

‡3 Aubrey Diller, Hipparchos, & Sph Trig's History

**Does Spherical Trigonometry Go Back to the 2nd Century BC?
Unexpected Perfect-Fit 2009 Induction Snuffs 75^y Controversy
Reflex Alex Jones Banzai-Sneers at Three Refereed Discoveries
Strange JHA 2002 Attack on A.Diller Finally Refereed 7^y Late
Uncriticized Inept Archonal Chauvinism Disgraces Entire Field**

A Diller's Tri-Discovery: Early Sph Trig, Accurate Tilt, Klimata Key

A1 In 1934, the eminent classical philologist Aubrey Diller provided the 1st conclusive proof that spherical trigonometry went back to the 2nd century BC, 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) ϵ_{H2} equal to $23^\circ 2/3$ or $23^\circ 40'$: eq.2 below.

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

A3 If we assume ϵ_{H2} was measured in standard fashion (eq.8, below) and account for refraction¶llax, an ideal Hipparchos determination of ϵ would have been $23^\circ 42'$, and standard ancient rounding was to the nearest $5'$, so $\epsilon_{H2} = 23^\circ 40'$ was correct to its precision. Even ignoring rounding and r&p, it was (as it stands) accurate to about a 20th 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 (ere Diller) no one had previously suspected that the ancients ever had an accurate obliquity. In short, Diller 1934 simultaneously announced 3 major discoveries: [1] solution of Strabo's klimata, [2] Hipparchos' use of spherical trig, [3] his adoption of the only accurate obliquity-measure we can recover from antiquity. That a pack of possessive snobsters has nearly submerged such scholarly triumphs for 81^y is itself a triumph of organized truth-warping, providing a history (see p.2 & fn 7) warmly recommended to sociologists of cult-think.

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

$$1^\circ = 700 \text{ stades} \quad (1)$$

(Strabo 2.5.7&34 or Neugebauer 1975 p.305 n.27).³ All 13 said data were computed from klimata M values via eq.4 (below), using the unattested but impressively accurate ϵ value

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

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

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

³JHA "refereeing" missed that [a] MuJHA n.7 cites the wrong volume of Neugebauer 1975, & [b] MuJHA n.10 consistently muffs Syracuse's Strabo L by 200 stades. Not the 1st time this fitfully perceptive & ever creative author has been let down by fake refereeing. See also fn 31 & DIO 1.2.

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

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

B1 Since 1934 it has been known that the standout non-fit for the Diller theory is Meroë, the 13th klima. Meroë was 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 and⁴ others) finally followed in the tradition of Kepler and A.C.Doyle⁵ by realizing that the aggravating non-fit is precisely what can be beckoning one on to new discoveries.

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

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

(Diller himself suspected this: §B5.) I.e., 11800 stades for Meroë city should never have been in the Strabo-klimata tables of Diller 1934, Neugebauer 1975 p.305, or *DIO 4.2* p.56 in the 1st place.⁶ 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 call⁷ the 14th klima "Alexandria". (Strabo 2.5.38 inadvertently does likewise: §F4.)

B3 The case of Meroë is complicated by the fact that there is both an "island" Meroë (described at Strabo 1.2.25 as the Nile's largest: §B4) containing, in its north part, the city Meroë, whose actual latitude is $L = 16^\circ 57'N$. It is the city-vs-island confusion (as also for Rhodos) we will now eliminate. *The very names of klimata illustrate that most were named not for cities but for sprawling regions*⁸ — such as bays, straits, river-mouths, or islands. (In ancient geography, Syene [modern Aswan] is often just a sloppy amalgam of city, tropic, & klima.)⁹ 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

⁴See, e.g., Rawlins 1991W §D2 and *DIO 4.1* ‡3 §A5 [p.35]. Also the ancient galactic-circle CygSegment on the Farnese globe: www.dioi.org/fff.htm#gtgm.

⁵See *DIO 4.1* ‡3 fn 2 [p.33].

⁶This realization is not our 1st restorative correction of the mis-filing of a famous item. See, e.g., *DIO 8* ‡5 fn 5.

⁷E.g., Pliny 6.212; Honigmann 1929 (*The Seven Klimata and the Important Cities*) pp.34, 40, 43.1, 52, 147; S&G p.116 n.4. Also Neugebauer 1975 pp.730&732, though at p.305, mathematician Neugebauer deluded himself into thinking his own theory better than non-mathematician Diller's (an imagined superiority insultingly expressed at p.734 n.14 — but now accepted nowhere) by: [i] Overruling 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, even while knowing that his scheme didn't fit them but the exiled Diller's did). [iii] Skipping the 15th 1/2 Pontus 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/biv.htm#lqsn.)

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

⁹Contradictory ancient definitions of Syene are touched upon below at eq.17.

