Spica placement.]

14 Toomer 1984 p.227 n.21 correctly points out the accuracy of Hipparchos’ longitudinal parallax correction for the lunar-solar observation he made on −126/5/2 at 6:20 Rhodes Apparent Time. (See also Neugebauer 1975 pp.92 & 323.) His correction was rightly positive (so he [or a member of his school] had by this late point in his career straightened out the signage of his procedure): $15^\circ/8^-\n15^\circ/12$. (That is, $+12^\circ1/2$, apt to a lunar distance of well over 60 Earth-radii.) The actual parallax was
those used by Ptolemy 3 centuries later. Since parallax tables are constructed by spherical trigonometry, this finding confirms once again 15 the contention of Diller, van der Waerden. 16

Dicks 1994, and DIO that spherical trig throw in the 2nd century BC.

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

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

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

D5 It has been (Rawlins 1991H fn 7 & §C4 [& Rawlins 2018U loc cit]) theorized that the PH solar theory & tables were based upon observations of [−146 &] −145 and that his subsequent UH tables were based upon observations of −142/9 & −143/6 (thus could not be earlier than the latter date) — independently suggesting that Hipparchos’ PH tables were used by him during the period −145/4/21–22 to −134/3/20–21.

E Sources of Error in Hipparchos’ Placement of Regulus

E1 We now turn to the 3rd (and only other) eclipse known to have been observed & reported by Hipparchos — an eclipse which happens to have occurred near the star Regulus. Two initial comments: [i] Only 2 stars’ explicit Hipparchos longitudes survive (Almagest 7.2): Regulus 119°5/6 and Spica 174°, ideal fundamental stars, the nearest 1st magnitude stars to the ecliptic. [ii] For Spica, the discrepant eclipse-based results he complains of (§§A6&B2) evidently (fn 22) induced him later to opt for placing this star instead 18 by conventional astroalbique technique (which was in fact more reliable than his min-sight eclipse method); however, Regulus is the zodiacal bright star with the largest Hipparchos Δ error for his Ancient Star Catalog’s epoch (−126.28: Rawlins 1991H §F4): −35°. Rawlins 1991W (fn 147) remarked aloud at the enormity of this error (which led Ptolemy into a fraudulent copy of it: DIO 8 §11 ⊗), despairing as to whether its explanation would ever 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°3/4, though that from Ptolemy’s tables was +19° for his ludicrous lunar distance of 43 Earth-radii. (The Neugebauer 1975 p.92 value [16°] is explicitly based upon Ptolemy’s simple syzygial lunar model, not his final one.Obviously, Hipparchos did not share Claudius Indoeller’s notorious belief that the Moon’s angular size varies by a huge factor (of up to nearly two) Indeed, the smallness of Hipparchos’ 12°1/2 parallax for the −126/5/2 observation is due to the parallax calculations used a conventional lunar distance (as we already realized at §C1). We may check this by computing via modern theory the lunar parallax on the assumption that the geocentric lunar distance was 60 Earth-radii (vs 57 in reality): 14°, thus correcting Hipparchos’ −126/5/2 observation of topocentric lunar longitude 351°2/3, we have 351°26′, for which the nearest Hipparchian approximation would be 351°3/8, which is just the Hipparchian geocentric longitude reported at Almagest 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 §9. 17 We may get a glimpse of the inevitable escape routes (from this evidence) at A.Bowen & B.Goldstein Amer Philos Soc Proc 135.2/233 (1991) where trig 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 −131/11/7–18 eclipse (record not extant), the result would have been roughly 119°5/2, 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).

fundamental astronomy was in the evening, and Rawlins 1991W fn 138 had remarked on this proposal’s redemption by Rawlins 1991H §G1. The only important exception seemed possibly to be Regulus. But the present results resolve the problem [indicating that Regulus alone among major Hipparchos-Ptolemy stars was not placed by astrolabe], so we may conclude that all the Hipparchos principal stars’ astrolabe-based placements occurred in the evening, just after the Sun’s setting, using a crescent Moon: Rawlins 1991H §G2.)

E2 Inductive detectives’ highest ecstasy is the experience of coherent fruitfulness: when a theory already successfully in one case is applied to an independent case and the very same theory comes up aces. (E.g., Jones & Duke at DIO 11.2 [2003] cover & p.33; A.Diller’s vindication below at §3 §E3; www.dio.org/cem.htm#xiv.) Our outstanding mystery here is Hipparchos’ perplexing super-misplacement (§E1), and a so-far successful theory is that eclipse-parallax-sign-error accounts for Hipparchos’ horrible stellar longitude errors. If the theory is valid, can it also explain the only other attested (Almagest 7.2) Hipparchian stellar longitude, the very worst of the lot: Regulus?

E3 We now apply §B3’s method — already good with both his two eclipse-based Spica observations (§B5&B6) plus Evans’ 1981 case — to Regulus & the nearby −140/1/27–28 eclipse (the only other Hipparchos-observed eclipse record we have: Almagest 6.5&B9).

E4 For the −140/1/27–28 eclipse: [a] At tabular−mid-eclipse (22°), actual Δλ was 5°07′, so (especially given his now-famous proclivity for integral data), he likely expressed the measurement as exactly Δλ = 5°. [b] Hipparchos’ PH theory gives λ3 = 305°09′ (Almagest 6.5 makes it 305°08′), so he would record λ3 = 125°1/6. [c] Almagest lunar theory distance = 5°43/4°. [d] So for Rhodos, Almagest parallelax tables, ps = 29′/2′ would become 17°2′ after sign-misplace. [e] So the 5° yields in 8° (c. 1°/6) of Hipparchos-Ptolemy 1°100′ prevision (from −140 to catalog epoch −126.28): λ3 = 305°1/6 + (−10°/2 + (−5°/) = −180° + 1°/6 = 119°5/6. (8)

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

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

19 Almagest 6.5 just computes the time of mid-eclipse as 22:10 by finding when the Hipparchos-Ptolemy lunisolar tables have the true geocentric lunar longitude (125°08′ by the Almagest calculation) 180° distant from the true solar longitude. But mid-eclipse accurately calculated (by one of Hipparchos’ computers) from these tables would be nearer 22°. The difference (about −10°) is due to the c.5° tilt of the lunar motion vs the ecliptic in this partial eclipse, a factor that is even more trivial (1° & −2°, resp) for the total −145 and −134 eclipses, where Hipparchos also likely rounded the tabular-predicted Lindos Apparent Times (to 23/23 and 35/25, resp). If Hipparchos’ presumed −140 use of 22° was not just a rounded value but due to accounting for tilt, then he made a tiny slip, since at the tabular mid-eclipse moment (22°) the Moon’s longitude was about 5° short of being opposite the Sun. (If Hipparchos actually used 22°16′ Lindos App. Time: −5° would still be the likely recorded Δλ.)


21 Hipparchos’ −140 Regulus longitude would’ve been listed at 119°23′. Though 8° is something less than 1°/6, the Ancient Star Catalog’s longitudes are almost exclusively expressed in units of 1°/6, so though prevision to the Catalog’s epoch yields 119°45′, this would still end up being listed as 119°5/6, since nothing in the Catalog is expressed in degree-fifths.

22 DIO 8 §1 ⊗11 found that astrolabe lunisolar observations showed ±0°.1 consistency. Ancient Star Catalogs’ mean error is 22 (R.Newton 1977 p.216), but stars brighter than 3rd magnitude used by pre-Hipparchos astronomers in the Almagest (dropping too-south Sco, and 2 quarter-degree ending stars: DIO 2.3 [fn 20], are β Tau, α Gen, β Gem, γ Vir, α Vir, α Lib, β Cap, whose longitudes’ deviations from a zodiacal error-wave (melded from Rawlins 1991H §III-F2) of −9° −13° sin(λ − 96°), show scatter ±0°.1 around an average of +0°.1. [Note added 2017. For −145, −140, −134, +1987 eclipses, JHA’s view insists on longitude errors of, resp, −33°, −35°, +33°, +40°. Removing parallax-mis-signs, & defects in Hipparchos’ PH solar theory: errors in outdoor-observed star-Moon gaps were, resp, −2°, −4°, +1°, +2°. (All 6 ancient results share in common the same trivial error, due to ∆T uncertainty, of ordmag 0.1 for that era.)]
Hipparchos’ Eclipse-Star Data

2009 December

DIO-J.H.A 16 1

–15′ (20′ offset from Regulus’ error). [b] Hipparchos stayed with his –140 Regulus λ fixed by eclipse, even after –144 indication (via Spica) of the method’s unreliability.
E7 Late Ptolemy works’ use of Regulus as a foundation-point suggests that Hipparchos held Regulus as a pivotal star in his astronomy, which could help explain why his –140 measurement of its position was retained inviolate to the end of his career. And Ptolemy’s.