Table 1: Hipparchan Klimata Fits: Princetitute vs Diller-DR

Klima	Longest Day <i>M</i>	Hipparchos-Strabo <i>L</i> [Data]	Princetitute-Muffia <i>L</i> [Babylonian]	A.Diller-DR <i>L</i> [Greek]
Equator	12 ^h	0	1500	0
Cinnamon	12 ^h 3/4	8800	10200	8800
Meroë	13 ^h	11600	12800	11600
Syene	13 ^h 1/2	16800	17600	16800
Lower Egypt	14 ^h	21400	21800	21400
Phoenicia	14 ^h 1/4	23400	23700	23400
Rhodos	14 ^h 1/2	25400	25500	25400
Hellespont	15 ^h	28800	28800	28800
Massalia	15 ^h 1/4	30300	30300	30300
Pontus	15 ^h 1/2	31700	31600	31700
Borysthenes	16 ^h	34100	34100	34100
Tanais	17 ^h	38000	38000	38000
S.Little Britain	18 ^h	40800	40800	40800
N.Little Britain	19 ^h	42800	42800	42800

Ptolemy's *GD* — perhaps as early as Hipparchos. See *DIO* 5 fn 19 on commerciality.) So it would make sense that the 13^h klima was for Meroë *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 Meroë are generally expressed with respect to other *cities*. Indeed, since these distances are (§C2) due to Eratosthenes (who probably did not use sph trig klimata) they cannot be klimata-based and their contexts usually do not discuss hours.¹⁰ When Strabo finally speaks of the supposed Meroë *klima*, he does not speak of a spot called Meroë (as elsewhere) but says (Strabo 2.5.36): “In the regions of Meroë and of [Ptolemy's Hunting Lodge], the longest day [*M*] has thirteen equinoctial hours”.

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

B5 Diller 1934 p.267 realized the difference between the Meroë city & klima but supposed (like DR for decades) that Strabo had neglected to supply the klima's *L*. Which brings us to reprising the shocker 1st revealed in *DIO* 5 (2009). By contrast to all his inter-city placements of Meroë *city* (fn 10): during his lone reference to the Meroë 13^h *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 5^d before *DIO* 5's online publication! — this, after 25^y of delay in publishing Diller's *GD* 8 ms in that volume, as long planned): *since the context*¹¹ *is klimata* (not cities) and since the 14^h klima is at 21400 stades (Table 1

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

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

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

Klima	Longest Day <i>M</i>	<i>L</i> Computed via Sph Trig Eq.4	Rounded to Nearest Degree 12 th	Converted to Stades via Eq.1	Rounded to Nearest 100 Stades
Equator	12 ^h	0°	0°	0	0
Cinnamon	12 ^h 3/4	12°36'23"	12°7/12	8808	8800
Meroë	13 ^h	16°35'04"	16°7/12	11608	11600
Syene	13 ^h 1/2	23°59'43"	24°	16800	16800
Lower Egypt	14 ^h	30°33'49"	30°7/12	21408	21400
Phoenicia	14 ^h 1/4	33°31'04"	33°1/2	23450	23400
Rhodos	14 ^h 1/2	36°15'25"	36°1/4	25375	25400
Hellespont	15 ^h	41°07'34"	41°1/6	28817	28800
Massalia	15 ^h 1/4	43°16'44"	43°1/4	30275	30300
Pontus	15 ^h 1/2	45°15'40"	45°1/4	31675	31700
Borysthenes	16 ^h	48°45'50"	48°3/4	34125	34100
Tanais	17 ^h	54°14'53"	54°1/4	37975	38000
S.Little Britain	18 ^h	58°12'31"	58°1/4	40775	40800
N.Little Britain	19 ^h	61°04'56"	61°1/12	42758	42800

AND¹² Strabo 2.5.38), we use this Alexandria **klima** latitude to solve for the Meroë **klima** latitude *K* by simple arithmetic in stades:

$$K - (21400 - K) = 1800 \implies K = (21400 + 1800)/2 = 11600 \quad (3)$$

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

B6 Muffia 2002-2009 reaction? Strabo's klimata data suddenly aren't trustworthy anymore!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] §F4 & [b] eq.3.)

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

¹³ Has any academic cult ever matched the Muffia's gift for (1934-2002) rejecting virtual perfection in favor of a theory fitting (Table 1) merely half the available data? See www.dioi.org/biv.htm#kpv.

¹⁴ A dodge which only entered the debate in 2002 when the hilarious Diller-vs-Princetitute-Neugebauer contrast of Table 1 finally caused *Isis*' citation of the devastating display of it at *DIO* 4.2 p.56. To sum up: **the very same data** that were for decades unrelievedly sacrosanct to Muffios (being the basis of the Muffia-Princetitute *DSB*-placed whacko Neugebauer scheme: see Table 1 & **especially** §J1 here) — who typically team-permitted not a peep of doubt on the subject — are dumped just at the moment the cult is *Isis*-faced with the fact that these data much more convincingly back Diller, not his slanderer (fn 58), Muffia-guru Neugebauer. One is reminded of the notorious BS 2001 *JHA* attack on Hipparchos' authorship of the Ancient Star Catalog, based on assuming a high atmospheric density. When BS told DR of this plan (1999/10/1), DR immediately suggested the reverse: use Hipparchos' established authorship to gauge ancient atmospheric opacity. (See www.dioi.org/gad.htm#fnpw for this and similar cases of mis-weighting evidences' relative strengths.) In the present instance, it would have been wiser to realize that the steadiness of the fit of Diller's math tells us that Strabo's data (if not always his interpretations) are more trustworthy than some of us had previously thought — and that is yet another enlightenment owed to the original intellect of Aubrey Diller.

of the all-too-predictable: “DR to Muffia: Is 13-out-of-13 Euffia?” See *DIO* 5 fn 22 and in-love-Osgood Gingerich at *DIO* 11.3 ‡6 §A1 on the Muffia’s decades-long tolerance of all manner of imperfection in Ptolemy&Neugebauer, even while (the source being of non-Muffia breeding) blind to a now-literally perfect fit. (Thereby inverse-fastidiously nonnutting *Some Like It Hot’s* original indefatigable old masher 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 numerical 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 was most likely careful 1′ measurement. Note that the city latitudes cited at §B3 are largely accurate¹⁵ to ordmag 0°.1. Rawlins 1982G shows that Eratosthenes had learned that Rhodos City’s $L = 36°5/12$ (good to 1′) — or 25500 stades — and that only his foolish use of gnomon for Summer (not Winter) Solstice threw off his measure of Alexandria’s L by half the solar semi-diameter, yielding 31°04′ (Rawlins 1982C eq.10, Rawlins 1994L fn 44). His place for Alexandria was (like Meroë’s) adopted (Strabo 2.5.7) by Hipparchos (who never visited Africa) and typically rounded to 31°1/12¹⁶ & 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^d before S.Solst is encouragingly accurate.¹⁷ Strabo’s report of gnomon-use at first looks discouraging due to its systematic error from solar semi-diameter ssd . However, while outside the tropics, ssd will foul up the L half of eq.8 instead of the ϵ half, the reverse is true in the tropics. A transit instrument would

¹⁵Even the rough latitude for Athens (Strabo 2.1.2), 38°+, is much better than Hipparchos’ later false value of c.37° (Hipparchos *Comm* 1.11.3&11), which became adopted in astrology manuals for centuries after, e.g., *GD* 3.15.22 & (see *DIO* 5) 8.12.18. (For speculative explanation of his error: www.dioi.org/fff.htm#rvbv.) This relates to DR’s contention (www.dioi.org/fff.htm#gbpp) that most astronomers (as against astrologers) knew Athens’ actual latitude, and that this may relate to the origin of the Farnese globe: of indicated home latitude 38° (presumably either Pergamon or Athens).

¹⁶Rawlins 1994L fn 44. Strabo 2.5.39 confirms this by putting Hipparchos’ Alexandria 3640 stades south of the Rhodos 14^b1/2 klima, thus at latitude 21760 stades or 31°1/12. Further if less precise confirmation: Strabo 2.5.38 says the transit of Arcturus is a little south of the zenith, consistent with the star’s quite erroneous 31° Hipparchan declination (*Almajest* 7.3). We have elsewhere proposed that since culminating Arcturus was actually c.0°.1 north of the zenith at Alexandria in Hipparchos’ era, he (again: ‡1) made a sign or translation error and subtracted c.0°.1 from his 31°1/12 Eratosthenian Alexandria latitude to find the awful figure 31° (error –0°.3) for the declination of Arcturus.

¹⁷The actual interval would have been 46^d. 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^d apart? Philo would then find the S.Solst 1/2 way between those two dates and report the semi-arc as half of 91^d—, or: 45^d.

get the correct L , but even if we assume¹⁸ that Philo used a gnomon at both solstices, he would have found (accounting for both r&p 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 - threesome: Meroë 16°11/12, Syene 24°, Alexandria 31°1/12: 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-discovered¹⁹ (Eratosthenes&Hipparchos-adopted), that Syene is exactly 1/2-way between Alexandria & Meroë, the Hipparchan value²⁰ for both intervals being 7°1/12 or (by eq.1) 5000 stades. In reality (using eq.1), both L intervals are even closer to 5000 stades (sum 9975 stades): could this accidental symmetry be one of the causes of eq.1’s establishment? (By Philo? Sostratus? Eratosthenes? Anonymous?)

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

D The Birth of Spherical Trig

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

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

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

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

$$\epsilon_E \doteq 23°51'20'' \doteq 180° \cdot 11/83 \quad (5)$$

or the early Hipparchos obliquity

$$\epsilon_{H1} = 23°55' \quad (6)$$

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

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

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

¹⁸Possibly Strabo made no distinction between asymmetric gnomon, symmetric gnomon, and transit instrument. Regardless, it appears that Philo was discoverer of the later-canonical A-S-M symmetry, which was abandoned by the time of Ptolemy, whose intervals were: A-S = 7°1/6 vs S-M = 7°5/12. Ptolemy’s klima→city Meroë confusion caused a 1°/2 discord between his & Hipparchos’ L , hinting that Hipparchos was not responsible for the GD ’s klima-polluted L mis-geography.

¹⁹If Philo travelled to Meroë, he must have visited Syene. So he presumably knew that its latitude was 24°05′. And every scientist but Eratosthenes (§C2, Rawlins 1982G, Rawlins 1994L Table 3) then knew that Alexandria’s L was nearly 31°1/5. So the A-S-M symmetry was not only true but competently known to be true in Alexandria’s community of genuine scientists, which again excludes Eratosthenes. His & Hipparchos’ later symmetrical A-S-M schemes were (as just noted) slightly less accurate than the presumed original latitudes (of, e.g., Philo) but were perhaps nudged to ensure adherence to an A-S-M symmetry likely well-known long before either’s geographical scheme.

²⁰Doubling makes Hipparchos’ Meroë-Alexandria distance 14°1/6, so (eq.1) not 10000 but 9900 stades. But Strabo (2.5.7 & 17.3.1) says “about” 10000 stades. This favors 16°11/12 (& thus Philo’s accuracy) as Eratosthenes’ & Hipparchos’ Meroë L , rather than 16°5/6. Either satisfies 11800 stades.