F Evaluating Hipparchos & the Sign-Slip Theory

F1 We conclude that our parallax-sign-error theory has survived the §E2 fruitfulness test: 4 hits for 4 at-bats. But the traditional image of Hipparchos as among the greatest of ancient scholars survives less robustly. (See also Rawlins 1991W §N16&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 1990 shows that it was known to Aristarchos of Samos about 1 1/2 centuries earlier.
F3 But this doesn’t dim our gratitude for his merits, e.g., [a] Grounded in empiricism. [b] Developed nested calendar (Rawlins 2002A fnn 14&17) and durable luni-solar theory. [c] Likely invented the clever “circuli” scheme (§ §1). [d] Determined accurate obliquity. [e] Took accurate solstices [DIO 20 1 Table 3]. [f] Oversaw creation of his ever-remembered Ancient Star Catalog, the oldest extant detailed compendium of the starry heavens. [g] Draconitic ratio good to ordmag 0.1 mile with 1244/11/13 eclipse: www.dio.org/jb13.pdf.

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Pytheas’ Solstice Observation Locates Him: Cape Croisette

Pytheas’ Solstice: Oldest Vertical-Instrument Transit Observation

We conclude that our parallax-sign-error theory has survived the § 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.)

F Evaluating Hipparchos & the Sign-Slip Theory

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

$$s_n/g = {41 \over 120} \approx \tan 19^\circ 12'$$

A Having a Fortuitous Ball

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 MunJHA p.15’s flip sneer at the firmness of Diller-DR’s diamond-clear discovery): we will respond to the offending paper’s mis-adducement of the famous S.Solstice gnomon observation of Pytheas of Marseilles (which alleges it was just a calculated non-observation), by running with the ball fortuitously lobbed our way, recognizing the datum as that of a patently high-precision observation — and thereby locating the Mediterranean spot near Marseilles where this legendary astronomer-navigator-explorer did his astronomy: Cape Croisette (Fig.2), 0°1 south of Marseilles-habor proper (Fig.1).

Without certainty, one presumes Pytheas observed before Timocharis since the latter probably used a transit circle, an advance over the gnomon. Anyway, Timocharis’ star declinations are not raw data.
MuJHA p.17 having claimed that the Summer Solstice datum (eq.1 or 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, Rawlins 1973, and 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 Z₀ 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 ssd/g with such an instrument will produce a latitude L which is 16′ less¹ than the true value. A useful 1st estimate of the uncertainty in Pytheas’ Z₀ 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:

\[
\begin{align*}
\text{ssd}/g &= \frac{41\frac{4}{5}}{120} \pm \tan 19°11′ \quad &\text{&} \quad \text{ssd}/g &= \frac{41\frac{4}{5}}{120} \pm \tan 19°13′ \\
\end{align*}
\]

Thus, crudely:

\[
Z = 19°12′ \pm 1′
\]

²The 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 miniscule diffraction. Arrange that the gnomon’s shadow be cast into a room protected from non-direct sunlight. Use a vertically-oriented rectangular-plate gnomon (see, e.g., R.Newton 1973-4 p.373 Fig.1). Then, between it and the shadow, bring down another vertically-oriented rectangular-plate until it virtually chops off the solar beam cast upon a at-horizontal, carefully ruled shadow-measurer. (Due to diffraction, for 5m-high equipment, the gap between shadow-edge & first intensity maximum is c.1′; but the uncertainty in that edge’s position is smaller: ordmag 1/10.)

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 \( ssd = 15^\circ.8 \) and \( r&\oplus = 0^\circ.3 \), with [for epoch \(-310 \pm 25^\circ\)] obliquity \( 23^\circ.44^\circ.0 \pm 0^\circ.2 \), error from uncertainty of Pytheas’ exact epoch):

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

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 \( 43^\circ.11^\circ.6 \) to \( 43^\circ.13^\circ.0 \) and is drawn in pale lines upon Fig.1.

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

B3 But let us instead pay close attention to some previously neglected points. [a] Pytheas’ clear precision was \( \pm0^\circ.5 \) (eq.4), not \( \pm0^\circ.1 \) (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 Rhodos] for his southern stars.) Central novel realization here: the southern part of the Cape Croisette peninsula is a far better location for an astronomical observatory than Marseilles itself, which (Fig.1) faces westward on the water. And what is Cape Croisette’s location? It is at latitude \( 43^\circ.2 \) N (longitude \( 5^\circ.3 \) 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''), 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''1/7 in modern measure), the high, steep cliffs of Maire (Fig.3) might allow a mostly natural gnomon of such height (which would ensure negligible imprecision from diffraction): the gnomon’s verticality verified by plumb-line with a bob dense enough to minimize wind-influence, and the shadow-surface’s horizontality verified by use of a water-filled hose. A direct exam of Maire’s topography could determine whether this would be feasible.

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

C5 Does that striking coincidence assure us that the Cape Croisette region is where Pytheas made his observations? — including the miraculously extant Summer Solstice s/?. 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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Footnotes:

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

Does Spherical Trigonometry Go Back to the 2nd Century BC?

Unexpected Perfect-Fit 2009 Induction Snuffs

Controversy Refless AlexJones Banzai-Sneers at Three Refereed Discoveries

Strange JHA 2002 Attack on A.Diller Finally Refereed

Late Uncriticized Inept Archonial Chauvinism Disgraces Entire Field

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

A 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’ klimata1 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) \( \epsilon_{12} \) equal to 23°2/3 or 23°40′; eq.2 below.

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

C If we assume \( \epsilon_{12} \) was measured in standard fashion (eq.8, below) and account for refraction&parallax, an ideal Hipparchos determination of \( \epsilon \) would have been 23°42′, and standard ancient rounding was to the nearest 5′, so \( \epsilon_{12} \approx 23°2/3 \) correct to its precision. Even ignoring rounding and r&p, it was (as it stands) accurate to about a 20° of a degree. This long-lost value for the obliquity was probably measured using Hipparchos’ 135 BC Summer Solstice (Almajest 3.1; Rawlins 1991H), but attestation of it had not survived2 so (were Diller) no one had previously suspected that the ancients ever had an accurate obliquity. In short, Diller 1934 simultaneously announced 3 major discoveries: [1] solution of Strabo’s klimata, [2] Hipparchos’ use of spherical trig, [3] his adoption of the only accurate obliquity-measure we can recover from antiquity. That a pack of possessive snobs has nearly obscured such scholarly triumphs for 81 years is itself a triumph of organized truth-warping, providing a history (see p.2 & fn 7) warmly recommended to sociologists of cult-think.

A 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 (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 \( \epsilon_{11} = 23°2/3 = 23°40′ \) (2)

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%

1 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.Draye3 by realizing that the aggravating non-fit is precisely what can be beckoning one on to new discoveries.

2 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.

3 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.1313) provides explicitly in the case of Alexandria, noting that this city is separated from the nearby “Lower Egypt” 14th klima by 400 stades — this, though it was common in antiquity to casually call the 14th klima “Alexandria”. (Strabo 2.5.38 inadvertently does likewise: §§4.)

B3 The case of Meroë is complicated by the fact that there is both an “island” Meroë (described at Strabo 1.25 as the Nile’s largest: §§4) containing, in its north part, the island “Meroë”. (Note: the Don klima was generally placed at c.54° N. Perhaps an ancient slip occurred when its distance to Alexandria city and klima at 21800 stades. [ii] Ignoring all klimata south thereof (this, even while the island “Meroë” we will now eliminate. The very names of klimata illustrate that most were named for latitudes — such as bays, straits, river-mouths, or islands. (In ancient geography, Syene [modern Aswan] is often just a sloppy amalgam of city, tropic, & klima.) This, because few if any important cities were likely (being tiny areas) to oblige by falling smack upon a klima. (This obvious point had become obscured by the time of


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3 See, e.g., Rawlins 1991W §D2 and DIO 4.1 §A5 [p.35]. Also the ancient galactic CygSegment on the Farnese globe: www.dioi.org/fff.htm#gtgm.

4 See DIO 4.1 [§3 fn 2 p.33].

5 This realization is not our 1st restoration correction of the mis-filing of a famous item. See, e.g., DIO 8 §15 fn 5.

6 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.730k-732, though at p.305, mathematician Neugebauer denied himself into thinking his own theory better than non-mathematician Diller’s (an imagined superiority insultingly expressed at p.734 n.14 — but now accepted nowhere) by: [i] Over-ruling Strabo 2.5.38 (and his own Neugebauer 1975 p.336 n.29: see below at §F4) so as to equate Alexandria city and klima at 21800 stades. [ii] Ignoring all klimata south thereof (this, while knowing that his scheme didn’t fit them but the exiled Diller’s did). [iii] Skipping the 15th Pontos klima, where his scheme also failed. At DIO 4.2 p.55 fn 4, the Neugebauer theory (Neugebauer 1975 p.305) was reduced to a cubic polynomial (4 coefficients) \( L = 50[M^3 - 62M^2 + 1307M - 8454] \). (If one tries a polynomial of high enough order, one can mimic any curve of the sort examined here. See www.dioi.org/bv.htm#igp.)