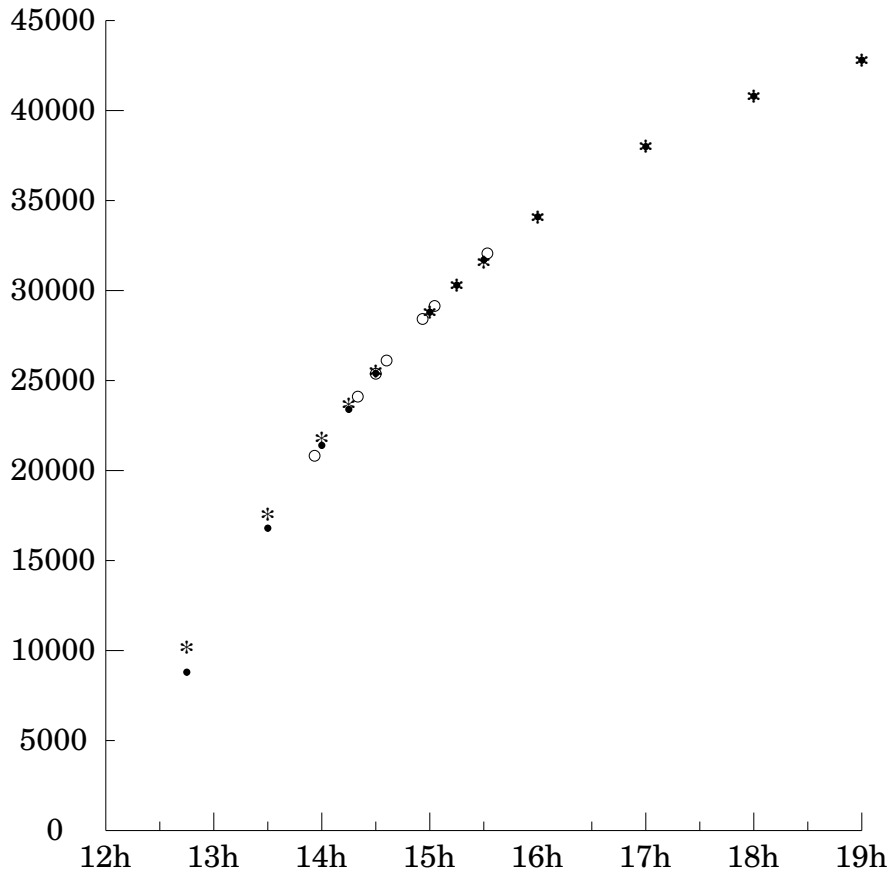


Figure 1: Latitude L graphed in dark (filled) dots as function of longest day M , sph-trig-computed via Diller's eqs.1, 2, & 4, at all 14 M for which Strabo gives Hipparchan L : M in hours; L in stades, rounded (like all Strabo klimata) to 100s. Each Strabo datum is marked by the same dark dot, since Diller's sph trig theory matches perfectly for all 14 cases. Asterisks mark the corresponding L for the arithmetical folly (fn 7) which Neugebauer ineducably Princetitooted his horn for. North of Rhodos, the dots&asterisks nearly merge in most cases (former smaller, so superposition won't prevent seeing both), but the Neugebauer theory's failure at lower L (most amusingly at Equator: §J6!) is lethally blatant. Hollow dots mark the 7 klimata of Rawlins 1985G p.263's reconstruction of the pre-tampered original of the scheme underlying Pliny's "circuli" (fn 50), showing how neatly the ingenious device of the ancient creator (§I: Hipparchos?) tracks klimata for the slim range of Mediterranean L which it was invented to fit via Diller's Hipparchan ϵ : fn 50. Jones' "unshifting" all Strabo L by 100 stades would be hard to show in our graph since the amount is so tiny ($1^\circ/7$) that the shifted points would be *inside the dark dots* marking Strabo's data. The fit is so *fragile* that such a minuscule shift destroys (§I3) any chance of fitting eq.4 to the Strabo data, regardless of ϵ -choice. So the graph's larger message ironically redounds against Jones' §I1 prong [b] wetdream that "one or two modest changes in the intervals" could best Diller: the above curve is too *super-precisely* characteristic of sph trig eq.4 & Hipparchan ϵ to allow explicit or even implicit denial of credit to Diller as discoverer of the true basis of Strabo's Hipparchan klimata, which lay secret for 2 millennia.

D4 Since §D3's correlation [a] was found via the sph trig of eq.4, we have here (also Rawlins 1982N n.11) a shaky suggestion that (contra §E1) sph trig was known in the 3rd century BC. Indeed, there is even a hint (†2 fn 32) that trig may go back to c.300 BC. Trig's absence from surviving mathematical texts (e.g., Rawlins 2008Q fn 32) has been taken to indicate its late appearance; but another possible explanation is that trig was long scorned (by academically powerful pedantic pure-math geometers) as a mere engineers' tool which should not foul mathematical treatises. (The potential analogy with Isaac Newton's presentations in his *Principia* is obvious.) Powerfully against this theory, however: Eratosthenes' important geographical parallels (e.g., Meroë, Athens, Hellespont, Borysthenes) appear to be unrelated to klimata calculations. (The eq.2 calculations via eq.5 in §D3 [a] seem to be Ptolemy's, not Eratosthenes'.)