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

8 Contradictory ancient definitions of Syene are touched upon below at eq.17.
Ptolemy’s GD — perhaps as early as Hipparchos. See DIO 5 fn 19 on commerciality.) So it would make sense that the 13th klima was for Meroe Island. (This is made explicit at Pliny 6.220 & Almajest 2.6.) Moreover, we notice that the latitude differences in stades given by Strabo connected to Meroe are generally expressed with respect to other cities. Indeed, since these distances are (§C2) due to Eratosthenes (who probably did not use sph trig klimata) they cannot be klimata-based and their contexts usually do not discuss hours.10 When Strabo finally speaks of the supposed Meroe klima, he does not speak of a spot called Meroe elsewhere) but says (Strabo 2.5.36): “in the regions of the Meroe city and of [Ptolemy’s Hunting Lodge], the longest day [M] has thirteen equinoctial hours”.

B4 And Strabo 17.2.2 estimates the north-south extent of Meroe as about 300 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 Meroe klima (11600 stades) and the city’s measured latitude (11800 stades) which has hitherto been mis-filed among the Hipparchos-Strabo klimata. Conclusion: Meroe at 11800 stades latitude is a city and thus (as noted at §B2) no more belongs in klimata Table 1 than does Alexandria city, which had thus already at the outset been eliminated by everyone but Neugebauer 1975 p.305.

B5 Diller 1934 p.267 realized the difference between the Meroe city & klima but supposed (like DR for decades) that Strabo had neglected to supply the klima’s L. Which brings us to reprising the shocker 1st revealed in DIO 5 (2009). By contrast to all his inter-city placements of Meroe city (fn 10); during his lone reference to the Meroe 13th klima, Strabo 2.5.36 hands us its latitude by stating that it is 1800 stades nearer Alexandria than to the Equator. As DR 1st realized 2009/4/1 (merely 5th before DIO 5’s online publication! — this, after 25y of delay in publishing Diller’s GD 8 ms in that volume, as long planned): since the context11 is klimata (not cities) and since the 14th klima is at 21400 stades (Table 1)

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<td>10200</td>
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<td>17°</td>
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AND12 Strabo 2.5.38), we use this Alexandria klima latitude to solve for the Meroe klima latitude K by simple arithmetic in stades:

\[ K = (21400 - K) = 1800 \implies K = (21400 + 1800)/2 = 11600 \tag{3} \]

— precisely the Meroe latitude predicted at Diller 1934 p.267, over 3/4 of a century of Muffia sneering after. Result: ALL FOURTEEN data fit the Diller-DR scheme. This is evident from our depictions of Diller’s triumph in Tables 1&2 and Fig.1: an astonishingly flawlose record of, again, fourteen successive hits out of fourteen data. Has any comparable ancient astronomy discovery ever13 enjoyed such perfect verification?

B6 Muffia 2002-2009 reaction? Strabo’s klimata data suddenly aren’t trustworthy any-more!14 As posted by DR (www.dioi.org/cot.htm#dmfe) a few days earlier in anticipation subject is so thoroughly klimata that even key supposed non-klimata entities turn out to be [a] twisted or [b] mis-taken versions of klimata after all. (Respective restorations: [a] §F & [b] eq.3.)

10 Examples for Meroe 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 demonstrous 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 Mufa: Is 13-out-of-13 Enuffa?” See DIO 5 fn 22 and in-love-Osgood Gingerich at DIO 11.3.36 §A1 on the Mufa’s decades-long tolerance of all manner of imperfection in Ptolemy&Neugebauer, even while (the source being of non-Mufa breeding) blind to a now-literally perfect fit. (Thereby inverse-fattidiously outnuttting Some Like It Hot’s original indefatigable old master Osgood, even while Diller-DR provides an exception to his Nobody’s-Perfect capper: idem.) As observed in *ibid* (e.g., fn 12): a cult which systematically, pseudo-effetely labels&treats others as cranks (www.dioi.org/cot.htm#slst), while transforming journals & conferences into 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°1/8; or, using eq.1 and rounding as usual to the nearest 100 stades: 5000 stades.

C2 So the ancient finding of the equality of Alex-to-Syene and Syene-to-Meroë turns out to be impressively true: to ±1°. (And it is less likely to be based upon accident than the equally remarkable ancient record [also correct to ±1°] that Aldebaran and Antares were 180° apart in celestial longitude: DIO 2.1.2 fn 5.) The basis of this geographical discovery is the likely careful measurement of the city latitudes around |-12°|; since then §B3 are largely accurate to ordmg 0°.1. Rawlins 1982G shows that Eratosthenes had learned that Rhodes City’s L = 36°5/12 (good to 1°) — or 25500 stades — and that only his foolish use of gnomon for Summer (not Winter) Solstice threw off his measure of Alexandria’s L by half the solar semi-diameter, yielding 31°4/0 (Rawlins 1982C eq.10, Rawlins 1994L fn 44). His place for Alexandria was, like Meroë’s, adopted (Strabo 2.5.7) by Hipparchos (who never visited Africa) and typically rounded to 31°1/12 & 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.17 Strabo’s report of gnomon-use at first looks discouraging due to its systematic error from solar semi-diameter ssd. However, while outside the tropics, ssd will foul up the L half of eq.8 instead of the ε half, the reverse is true in the tropics. A transit instrument would

16Even the rough latitude for Athens (Strabo 2.1.2), 38°+ , is much better than Hipparchos’ later false value of c.37° (Hipparchos Comm 1.11.3&11), which became adopted in astrology manuals for centuries after, e.g., GD 3.15.22 & (see DIO 5) 8.12.18. (For speculative explanation of his error: www.dioi.org/ffl.htm#rbv.) This relates to DR’s contention (www.dioi.org/ffl.htm#bpp) that most astronomers (as against astrologers) knew Athens’ actual latitude, and that this may relate to the origin of the Farnean globe: of indicated home latitude 38° (presumably either Pergamon or Athens).

17Rawlins 1994L fn 44. Strabo 2.5.39 confirms this by putting Hipparchos’ Alexandria 3640 stades south of the Rhodes 14°1/2 klima, thus at latitude 21760 stades or 31°11/12. Further if less precise confirmation: Strabo 2.35.38 says the transit of Arcturus is a little south of the zenith, consistent with the star’s quite erroneous 31° Hipparchan declination (Almajest 7.3). We have elsewhere proposed that since culminating Arcturus was actually c.0°1/3 north at Alexandria in Hipparchos’ era, he (again: §1) made a sign or translation error and subtracted c.0°1 from his 31°11/12 Eratosthenian Alexandria latitude to find the awful figure 31° (error -0°3) for the declination of Arcturus.

18The actual interval would have been 46d. But we find that his figure is accurate to its precision, if we inquire as to how Philo determined the time of Summer Solstice: he would use equal-altitudes, so why not choose the two zeniths’ dates, for L = 16°57’ (assuming epoch c.270BC, though there is little time-sensitivity here) just under 91° apart? Philo would then find the S.Solst 1/2 way between those two dates and report the semi-arc as half of 91°—or: 45°.

get the correct L, but even if we assume that Philo used a gnomon at both solstices, he would have found (accounting for both rdp and ssd) zenith distances Z_W = 40°24’— and Z_S = 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-threees: 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 (Eratosthenes&Hipparchos-adopted), that Syene is exactly 1/2-way between Alexandria & Meroë, the Hipparchan value 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:

<table>
<thead>
<tr>
<th>Eqn</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>(4)</td>
<td>cos(15M/2) = - tan L tan ε</td>
</tr>
</tbody>
</table>

(Almajest 2.3) where obliquity ε was usually taken to be that of Eratosthenes-Ptolemy (eq.5) or nearby 23° for Ptolemy’s or Hipparchos’ two values (eqs &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 (Almajest 1.12)

\[ \epsilon_E \approx 23°51’20” = 180° \cdot 11/83 \]

or the early Hipparchos obliquity

\[ \epsilon_H = 23°55’ \]

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

[a] The major cities correlated with Eratosthenes’ eq.5 or 23°56/6 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.

19Possibly 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°2/3 discord between his & Hipparchos’ L, hinting that Hipparchos was not responsible for the GD’s klima-polluted L mis-geography.

20If Philo travelled to Meroë, he must have visited Syene. So he presumably knew that its latitude was 24°05’. And every scientific but Eratosthenes (§C2, Rawlins 1982G, Rawlins 1994L Table 5) then knew that Alexandria’s L was nearly 31°1/5’t of the zenith’s S-M symmetry was not only true but completely known to be true in Alexandria’s community of genuine scientists, which again excludes Eratosthenes. His & Hipparchos’ later symmetrical A-S-M schemes were (as just noted) slightly less accurate than the presumed original latitudes (of, e.g., Philo) but were perhaps nudged to ensure adherence to an A-S-M symmetry likely well-known long before either’s geographical scheme.