D5 If known to Eratosthenes, the simple double-sunset Earth-measure method (requiring sph trig) would have faced him with the large disagreement between the lighthouse method's 256000-stades (likely known before him: Rawlins 1982N p.215 & Rawlins 2008Q §I1) vs the sunset method's 180000-stades. (The latter being the Poseidonios-Marinos-Ptolemy value which eventually became dominant. Conversion discussed in Rawlins 2008Q & Rawlins 2008S.) Did he face the disjunct? (See detailed discussion at *DIO* 5 fn 18.)²¹

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

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

E2 But, at a time when the hist.astron field is run by "just a bunch of politicians" (as an aghast eminent astronomer describes the field's debate-fleeing dominatrices), the reaction to such a massive demonstration of said gang's fallibility is predictably Doc-Cookian: deny, deny, deny — *never confess*. (See Joey Bishop at *DIO* 11.2 [2003] pp.32-33.) It would be merely pathetically funny if it weren't so damaging to the balance of communal micro judgement here and macro understanding of the entire subject of ancient astronomy.

E3 Continuing obtuseness (§E1 item [c]), defying Diller 1934's multiple [now *SEVEN-FOLD*] predictive vindications (*DIO* 5 §D3), is noted at *DIO* 11.1 p.26 fn 1 item [iv] — as well as the even more revealing fact that the near (now total) perfection of *DIO* 4.2 Table 1's fit is not mathematically challenged (or challengeable) *nor is this literally PERFECT fit even mentioned by the history of ancient astronomy community*: As of 2015, that's 21^y of cultist bibliographical deceit²² by this odd community, which only adds to the parallel disgrace of not even comprehending the statistical and Occamite preferability glaring from Table 1.²³

²¹If 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*.

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

²³The 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 $23^\circ 2/3$ obliquity have been the *ArchiveHistExactSci* (Nadal & Brunet 1984 p.231 & n.17); also *Isis* (Thurston 2002 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 coincidental

E4 Is it really this easy for a few rebel scholars to cripple (into freewill-deprived zombiedom) a whole community, merely by inadvertently manipulating that community's lethal mixture of [A] historical bigotry (the inertia of which tends to produce embarrassing non-priority in discovery-making), plus [B] the resultant sociological bigotry of embarrassing (thus silent) careerist cooperation in academically-outwitted power-genius archons' vindictive exilings?²⁴ May bigots' turf-possessiveness be succinctly summed up as: Pigotry?

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

E6 Sadly, an apostolic succession of modern cultists has outrageously defied common sense — and (needlessly) risked degrading still further their reputations *vis-à-vis* balanced scholarship — by fighting the obvious for **four-fifths of a century, now**, 1934-2015 (a span whose very magnitude has thus far only intensified the amorally unrepentant culprits' never-confess determination to throw worse reputation after bad), even though their methods for doing so have run the gamut from [1] Babylonianly claiming a more historical theory than Diller's (Neugebauer 1975 p.305 & p.734 n.14), to [2] a contentless argument that Neugebauer's authority meant more than Diller's math (top Muffioso to DR by phone c.2000), to [3] randomwalkingly or vaudevillianly (§K2) reversing field by jettisoning²⁷ Neugebauer's theory (after our Table 1 [originally 1994, augm. 2002] had made it a failed joke among neutrals) and now instead shifting to trashing the very same universally-understood coherent data-base (middle column of above Table 1: Neugebauer 1975 p.1313) upon which Neugebauer's authority on the issue had been founded and accepted for decades. This final stoat-squirm (above, p.2) occurred in a curiously semi-Muffiose-theology apologia-paper Jones 2002E (frequently called "MuJHA" here to accent its cult-think roots). The new 2002 tactic: outside the beautifully consistent Table 1 (whose klimata were clearly published by Hipparchos as a whole at a single time), find some sort of inconsistency elsewhere in other

that the 2002 *JHA* anti-crimethought exercise appeared instantly after the *Isis* paper [citing *DIO 4.2* p.56 Table 1] was circulating among cultists for refereeing? See www.dioi.org/biv.htm#txpv.)

²⁴ For one of the most flagrant deadbrain-kissing non-citation performances ever accomplished in the Ptolemy controversy, see at *DIO 8* p.2 [1998] the details of the deliberate, systematic behavior (where the osculation factor trumped even the shun-factor: www.dioi.org/fff.htm#msnc) which helped earn super-cowerer J.Evans his advancement into Assoc. Editorship of the *Journal for the History of Astronomy*. (Evans has recently tried silkpursing a handy establishment goon by publishing his output at *JHA 38*:199-206 [2007], without realizing that the paper's proposal lacks statistical significance [www.dioi.org/aeg.htm] or, indeed, perceiving that the paper is statistical at all.)

²⁵ From www.dioi.org/mot.htm#gddb: "There is no agnostic so ready to embrace doubt as a believer when faced with . . . evidence . . . inconveniently-inconsistent with his undislodgably-sacred tenets."

²⁶ At www.dioi.org/vols/we0.pdf p.31. Two pregnant questions are naturally suggested by the outré spectacle of Muffiosi shunning (& thus trying to suppress public gratitude for) Diller's greatest discovery: [i] Why do observers so rarely note that shunners are customarily less brilliant **and by-definition less brave** than shunnees? (See, e.g., p.2 fn 1; or www.dioi.org/sno.htm#hvtv.) [ii] And why, in such situations, can one count on all but the best of the "science press" to undeviatingly, fawningly trust & promote the former, not the latter? — while censoring all mention of their gurus' censorial behavior.