21Doubling 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 favorable 16°11/12 (& thus Philo’s accuracy) as Eratosthenes’ & Hipparchos’ Meroë, L rather than 16°5/6. Either satisfies 11800 stades.
Diller Vindicated: Early Sph Trig

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D4  Since §D3’s correlation [a] was found via the sph trig of eq.4, we have here (also Rawlins 1982N n.11) a shaky suggestion that (contra §E1) sph trig was known in the 3rd century BC. Indeed, there is even a hint (§2 fn 32) that trig may go back to c.300 BC. Trig’s absence from surviving mathematical texts (e.g., Rawlins 2008Q fn 32) has been taken to indicate its late appearance; but another possible explanation is that this 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’s.)

D5  If known to Eratosthenes, the simple double-sunset Earth-measure method (requiring sph trig) would have faced him with the large disagreement between the lighthouse method’s 256000-stades (likely known before him: Rawlins 1982N p.215 & Rawlins 2008Q §I) vs the sunset method’s 180000-stades. (The latter being the Poseidonios-Marinus-Ptolemy value which eventually became dominant. Conversion discussed in Rawlins 2008Q & Rawlins 2008S.) Did he face the disjunct? (See detailed discussion at *DIO* 5 fn 18).

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

E1  While such speculations provide no proof that sph trig was known to Eratosthenes, Table 1 proves positively that sph trig (eq.4) was known to Hipparchos, as *Diller 1934 was 1st to prove*. (A powerful array of the evidences for sph trig’s use in Hipparchos’ century is brought together for the 1st time at www.dioj.org/cot.htm#mmmsz.)

E2  But, at a time when the hist.astron eld is run by “just a bunch of politicians” (as an asthag eminent astronomer describes the field’s debate-fooling 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.52-33.) It would be merely pathetically funny if it weren’t so damaging to the balance of communal micro judgement here and macro understanding of the entire subject of ancient astronomy.

E3  Continuing obtuseness ([E1 item c]), defining Diller 1934’s multiple [now SEVENFOLD] 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 decei23 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-If Eratosthenes and-or his critics tried both the lighthouse and sunset Earth-measure methods via the Pharos, the azimuths would be different since land beyond the point 202 stades away (where the Pharos flame became invisible: Rawlins 2008Q) would render clean settings of the Sun’s disk impossible, so viewing sunsets from the Pharos would be at more northerly azimuths. Strabo 2.2.2 is chronologically valuable in its implicit suggestion (perhaps contra the nonetheless intriguing and original analysis of Taisbak 1974) that Poseidonios was indeed the 1st prominent adopter of the much smaller circumference 180000-stades cited to him at *Strabo loc.cit*.

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

23-The closeness of Diller’s fit is definitely known to the *Muffia-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 23°2/3 obliquity have been the *ArchiveHistExactSci* (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 Muffia 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-outrighted-power-genius archons’ vindictive exilings?26 May bigots’ turf-possessiveness be succinctly summed up as: Pigotry? E5 Readers can examine this matter (bluntly condensed at www.dioi.org/col.htm#dpd) carefully for themselves and then opt for which view to go with, on Diller’s grand discovery: Occam’s Razor? Or Mufa theology?27 I have friends who claim (2009) the numbers of Hipparchos (who was glorious or notorious for varying his parameters through­biedom) a whole community, merely by inadvertently manipulating that community’s lethal out his career:

Prong [a]: At p.16 mistaking a calculation for an observation. Reverse at p.17 (F here).

Prong [b]: More such confusion at MuJHA n.9. (§1 below).

E7 Putative JHA for MuJHA missed the stark contradiction between §6’s prongs [a] & [b] (such embarrasments inevitably issued from chattel­infamous-Sea determinations tryanyhingitis): the $e = 23.5 \textdegree 21' 20''$ (eq.5) MuJHA pp.16-17 proposes in §1’s prong [a], is inconsistent with that which would be produced by prong [b]’s proposed data-set-alteration. (Awful numbers & data at §6 b & §1.) We now examine the §6 [a] & [b] prongs of Jones’ indiscriminate creativity (which he’s very fortunately connected in DIO work as well), following 11.2’s cover), a classic Muffa vision of ancients as semi-empirical number-jugglers (Neugebauer 1975 pp.642-644; DIO 1.1 §1 fn 24), contra Almajest 1.12’s description of e’s capture being via the outdoor transit circle presumed in Rawlins 1982G.

F Jones’ Subtraction from the Sum of Human Knowledge

F1 MuJHA’s initial knowledge-subtractive (fn 55) attempt to undercut Diller 1934 was above-cited prong [a] (§6). Diller had been 1st to discover that Hipparchos’ ultimately adopted obliquity was the lost value 23°2/3, which is accurate (much better than the values MuJHA urges for H, implicitly&explicitly) and has since been validated in various ways (summarized at DIO 5 §3D). MuJHA pp.15&17 notes that Ptolemy ascribes to Hipparchos the Eratosthenes value $e = 23.5 \textdegree 21' 20''$ (eq.5). As §5 notes, this is of little weight since we know (see the MuJHA author’s own Hipparchos entry in the Encyclopedia of Astronomy and Astrophysics 2000) that Hipparchos repeatedly changed parameters.

F2 Jones 2002E p.16 rigorously rounds $L$ from arctan($35° = 30° 58'$ to a precision of a few arcmin. Again, Jones 2002E p.16.1 rigorously rounds $L$ from arctan($35° = 30° 58'$ 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) $L = 2170$ whole 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 2170 degrees: i.e., no confirmation. When he subtracts the traditional Alexandria—Syene 5000 stades $31° - 5000$ stades/(700 stades/degree) $\approx 23° 51' 26''$. (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 un solving (Rawlins 1982G) which simultaneously solves precisely for three Eratosthenesian data: [1] $e = 23° 51'$. [2] $L = 31° 04'$ (not 31°), & [3] $Z = 7°.5$. Jones 2002E pp.15-16 basely says Strabo’s supposed (vs §4F) Alexandria “equinox-shadow-ratio” 3.5 & its image $L = 30° 58'$ were Eratosthenes’. A key misimpression ($\frac{L}{F}$): $L = 31°$ is said (p.16) to be “derived from the equinoctial shadow” though standard ancient measure of L got it via solstice29 (not equinox) data. The method is attractively simple (Almajest 1.12): just halve the sum of S.Solst & W.Solst app.noon zenith distances $Z$. But the process automatically produces the obliquity $e$, if one just halves the very same two $Z$’s difference. See eq.8. Therefore, unless an ancient astronomer deemed subtraction more challenging than addition, he would find $e$ as part of finding $L$, so (don’t miss Jones 2002E p.15’s curious phrase “might suggest”): why compute already-known (via eq.8) obliquity e through the more laborious long-division required (above: eq.7) by the MuJHA p.16 method?

26 Hipparchos may’ve used eq.5 at some point in his long career. But not when computing Table 1.

27 Solstices are used to find $L$ because measuring equinoctial $s_e/g$ is vitiated by non-parallelism 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.Newton 1977 pp.81f.

30 Whatever may be the merit of MuJHA’s try (our eq.7) at relating $e$, 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 rounding, i.e., because the $s_e$ 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 1985PG pp.263-264 & n.17.)

Alexandria $L = \arctan \frac{s_e}{g} = \arctan \frac{3}{5} = 30^\circ 58' \approx 21700$ stades

Carthage $L = \arctan \frac{s_e}{g} = \arctan \frac{7}{11} = 32^\circ 28' \approx 22700$ stades

Eq.11’s 100 stade difference vs Strabo’s 21800 stades for Alexandria (Neugebauer 1975 p.1313) is one of the three bases for Jones 2002E n.9’s proposed 100 stade shift of all the klimata. But such a shift would maintain Strabo’s L-differences, yet the difference between eqs.11&12 is 1000 stades, contradicting the difference at Strabo 2.1.12 (eq.9). (I.e., why does Jones 2002E use the Alexandria discrepancy between eq.11 & Strabo’s L, while ignoring the corresponding Carthage non-discrepacy?) Carthage’s 7:11 ratio is obviously non-empirical, since 32°1/2 is way too far south of actual Carthage, fatefully distorting maps of the N.Africa coastline for the next millennium. And the explanation for this ancient disaster is the very same as for Alexander’s *true* Strabo ms reading, namely 7:5. (Not 3:5, as MuJHA n.10 scrupulously notes.) Thus it is not the $g/s_e$ ratio but is the longest/shortest-day ratio $M/m$ for the Alexandria klima where $M = 14^\circ$ — just as the 7:11 ratio for Carthage is not the $s_e/g$ ratio but the $m/M$ ratio for the $M = 14^\circ$ klima around actual Carthage ($L = 36^\circ 51'N$, not 32°1/2 which is the arctan of $s_e/g = 7/11$: eq.12), as 1st 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 (MuJHA’s native area) entirely. But then: if we are reduced to Marseilles (Pytheas’ native city: $g$, wouldn’t MuJHA’s p.17 sph trig be Pytheas’ calculation? In c.300 BC?!) (Note: the later Almajest 2.6’s calculated Solst $s_e/g$ for Marseilles does not equal the Pytheas’ $s_e/g$ in 38! MuJHA’s author isn’t really invoking a Solst calculation, which actually HIPHARCHOS claimed he measured [Strabo 1.4]), even when He knows [Motivated by tradition or cataloguing priorities] to name the 15°1/4 klima. (Did he treat Pytheas’ $s_e/g$ as a valued heritage [perhaps famous for its obvious precision] from the earliest days of observational transit-astromony? — 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 13°1/4, so for purposes of testing the reality of eq.10 (& 12 eq.5), we may ignore Byzantine (MuJHA’s native area) entirely. But then: if we are reduced to Marseilles (Pytheas’ native city: $g$, wouldn’t MuJHA’s p.17 sph trig be Pytheas’ calculation? In c.300 BC?!) (Note: the later Almajest 2.6’s calculated Solst $s_e/g$ for Marseilles does not equal the Pytheas’ $s_e/g$ in 38! MuJHA’s author isn’t really invoking a Solst calculation, which actually HIPHARCHOS claimed he measured [Strabo 1.4]), even when He knows [Motivated by tradition or cataloguing priorities] to name the 15°1/4 klima. (Did he treat Pytheas’ $s_e/g$ as a valued heritage [perhaps famous for its obvious precision] from the earliest days of observational transit-astromony? — 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 13°1/4 klima (eq.17). HIPHARCHOS similarly used (fn 55) the proximity of $\alpha$ UMÎ’s NPD (not its exact value) to indicate the position of the 12°3/4 Cinnammon klima. (Are we to suppose that HIPHARCHOS 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) astronaut HIPHARCHOS just indoor-computed (by eq.4) the Z of such major stars as $\alpha$ UMÎ (2nd magnitude present-day Polors)?)