²⁷ If this indicates that denigrating a DR-associated achievement has higher Muffia priority than worshipping Muffia-godpop Neugebauer, that's some progress. But such little-steps (see 1998 note at *DIO 1.2*) haven't taken us far in the last decade. Except backwards, via Muffiosi's very littleness?

numbers of Hipparchos (who was glorious or notorious for varying his parameters throughout his career: §F1) — which can then be used to engender doubts of Diller's [a] deduced obliquity (eq.2); & [b] data-base (Table 1: middle col.). MuJHA uses a two-prong attack. Prong [a]: At p.16 mis-taking a calculation for an observation. Reverse at p.17 (§F here). Prong [b]: More such confusion at MuJHA n.9. (§I below).

E7 Putative *JHA* refs for MuJHA missed the stark contradiction between §E6's prongs [a]&[b] (such embarrassments inevitably issue from chavinist lastditch banzai-determination tryanythingitis): the $\epsilon = 23^\circ 51' 20''$ (eq.5) MuJHA pp.16-17 proposes in §F1's prong [a], is inconsistent with that which would be produced by prong [b]'s proposed data-set-alteration. (Awful numerical details at §§I3&J.) We now examine the §E6 [a]&[b] prongs of Jones' indiscriminate creativity (which he's very fortunately corrected in DR's work as well: see *DIO 11.2*'s cover), a classic Muffia vision of ancients as semi-empirical number-jugglers (Neugebauer 1975 pp.642-644; *DIO 1.1* ‡1 fn 24), contra *Almajest 1.12*'s description of ϵ '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] (§E6). Diller had been 1st to discover that Hipparchos' ultimately adopted obliquity was the lost value $23^\circ 2/3$, which is accurate (much better than the values MuJHA urges for H, implicitly&explicitly) and has since been validated in various ways (summarized at *DIO 5* §D3). MuJHA pp.15&17 notes that Ptolemy ascribes to Hipparchos the Eratosthenes value²⁸ $\epsilon_E = 23^\circ 51' 20''$ (eq.5). As §J5 notes, this is of little weight since we know (see the MuJHA author's own Hipparchos entry in the *Encyclopedia of Astronomy and Astrophysics* 2000) that Hipparchos repeatedly changed parameters.

F2 To "prove" to a precision of a few arcsec ϵ_E 's origin, Jones 2002E p.16 rigorously rounds L from $\arctan(3/5) = 30^\circ 58'$ 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) $L = 21700$ stades for Alexandria without noting that not just 31° but $30^\circ 58'$ and (Rawlins 1982G) $31^\circ 04'$ (the only L of the three which is independently related to Eratosthenes: Rawlins 1982G) are also consistent with 21700 stades: i.e., no confirmation. When he subtracts the traditional Alexandria→Syene 5000 stades

$$31^\circ - 5000 \text{ stades} / (700 \text{ stades/degree}) \doteq 23^\circ 51' 26'' \quad (7)$$

he gets close to eq.5 instead of the $23^\circ 49'$ (like eqs.16&17) which he **would** have gotten without that arbitrary 31° rounding. Compare via Occam's Razor to an unjuggled solution (Rawlins 1982G) which **simultaneously** solves precisely for three Eratosthenean data: [1] eq.5's $23^\circ 51'$, [2] $L = 31^\circ 04'$ (not 31°), & [3] $Z_s = 7^\circ 1/5$. Jones 2002E pp.15-16 baselessly says Strabo's *supposed* (vs §F4) Alexandria "equinox-shadow-ratio" 3:5 & its implicit $L = 30^\circ 58'$ were Eratosthenes'. A key misimpression (§F3): $L = 31^\circ$ is said (p.16) to be "derived from the equinoctial shadow" though standard ancient measure of L got it via *solstice*²⁹ (not equinox) data. The method is attractively simple (*Almajest 1.12*): just halve the sum of S.Solst & W.Solst app.noon zenith distances Z . But the process also automatically produces the obliquity ϵ , 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 ϵ as part of finding L , so (don't miss Jones 2002E p.15's curious phrase "might suggest"): why compute **already-known** (via eq.8) obliquity ϵ through the more laborious long-division required (above: eq.7) by the MuJHA p.16 method?³⁰

²⁸Hipparchos may've used eq.5 at some point in his long career. But not when computing Table 1.

²⁹Solstices are used to find L because measuring equinoctial s_e/g is vitiated by non-parallelness of instrumental & real equators. Which, among other reasons, is why all known observers before non-observer Ptolemy fixed their calendars by solstices, whose times are not affected by equatorial mis-set (or refraction or parallax). And even Ptolemy knew to find L by solstice observations; *Almajest 1.12*. The superiority of solstitial data (vs equinoctial) is well explained at R.Newton 1977 pp.81f.

³⁰Whatever may be the merit of MuJHA's try (our eq.7) at relating ϵ , 5000 stades, & an (inexplicitly)

F3 MuJHA's non-refereed history is revealed by two slips.³¹ (At *literally* [§F2] chapter-one-*Almajest* sophistication.) Ancient astronomers' "equinoctial" ratio s_e/g (horizontal shadow length s , divided by vertical gnomon height g) isn't "derived from the *equinoctial* shadow" (MuJHA p.16 emph added) but from *solstitial* observations (*Almajest* 1.12):

$$\text{latitude } L = (Z_w + Z_s)/2 \quad \text{obliquity } \epsilon = (Z_w - Z_s)/2 \quad (8)$$

With g standardized at 60 (*Almajest* 2.6; evidently 120³² in Pytheas' day: eq.10), the equinoctial equation is:

$$s_e/g = \tan L = \tan[(Z_w + Z_s)/2] \quad (9)$$

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

F4 MuJHA's prong [b] 2nd try at gutting Diller 1934 is an argument for E's obliquity having been used by Hipparchos: contra §F3, Jones claims that Pytheas' famous S.Solst. solar altitude (c.300 BC) shadow/gnomon³³ ratio s_s/g at Massalia³⁴ (modern Marseilles)

$$s_s/g = \frac{41 \frac{4}{5}}{120} \doteq \tan 19^\circ 12' = \tan Z_s \quad [\ddagger \text{ eq.1}] \quad (10)$$

whole-degree-rounded L , the chronological order of finding these data is unlikely to have been as suggested. Jones 2002E p.16's pure speculation, that Eratosthenes' Alexandria $L = 31^\circ$, is as unsupported as is the same page's connexion of our eq.11 to him. By contrast, Rawlins 1982G p.264 used Eratosthenian data with expected solar-semi-diameter errors in a *coherent argument* to show that his Alexandria $L = 31^\circ 04' = 21750$ stades, which accounts for the unusually precise stades' 10s-place ending of Strabo 2.5.24, as well as Hipparchos' Alexandria $L = 31^\circ 05' = 21760$ stades (explaining the also-exceptional stades' 10s-place ending of Strabo 2.5.39, as well as *GD* 4.5.76's Pharos L), which is merely Eratosthenes' L processed through Hipparchos' usual 5' rounding. (See similar E→H rounding of 4' to 5' at §H3.) These 2 Strabo passages are thus consistent with central site Rhodos' L being 25500 stades (city) for Eratosthenes & 25400 stades (klima) for Hipparchos.

³¹ One key error is at p.15 line 6 [detected in 2002 by Thurston], confirmed by another at p.16 line 4. These are noted at *DIO* 11.1 p.26 n.1 & www.dioi.org/cot.htm#ucmf. If one repeatedly chooses (though legitimate journals exist in the field) to publish in a forum which one knows perfectly well has a long record of slapdash (see www.dioi.org/qqq.htm#hhwc!) to non-existent refereeing (and whose Editor-for-Life strikes hate-objects from His list of those scholars whom He might choose as His referees [an offense which alone destroys a journal's claim to having a reliable&honest refereeing process] even when He knows [*DIO* 6 ‡3 §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 improprietly-unanticipated destruction of communication inevitably created by fawning on semi-numerate archons who atone for their own inductive sterility by such creativity as shunnings.

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

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

³⁴ Strabo 1.4.4 (& 2.1.12), 2.5.8&41. Some of the Strabo report has Hipparchos placing Massalia and Byzantion on the same 15^h1/4 klima. While Massalia is close to the implied 43° parallel, Byzantion

was not an observation! (Zenith distance Z is the complement of altitude h .) Though, almost everybody else has realized it was an observation, as MuJHA n.11 creditably notes. MuJHA claims it was instead just a calculation, because (?) it was presented in such precise form. Jones adjacently claims that 2 other gnomon ratios, both equinoctial (Alexandria 3:5, Carthage 7:11), are *empirical* because of roundness, i.e., *because the 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 1985G pp.263-264 & n.17.)

$$\text{Alexandria } L = \arctan[s_e/g] = \arctan[3/5] = 30^\circ 58' \doteq 21700 \text{ stades} \quad (11)$$

$$\text{Carthage } L = \arctan[s_e/g] = \arctan[7/11] = 32^\circ 28' \doteq 22700 \text{ stades} \quad (12)$$

Eq.11's 100 stade difference vs Strabo's 21800 stades for Alexandria (Neugebauer 1975 p.1313) is one of the three bases for Jones 2002E n.9's proposed 100 stade shift of all the klimata. But such a shift would maintain Strabo's L -differences, yet the difference between eqs.11&12 is 1000 stades, contradicting the difference at Strabo 2.5.38 (900 stades). (I.e., why does Jones 2002E use the Alexandria discrepancy between eq.11 & Strabo's L , while ignoring the corresponding Carthage non-discrepancy?) Carthage's 7:11 ratio is obviously **non-empirical**, since 32° 1/2 is waaaaay (over 4°!) too far south of actual Carthage, fatefully distorting maps of the N.Africa coastline for the next millennium. And the explanation for this ancient disaster is the very same as for Alexandria's **true Strabo ms reading**, namely 7:5. (Not 3:5, as MuJHA n.10 scrupulously notes.) Thus it is not the g/s_e ratio but is the *longest/shortest-day ratio* M/m for the Alexandria klima where $M = 14^h$ — just as the 7/11 ratio for Carthage is not the s_e/g ratio but the m/M ratio for the $M \doteq 14^h/3$ 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.³⁵