1. 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 $s_e/g$ to $Z$, as in eq.10?

2. If HIPHARCHOS 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 $s_e/g$ to $Z$, as in eq.10?

3. Question: why do classicists persist (as in the LCL version of this passage) in translating the Greek for ancient scientists’ ouden (“gnomon”) as “index”, when it is important that modern scientists analysing ancients’ work understand what instrument was being used?

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 11.4 p.26 n.1 & www.dioi.org/cot.htm#ucmcf. 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 slapdash (see www.dioi.org/eqpp.htm#hhhcw)! 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 article which alone destroys a journal’s claim to having a reliable/keen refereeing process even when He knows [DIO 6 13 §11 they are competent], one cannot be surprised when misunderstandings pass into print unapprehended. It is also disappointing to find an attack on DR in a journal from which appreciative citation of his work has been strictly barred for many years, even while DIO’s fair-debate doors are always open: www.dioi.org/deb.htm. This, again, is the kind of improperly-scientifically anticipated destruction of communication irresistibly created by fawning on semi-numerate archons who assign for their own inductive sterility by such creativity as shumming.

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33 Question: why do classicists persist (as in the LCL version of this passage) in translating the Greek for ancient scientists’ ouden (“gnomon”) as “index”, when it is important that modern scientists analysing ancients’ work understand what instrument was being used?

34 Strabo 1.4.4 & (2.1.12), 2.5.8&41. Some of the Strabo report has HIPHARCHOS placing Massalia and Byzantium on the same 15°1/4 klima. While Massalia is close to the implied 43° 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, j) just ordm 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 **Mansaliar native**, obviously enhancing odds that eq.10 is a real 1-hand observation, and that this observation was **off repeated to get it just right**. So there is no reason to follow MuJHA’s p.17 rejection of Diller’s unquestionably-calculated 17 fourteen perfect fits to eq.4, just on the basis of MuJHA’s inextr speculation38 that a reality-accordant reading (ss, in eq.10) was actually non-real and thus also calculated. The MuJHA roundness arguments are curiously perverse. Highly rounded ss/g ratios (Alexandria & Carthage) are obviously not directly empirical (and in these cases aren’t even ss/g ! : §F4), because in the real world, an outdoor eq.9 measurement of ss/g will probably be as unround as eq.10 (Massalia). Summing up: MuJHA is simultaneously taking equinclional ss/g as directly empirical and solstitial ss/g as non-empirical when (§F3 & eq.9) the reverse is true. So by 2 independent criteria, MuJHA’s 3 assessments of the Strabo s/g data’s reality are all inverted. G2 Return to MuJHA p.17’s fundamental Pytheas-Massalia theory, we see that Jones’ argument is two-step: Massalia L is computed via eq.4 using M = 15°1/4 and the Eratosthenes obliquity (eq.5) which MuJHA is proposing for Hipparchos. Massalia L = arctan[cos(7° + 15° 4/5) / 23°51’20”] = 43°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 ss/g of eq.10 but doesn’t:

\[ ss/g \approx \tan[(41°3/4)° - 23°51’20”] \approx 41.713/120 \neq (41 4/5)/120 \] (14)

a failure which leads (§J5) to Jones 2002E’s p.17 plea — not necessary for ANY of Diller-DR’s FOURTEEN hits (Table 2) — that we tolerate Slight-Miscalculation (eq.5) in the CENTRAL attempt 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 1000s of years. Some gauge ancients’ solar-data accuracy by reference to their star observations. But the Sun is immensely brighter & easier to place. The S.Solstices of Kalippus (–329/628 1/4) & Hipparchus ( –134/626 1/4) were both accurate within 1°4 rounding precision (see Archimedes at Almajest 3.1), but such success requires 1 accuracy since it is done by equal altitudes. (Raw human ocular accuracy is to c.1’.3: Rawlins 1985G. As ancient scientists were aware: DIO 14 [2] all 1 I.e., with math not reality, obviously the normal situation for klima: In 34.

37Based without justification upon claimed (§F4) significance of the eq.10 ratio’s precision. But how would Pytheas calculate Zs (thereby giving him his ss/g via tangent) by subtracting obliquity from L (MuJHA p.17 middle equation), when he didn’t know e without using eq.8, which requires one already to have determined Zs, the equivalent (via arctan) of the very item supposedly being sought (ss/g)!

38So MuJHA has to speculate that 41 4/5 is Hipparchos’ calculation, via eq.4 using eq.5’s obliquity. Yet Strabo (1.4.5 & 2.5.4) ascribes 41 4/5 to Pytheas, not Hipparchos. MuJHA proceeds according to his beliefs that 41 4/5 is a klima calculation (for which there is no evidence) and that the klima’s L = 43°01’ — though we multiply demonstrate elsewhere (§H) here that the L upon which MuJHA bases his argument is actually L = 43°04’, which undercuts his whole case for eq.5’s involvement in the origin of Pytheas’ 41 4/5. (See, e.g., eq.16.) Note that, if using Eratosthenes’ obliquity (eq.5) for M = 15°1/4, MuJHA’s hypothetical calculator would have (via eq.4) gotten not 41 4/5 but the Almajest 2.6 value, 41 4/5. But MuJHA p.17 realizes this: so MuJHA then holds Hipparchos ispy an assumed, thus-contradicted (and Strabo 1.4.4-contradicted) theory that 41 4/5 was calculated, and accept instead that Diller-DR’s theory fits better and lots more often? No, he concludes (MuJHA p.17) that his own hypothesized (central-to-countering-Diller) “calculator” must’ve screwed up. A stark example of the effect of cultism upon judgement. (Since there is no evidence for eq.4’s existence in Pytheas’ day, the MuJHA-hypothesized calculation must be alleged to have occurred far later, which much diminishes any excuse for imprecision.) Notably, Jones 2002E convinced not one among even his friends on the committee for the $1000 DIO van der Waarden Award.

G3 MuJHA p.17: “I believe we have to regard the shadow ratio [(41 4/5)/120] as the more trustworthy datum” backed up by “the closeness [!] of the agreement between text and re-computation for ε = 23°51’20” — this, though Diller-DR’s 14 fits (Tables 1&2) are all within their precision (100000) while MuJHA’s foundation datum doesn’t fit within its (standard ancient fractional representations of s: 2/3, 3/4, 4/5, 5/6, etc). Moreover, even if we accept this dubious claim, that would just mean that both the fourteen data and the lone datum were calculated; so why (except to satisfy cult-straitjacket theology) rank a SINGLE mysteriously-presumed (§F4) and admittedly botched (eq.14; §G2&J5) calculation as superior to FOURTEEN Strabo klimata (several redundantly) established which perfectly satisfy (14-for-14) the whole problem at hand, as Table 2 illustrates.

G4 Yet, Jones astonishingly deems his nonfitting “shadow ratio” (eqs.13&14, doubly-displacing eq.10 as eq.5-based and calculated) to be “the more trustworthy datum”. Instead of citing DIO’s 1994 table and display-comparing his own, Jones deliberately omits to provide readers either, since such would instantly reveal the vacuity of his paper.

H New Implications of Marseilles Latitude 43°04’

H1 We now produce new, independent, & fruitful evidence for eq.10’s 41 4/5 being accepted 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°1/4 klima, via sph trigg, having no mathematical relation40 whatever to empirical eq.10.

H2 We next reveal that Massalia’s Almajest 2.6 latitude L = 43°04’ was in truth elevated by an’? with Prometheus (Eratosthenes’?) calculation of standard transit-reduction, using39 eqs.5&10, which produces a perfect hit upon this (previously unexplained?) latitude:

\[ \text{arctan}[(41 4/5)/120] + 23°51’20” = 43°04’ \] (15)

H3 The fact that 43°04’ is the correct reading is confirmed by the GD latitudes for Marseilles (GD 2.10.8) and Byzantion (GD 3.11.5); both indisputably 43°1/2, which (in a work whose degree-angles are all Hipparchally 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 MuJHA-upsetting DR-related achieve ment is similar to Schaefer’s blinded attraction (Schaefer 2001) to depending upon the least reliable test (low altitude atmospheric extinction) of all those available for determining the authorship of the Ancient Star Catalog. (Schaefer 2001 was also published in the DR-banishing JHA.)

40 See the vertical arrows of Neugebauer 1975 p.1313 Fig.291, each of which is (except the Equator-to-Merow 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).

41 Our thanks to Toomer 1984 p.86 n.43 for fairly and helpfully pointing out the original’s δ (the Greek math symbol for 4) even while arguing against it in favor of α (Greek math for 1), since the latter explains the Almajest 2.6 shadow ratios but only if one rounds to the nearest 1/12. (This step also fails at fn 56.) That is, both L = 43°01’ and the eq.5 obliquity must be so rounded: to 43°1/2 & 23°5/6, resp, before the Almajest 2.6 shadow data can be recovered. The original 43°04’ is properly maintained in the Almajest editions of Heiberg, Maass, & Taliaferro.

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

43 Neugebauer 1975 p.336 rightly backs Honigmann in preferring the 5:7 Alexandria ratio. (The original ms’ ratio, not the Vitruvius-Ptolemy 3:5 ratio later substituted. See LCL’s Strabo 1.510, & Rawlins 1985G p.263&266 on GD Pharos’ L vs Alexandria’s.) He (ideom) used round e = 24° (not 5°) to develop Pytheas’ L, thereby missing our eq.15 & getting accurate L = 43°12” only by chance cancellation of 16 errors (e & ssd). (Note: Almajest 2.6’s three ss/g are consistent with L = 43°01’.)
I Inconsistencies’ Inconsistencies & Hipparchos’ Circuli

1. Thus, MuJHA p.17’s attempt to connect Hipparchos to 23°31’20” fails both because eq.15 could as easily be (say) Eratosthenes’ as Hipparchos’ and because MuJHA’s eq.13 relation of \(L \& \epsilon\) now (revised here to accord with mss-based eq.15) leads to obliquity:

\[
\epsilon = \arctan \left( \frac{\cos[\left(15^{\circ}/4\right)/(15^{\circ}/15)]/2}{\tan 43^{\circ}04'} \right) = 23^{\circ}49’25”
\]

(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-straining work — instead of recognizing the merit of a coherent solution to an inter-related (and uniformly uninitiated: [§2] data-pool, such as the Hipparchos-Strabo klimata.) From prong [b] (§E6): in trying to weaken the Diller achievement, MuJHA states (p.17 [bracket added])

A.Diller and D.Rawlins have derived a value for the obliquity, 23°40’, that yields a close fit to Strabo’s stade figures (which are expressed in round hundreds of stades, thus to a precision of 1°/7). Unfortunately [?], there are some inconsistencies\(^{44}\) 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\(^{45}\) have introduced systematic errors which would affect the value of the obliquity best fitting the data.

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

\(^{44}\) The Strabo Hipparchos klimata data are given mostly as intervals rather than as absolute values, which is why Diller 1934 refers to them as garbled. And there’s been some very obvious reconstruction (to my knowledge) of a large number of Strabo’s stades of school (see Evans 1987 & 1991W). MuJHA 1984 p.1313’s valuable & crystal-clear Fig.291 has succeeded in establishing these klimata beyond any reasonable doubt (outside Mero¯e: fn 40). It is thus retrograde scholarship (fn 47&55) to try treading down one of the grander cumulative achievements of classicism.

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

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

\(^{47}\) Jones’ 1900-stade-shift proposal suspiciously — and invalidly — mimicks (uncalled) Rawlins 1985G’s valid restoration of the “circuli” of Pliny 6.39.211-218; the key distinction: while Evans 1987 & Jones 2002E (for huge \(L\text{-a-JHA political advancement}\) replaced order with chaos (fn 55), DR’s circuli-restoration did the reverse. (In other cases, e.g., the DIO 9.7 [3 continued-fraction decipherment of ancient yearlength mss.) The M-K-L pairs found in Pliny were not consistent (fn 50) via eq.4 for any Hipparchos \(\epsilon\). As shown at Rawlins 1985G p.263, an ancient dabbler had noted that the original scheme didn’t give \(M = 12^\circ\) for the Equator (\(L = 0^\circ\)); 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.

34        3

fourteenth that Diller-DR solves all fourteen of. If we try the Eratosthenes obliquity (eq.5) of Jones 2002E’s [a] attack and compute via eq.4, the results disagree with about 60% of §11 prong [b]’s proposed 100 stade-shifted klimata data-set. These unbelievable items provide independent validation of the untampered original data-set of Table 1, upon which Diller-DR’s solution is founded. Conversely, if we hold at $e = 23°51’20”$ & $\alpha$ = 100 stades (not Jones’ 100): impossibly far from the unstrained best fit we are about to locate (eq.18) in $\alpha$-space. But $A = 158$ stades would anciently round to $A = 200$ stades, which fails for 5 out of 13 matches. Probability $P$ (eq.19): $10^{-4}$ for 158 stades; ordmag $10^2$ for 200 stades.

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

J1 But these are tipping odds compared to those against adopting Jones’ 2 prongs simultaneously (§13): $e = 23°51’20”$ (prong [a]) & $A = 100$ stades (prong [b]). For this remote position in $\alpha$-space, $P < 10^{-11}$ (eq.19), i.e., odds of tens of trillions-to-1 against.

J2 But even were Strabo’s data infected by the Jones shift, the truth would be recoverable: [1] the mis-shift could be detected by least-squares analysis (§13) and corrected-for; [2] the L-vs-M curve would still (see Fig.1 & caption) show a suspiciously slighted to trace the sort of curve produced by sph trig with a Hipparchan & accurate obliquity. Which vindicates Diller, though this important point is (ungenerously: www.dioi.org/biv.htm#ncmf) left unstated by JHA; so how is MuJHA a refutation of Diller’s essential discovery50 of Hipparchan-era sph trig? That Diller has made this discovery is known to JHA, Jones, & the Muffia. But all have chosen to leave it publicly unexpressed in explicit terms. (See comment [2] at §11.) A near-century of collective shame is just too awful to openly confess. J3 We next carry the previous discussion to its logical conclusion. It is obvious (§12) from the shape of the Hipparchos-Strabo data’s L-vs-M curve that it was generated from sph trig calculations. Jones agrees that sph trig was known to Hipparchos and (n.7) dumps Neugebauer’s fallacy because it (unlike sph trig) “failed to show how Hipparchus could have found such a sequence matching so accurately the theoretically correct latitudes”. (Which Diller 1934 had done, heretofore to Muffia’s arbitrary non-please.) We run a least-squares fit (no roundings) of the function, $\arctan[cos(15.5/2)]/tan \epsilon + A$, upon the M&L data of Table 1, to check the fit of eq.4 simultaneously with Jones’ 100-stades-shift proposal, thus treating obliquity $\epsilon$ (eq.2) AND Jones fudge-factor $A$ as unknowns. Formal results:

$$e = 23.5’6’6’’ + 0.3’2’’$$

and $A = -28.44$ stades (18)

Jones 2002E n.49 haggle-adduces disparate Hipparchan data (having nothing to do with Table 1’s coherent data-set) to up with his $A = 100$ stades (which is $e’9$). But the foregoing best-solution equation shows that Jones’ +100 stades is statistically ruled out, since his $A$ (like even the most helpful $\epsilon$) is adjusted for it is several standard deviations distant from the $A$ & $\epsilon$ (eq.18) which minimize the residual-sum, with probability $P = 1/70$. I.e., we can find A mathematically.52 (A non-fictional JHA referee would have known that and tested for $A$.) By contrast, Diller’s solution ($e = 23°2’3’’$ & $A = 0$) easily falls within 1 standard deviation (sd) for both variables. (Probability $P$ exceeds 2/3.) I.e., Diller is again vindicated. Doubly. On the nose.51 But who will be the 1st Muffioso — after over 80° of bigotry, ungenerosity, & even viciousness52 — to own up to this?

J4 Under the 2-dimensional elliptical-cross-section Gaussian surface representing the probability density $pd$ of any point on the $\epsilon$-$\alpha$ plane, probability $P$ is the integrated volume exterior to the locus of points whose $pd$ equals that of the point in question:

$$P = e^{-\frac{d^2}{2\pi^2}}$$

where $S = \text{square-residuals sum there}$; $Sm = \text{best-fit}$; $S$ = single-datum standard deviation; sums’ relative difference $D = (S - Sm)/(Sm)$; $S = Sm - 1$; $N = \text{no. of data}$; $F = \text{degrees of freedom}$ ($= N \text{ minus the number of unknowns}$, that being 2 in this case). For the Princettitude 4-dimensional case (fn 7): $P = (1 + F D/2) e^{-F D/2} = 10^{-11.8}$. J5 A general observation: the MuJHA paper (which never remotely 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 §33 of Diller’s $e$ and data-fit.

data gives, the mean is $24°07’5’’$, disagreeing with Hipparchan $e$. But, after shift-restoring (Rawlins 1985G p.263) the $M$ by $\pm 1’’$ or $\pm 4’’$ (an amount explained at fn 47), we find the corresponding mean for the reconstructed $33°37’2’’$, statistically consistent with the now-thoroughly-established (fn 46) 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 $e$ 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°12’, the traditional Rhodos klima $M$. The DR reconstruction also allows us to recover (Rawlins 1985G p.263) the circoli’s origin: using eqs.2&4, we find for Pliny’s Alexandria klima ($M = 13°56’’$) tan $L = 34°17’’$; and for his $Rhodos$ $L = 440’’$. The $L = 440’’$ (Continued from the anciently normal formula of anciently normal ratios of fractions’ sexagesimal expressions) 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 circoli’s original linear equation (tan $L = [30 M – 358’/105]$) arose historically when an ancient mathematician just drew (on a graph of $M$-vs-$\tan L$) a straight line through the two points representing the key ancient klimata of Alexandria & Rhodos. 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 undeniably precise sph trig signal.

50 The Diller-DR solution is superior even to the best-fit solution (eq.18), which fails for one klima: the $L$ for 14°14’ is a non-match. By contrast: though the Diller solution ($e = 23°3’2’’$ & $A = 0$) produces 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).

NB: BOTH of Table 2’s rounding-steps are anciently normal thus non-arbitrary: 5’ & 100’ stades.

51 Check out Neugebauer 1975 p.734 n.14. Pure Muffiosity. And by now merely an especially precious larf-reminder of the reliability of establishment exilings of ideas and persons. The former academic crime is long-term worse than the latter; but, following exile-decree, a shunned scholar’s fertil-

52 Some sources are cited (n.11), though key evidence discussed here is not. Arguments of Diller 1934 & Rawlins 1982E for Hipparchos’ sph trig & $e = 23°3’2’’$ are cited at MuJHA n.8, but there’s no mention of 23°2’3’’ & no official confirmation by Nadal & Brunet 1968, 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
Since Hipparchos changed (§Ff) 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 coherent55 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 alibi: “tiny errors in [H’s] calculation... might result from [trig] imprecisions”. Wouldn’t reality & the Diller-DIO Table 1 asked for such no such question? If the latter, then perfect H-trig-calculation fits of eqs.4&2 to Table 1? Does Occam’s Razor mean anything anymore? (Further at MuJHA p.17: for ς = 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 which Diller’s matches was published at DIO 4.2 p.56 Table 1 (a table in which 15° of determined, evidently-unanimous Mufia opposition has found no errors), and since the 2002 MuJHA paper’s timing suggests that it was concocted specifically to counter omertà-breaking 2002 Hist.Sci.Soc citation (fn 23) of said table, it is inexcusable that MuJHA did not cite56 the ultra-tight-fit new table, or at the very least: the info that D’R’s Rawlins 1982C (MuJHA n.8) while floating the undetalled Pliny-circuit confirmation of 23°2/3 at Rawlins 1982C p.368 (ignoring its eq.28’s extra evidence for 23°2/3), & [b] scoff at Pliny’s precise circuli as “crude” (MuJHA n.11, thereby sneering at Longitude Zero-refered Rawlins 1985G), then competent JHA refereeing 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°2/3. Cornered again (as at §1), Authoritative-in-His-Own-Mind Jones can only efficaciously describe as “time fit by decreeing as timing” the 21°2/3 ruling, 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 crankrank since his Winter Equinox (Rawlins 1991W §B4).57

No one finds Jones’ 8700 stades. But neither Hipparchos nor Strabo did so. MuJHA claims that a star’s 8700 doesn’t equal the Cinnamon klima’s 8800 (Strabo 2.5.7&35). (He thinks Hipparchos believed each klima had a bright star’s NPD sitting right on it!?) This is what co-operators MuJHA n.9 to urge lowering all Strabo L by 100 stades? When MuJHA appeared, DR phoned Jones to stress (§§3): no ε satisfies this hypothetical new set. Unlike Diller’s 23°2/3 (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). Snaid to find in the work of one who, despite erratic judgement (prior Jones Mufa mess: Rawlins 1991W, has made additions to said sum (e.g., DIO 11.1.1 §11D, DIO 11.2 §p.30, Rawlins 20085 fn 23 & p.58, DIO 9.1 p.2); as has JHA (§1 &E1; www.dioi.org/fff.htm#cskv); & Evans (www.dioi.org/cot.htm#gnie, ggg.htm#pppp). MuJHA yields nought but chaotic (e.g., §E6 item [c]) muddying of others’ achievements. Was this its cultish destructive aim? Unreferred Jones 2002E deems worthless all DR referrer-founds touched-on: L = 31°0’4” ([f2], circuli (fn 54), klimata (fn 44), refereed by Isidis (1982), Greenwich (1985), & Isis (2002), resp. DIO 1.2 [H2, §G]’s 1991 prediction of MuJHA DR-credit-denial tactic: “Publish a wild speculation (unattested method or inferior 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.2e&4 here) Diller’s klimata scheme (Table 2), though unnoticed by Diller & his Pliny-circuit confirmities. Yet MuJHA n.9 (www.dioi.org/bm.htm). Don’t blame DR’s sardonic style for the field’s pathology, Princetitter & Mufia goddd Neugebauer’s possessive shunning and Babylonianist attacks of Diller 1934 had been going on since before DR was born (indeed, for 458 before DR ever contacted Diller): privately since 1934, and at Neugebauer 1975 p.734 n.14, calling the discovery “absurd”, not to be “taken seriously”. Such (yet-continuing) slanderous attacks on non-Muffia scholarship have never caused a ripple of public or fiscal disapproval in hist.astron circles. Yet our weary suggestion that mayhap DIO is not always wrong and the slanderers not always right (see, e.g., satire at www.dioi.org/det.htm#mhrr) is regarded as shun-worthy horrific.
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 Merokil klima’s actual value.) The long-overdue detailed DIO counter-attack against Muffiosi’s 75° shun-trashing of Diller is also appropriately in-full.

K4 It is notable that all three of the scholars who have gone into print to repel Diller’s discovery are historians who have served time at the Princetitute. As we asked at DIO 4.3 §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] to carry unfalsifiability to kook dimensions — and to raise the question of whether it is worth discussing historical issues at all. (Of course, one may easily understand why certain mortals 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 2.318 §§C20&C25] as indefinitely irresolvable, then — why investigate anything?” When this quote was applied to a few Velkiskovians at DIO 7.1 §5 fn 40, no objection was made by historians. But, can they show equanimity when the same principle is found applicable to eminent personages of their own profession?

K5 Final thought: if MuJHA represents the best that the Muffia-defense team can muster against Diller’s truth (and, pathetically enough, it is), then the issue is no longer a legitimate controversy (even for those afflicted by the numeracy-gauging delusion that it ever was) — and Aubrey Diller’s ghost can rest content on his honestly & creatively earned laurels.

References
B Some Lives Are More Precious Than Others

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

B2 Many say, “Polit-Truths ‘Free Press’”. — Courthouses & newspapers. I confess, I have no newspaper readers doubt take 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’ wuvable reverence-for-the-sanctity-of-life, which abhors the horror of possibly executing even a single person unjustly? — while the opposite page has the latest body count for the latest US war: thousands of innocent people killed for the crime of being-in-the-way. How often does the US establishment’s “Mainstream” (fn 14) press point out the contrast between establishments’ ostentatious projected concern about a “culture of life” (Libs, lawyers, & centrists against snuffing criminals; and Romans & fundies against foetus-snuffing)? — versus the taken-for-granted right of the US military to kill 11 foreigners whenever their non-cooperation becomes inconvenient for cartels’ access to their nation’s resources. The spectacle constitutes a bigoted demotion of whole classes of humans, by a nation that incessantly and censoriously preaches domestic anti-racism. (For the non-rich, anyway: DIO 8 §5 §2.) DIO 4.2 §f. &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 §f 13 fn 14&19 [1994], www.dioi.org/pro.htm/#hsa.)

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

12 fn 23. Mussolini’s 1935 invasion of Ethiopia devastatingly resembles the US′ present Afghanistan occupation, in the screaming contrast (rigorously press-uncited) of modern weaponry vs ragged, primitive, resistance fighters...
**C Occasional DI0 Prescience?**

DR thought 19 Obama wouldn’t get off (1st time around). But otherwise DI0 commentary scored some hits outside scientific history. (For indications inside: www.dioi.org/vin.htm.)

C1 Our opposition to mammograms was published in 1992 (DIO 2.11 (1) on [6]). In 2009 Nov, the dam broke and the severe risk-benefit balance we discussed is now (partially) out in public. What will distant-future commentators say of a mass program (much driven by patients’ you-never-know fears, doctors’ fears of malpractice suits, & perhaps researchers’ wish for a national data-base) expensive in time, aggravation, pain, terror, with little if any net gain other than to medibiz money? (See fn 23.)

C2 The same DI0 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’ steady glaring rich-w-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, DI0licious tasteless 1979 farce-film, Amerrickathon.

C3 In 1998, we urged (DIO 8 §5 (C)) a reconstitutive “Gospel According to Judas”. In 2006, the National Geographic Society revealed a long-secret 1970s find of a “Gospel of Judas” papyrus in Egypt. One of the books soon growing out of that was J.Archer & F.Moloney Gospel According to Judas 2007. None of this related to our interpretation of Jesus as a fiscally canny typical cult-guru, privately enjoying the 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#bib.)

C4 On 2009/11/30, DIO posted www.dioi.org/pro.htm (1pp) the charge that the Dembos’ “pandemic of lies” (one 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 (§5 fn 22) [1998] suggested the identity of Deep Throat. On 2009/10/17, DIO 1st learned of ongoing detailed expert research indicating that Mark Felt’s sole source was indeed our nominee. We await further developments.

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

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

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19 See DIO 4.2 [9 §F3 & DIO 6 §4 §C5].

It took Maureen Dowd over 20 to catch up to DI0 on the “Miss Universe” recognition: International Herald Tribune 2009/10/12 p.9. What is disappointing, about the Dembo base’s pandemc of lies is that this idea was the old, kryptonizes Dembos’, it’s in-denial on the realities (§E7) right before the 2007 posting on the Dembo lie. The idea of “health” in the Dembo’s fear that this option is getting popular is a common sense could have seen way before 2009/10/20 that these betrayals were cynically planned not just from Day-One but (like the US’ Iraq occupation) from Day-Minus-One. What does Obama have to do before the Dembo base realizes it was snookered? Dumbo acts inversely inedicable: branding him socialist, though he’s a GOP dream-come-true. But this may be just theatre to keep pushing centrism rightward.) Pre-election: Obama used ambiguities & kept-press hype to convince hopeful college kids he was the peace-option and the populist, & promoted universal medical coverage (so who’d need insurance companies?) somehow mandate-less. Post-election: keeps Bush “Defense” Sec’y; expands Medicare already covers ED medication. How smart is banning taxmoney for the poor’s abortions (Medbiz’ fear that this option is getting popular is the cause of its “health-bill”: fn 2.) What’s the chance you’ll steadily spend even 1/3 of ordmag $10000/yr, the US’ bankrupting annual health-extravagance-citizen? DIO 4.3 §11 §G3-S. (Proposed $950yr fine for cartel-dodgers: lowball-prank.) Multiplying by roughly 100 million to extreme intervention. Why decree that irrational or illegal? Except for gov’t-enforced enrichment

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**D Two Unjustly Neglected Nobels**

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

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

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**E Definitions**

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

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

E3 DIO = post-crucifixion non-public circulation = Resurrectile Disfunction.

E4 IINO (antiself-pronounced: I-no) = Independent-in-name-only, calls self Independent but, TV-zombied: votes just for Dumbos&Dembos. (Talk about wasting your vote.)

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

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

E7 Obama = pathological lawyer: [1] help poor by bailing his fatcat owners, Auda-Fio into a sudden war of desperation, or a slomo foreclosure-sale (who’s funding stimulus-paybacks?) akin to the under-rated, liciously tasteless 1979 farce-film, middle class = only domestic scal blood left for rich-owned-gov’t’s fangs to suck.

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23 Candidate Obama promised non-mandated universal health coverage but, as prez, flipped to his insurance-cartel funders’ dream: the very reverse. Talkshowblab on a (mini) “public option” diverted from this bad bet, and only in that one more basic option, the only one that can forestall the market-curb medbiz’ cancerous growth. The Pasalian (DIO 8 §5 §L) risk-benefit option to simply OPT-OUT (Medbiz’ fear that this popular government is taking the cause of its “health-bill”: fn 2.) What’s the chance you’ll steadily spend even 1/3 of ordmag $10000/yr, the US’ bankrupting annual health-extravagance-citizen: DIO 4.3 §11 §G3-S. (Proposed $950yr fine for cartel-dodgers: lowball-prank.) Multiplying by roughly 100 million to extreme intervention. Why decree that irrational or illegal? Except for gov’t-enforced enrichment
E8 Voting Dumbo or Dembo = seeking an advantageous relationship with con-men.
E9 “Modern art” = an antique. (But newer than the other “Emperor’s New Clothes”.)
E10 “Progressive” 12-Tone music = regressive music, 29 times less free than tonal.
E11 Scrawlins = Christopher Marlowe.
E12 Evidence for ESP = fraud and or bad statistics. Always.
E13 “Mainstream” Press = media owned by 1% of 1% of the public.
E14 “Fringe” Press = media owned by the other 99.9999% of the public.
E15 Bunnyrabbit religion = kill us (§A8) or we take you over by sheer numbers.
E16 Montezuma’s Real Revenge = Border-Dysentry.
E17 California = “sanctuary-state” portside open scar in the US’ Titanic.
E18 “Affirmative Action” = rich-owned-media-promoted divide & conquer enrichment (of the lower-middle classes) about every iniquity-gravience but the big one: rich v. poor.
E19 999 = number of theories the press has entertained throughout its pseudosearch to explain its eternal pseudomystery of ethnic groups’ intractable differing success-histories.
E21 Christian = worships Satan’s & thus evil’s creator. Which segues smoothly to:

F Religion & Atheism

F1 Do popular religions fight human cloning because they resent the competition?
F2 I’m outraged at Danish newspaper-publication of Moslem-insulting cartoons. The European press should be ashamed of itself — for not publishing said cartoons continuously & prominently throughout the last 30 years, to stimulate Moslem immigrants into revealing their hothead-intolerance (too-often violent, even murderously) 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. Celebr-o-beseech & churches are pop-culture substitutes for reality. Media commentators justly laugh at celebrities who’re famous for being famous. Why not a parallel observation that mass-religion is believed-in primarily for being believed-in? (See www.dioi.org/rel.htm#msjs.) What other evidence is there?

of the medibiz 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-irradiation in general, etc.

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

25See [www.dioi.org/sha.htm], and the Marlovian cases of, e.g., C.Hoffman, S.Blumenfeld, R.Barber; & see Woody Allen’s 1976 film The Front. When promoting 90%-invented Shakespeare “biographies”, the Shakespeare industry has parallels to that of professional Babylon&Prolemy-astrology hustlers: non-citation and or insult of opponents replaces logical argument; non-mention that many eminent experts disbelieve the Industry view (e.g., Hawthorne, H.James, Whitman, & Twain knew business-sense). The world is ruled by visible greedy&corrupt interests and so looks like they don’t exist at all. Exactly why god has gotten so deeply into the (we-used-to-regard-as-satanic) realm of human affairs.

26ESP is as ridiculous as seeing with your nose or hearing with your tongue, but most ESP-trained hopefuls know little stats and (DIO 4.2 § §F3) even less of the acting & magic tricks con-men use.

27DIO 4.3 § §F3 even & or question: how can there be peaceful multi-culturalism where 2 or 3 cults are competing to outmultiply everybody else? See also DIO 8 § §O2.

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

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

G2 Dueling academics’ fear of cult or math mis-steps has 2 phases: [a] timidity ere

G3 Needlessly-Divisive PC At War With Itself. [a] To PCs, “native American” im-

G4 Morally-Superior? Or Navigationally-Challenged? Whites&blacks have been

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

H3 The Underappreciated Reality of Serial Resurrection. Desperate religious folk so

H5 Among the wisdoms that come with age: the realization that making others happier &

H6 No matter the lowness of attacks on one who is striving for the ideals of truth and

30 Johnny Carson’s eulogy for Abe Lincoln: “without whom, we would not have the junk-stuff.”
31 See DIO 4.3 §13 [§1]. The US, whose power made English the world language, now has a lower percentage of English-speaking inhabitants than Denmark, Holland, Sweden, etc. The media responsible for this transformational achievement are so modest, they never even mention it.
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B. L. van der Waerden (world-renowned University of Zürich mathematician), on DIO’s demonstration that Babylonian tablet BM 55555 (100 BC) used Greek data: “marvellous.” (Explicitly due to this theory, BM 55555 has gone on permanent British Museum display.)
Rob’t Headland (Scott Polar Research Institute, Cambridge University): Byrd's 1926 latitude-exaggeration has long been suspected, but DIO’s 1996 find “has clinched it.”
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British Society for the History of Mathematics (Newsletter 1993 Spring): “fearless . . . [on] the operation of structures of [academic] power & influence . . . much recommended to [readers] bored with . . . the more prominent public journals, or open to the possibility of scholars being motivated by other considerations than the pursuit of objective truth.”