is 2° south of it. So, for purposes of testing the reality of eq.10 (& ‡2 eq.5), we may ignore Byzantion (Hipparchos' native area) entirely. But then: if we are reduced to Marseilles (Pytheas' native city: §G1), wouldn't MuJHA's p.17 sph trig be *Pytheas'* calculation? In c.300 BC?! (Note: the later *Almajest* 2.6's *calculated* S.Solst s_s/g for Marseilles does not equal the Pytheas s_s/g : fn 38.) MuJHA's author isn't really asserting such a thing. (DR speculation: If Hipparchos claimed he measured [Strabo 1.4.4] the Byzantion latitude, he may have been referring not to vertical instrument work but to calculation from an observation [badly corrupted by e.g., refraction, dip, etc] of ortive amplitude $\arctan(2/3) = 33^\circ 41'$ [which is anciently listed for Byzantion: see Rawlins 1991W §K2], though this latitude can also be explained by computing via Neugebauer 1975 p.37 eq.5a with $\epsilon = 24^\circ$ for $M = 15^h 1/4$.) In any case, MuJHA is correct that Byzantion was a klima for Hipparchos and Ptolemy, but that does not mean that eq.10 was unreal, especially since it does not quite agree with a latitude calculated by eq.4, so that it appears that Hipparchos merely used the *proximity* of the L corresponding to eq.15 (motivated by tradition or cataloging priorities) to name the 15^h1/4 klima. (Did he treat Pytheas' s_s/g as a valued heritage [perhaps famous for its obvious precision] from the earliest days of observational transit-work astronomy? — evidently *the oldest surviving transit [vertical instrument] raw observation*, presumably prior even to those of Timocharis & Aristyllos.) For convenience, ancients casually merged-confused Syene city with the nearby 13^h1/2 klima (eq.17). Hipparchos similarly used (fn 55) the *proximity* of α UMi's *NPD* (not its exact value) to indicate the position of the 12^h3/4 Cinnamon klima. (Are we to suppose that Hipparchos could count on real stars being *exactly* on a Z which agreed with indoor klima computations of M ?) Or are we to suppose that immortal (stellar) astronomer Hipparchos just indoor-computed (by eq.4) the Z of such major stars as α UMi (2nd magnitude present-day Polaris)?

³⁵ See sources cited at fn 43, which remarks that these identical ancient confusions occur in the same identical paragraph: Strabo 2.5.38. (Analogous to the provocative coincidence pointed out in Rawlins 1996C §I15 at n.119.) Note that the Carthage $m:M$ theory's implicit L (38° to 37° 1/2, via eq.4, depending on the ϵ preferred by the ancient computer) fits Carthage's real L (36° 51') 4 to 6 times better than the s_e/g theory's 32° 1/2. Computing with eq.4 for Carthage's actual L (36° 51') & any anciently adopted ϵ , $M = 14^h.6$; and the nearest klima in the typical ancient tables reproduced at Neugebauer 1975 pp.722&732 would be 14^h2/3, which is the M corresponding to $M:m = 11:7$, the very Carthage ratio left us by Strabo 2.5.38.

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 (‡2 eq.2) just ordmag 1', which is suspiciously consistent with *careful outdoor measurement*.³⁶ NB: Strabo 7.3.1 regards Pytheas as an expert. He also reports (Strabo 2.5.8) Pytheas was a *Massalia native*, obviously enhancing odds that eq.10 is a real 1st-hand observation, and that *this observation was oft repeated to get it just right*. So there is no reason to follow MuJHA's p.17 rejection of Diller's unquestionably-calculated³⁷ *fourteen* perfect fits to eq.4, just on the basis of MuJHA's infirm *speculation*³⁸ that a *reality-accordant* reading (s_s in eq.10) was actually non-real and thus also calculated. The MuJHA roundness arguments are curiously perverse. Highly rounded s_e/g ratios (Alexandria & Carthage) are obviously not directly empirical (and in these cases aren't even s_e/g !: §F4), because in the real world, an outdoor eq.9 measurement of s_e/g will probably be as unround as eq.10 (Massalia). Summing up: MuJHA is simultaneously taking equinoctial s_e/g as directly empirical and solstitial s_s/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 Returning to MuJHA p.17's fundamental Pytheas-Massalia theory, we see that Jones' argument is two-step: Massalia L is computed via eq.4 using $M = 15^h 1/4$ and the Eratosthenes obliquity (eq.5) which MuJHA is proposing for Hipparchos,

$$\text{Massalia } L = \arctan \frac{-\cos(7\frac{1}{2} \cdot 15\frac{1}{4})}{\tan 23^\circ 51' 20''} = 43^\circ 01' 24'' N \quad (13)$$

Then, we subtract that same obliquity, and arrive at a S.Solst Z_s which is supposed to explain the "calculated" Pytheas s_s/g of eq.10 but doesn't:

$$s_s/g \doteq \tan[43^\circ 01' 24'' - 23^\circ 51' 20''] \doteq 41.713/120 \neq (41 \ 4/5)/120 \quad (14)$$

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 (§J5) in the CENTRAL attempted hit of Jones 2002E's concoction. Just one more unexplained inconsistency.

³⁶The experiment requires the gnomon's verticality checked by plumb-bob, an art preceding Pytheas by 1000s of years. Some gauge ancient's solar-data accuracy by reference to their star observations. But the Sun is immensely brighter & easier to place. The S.Solstices of Kallippus (–329/6/28 1/4) & Hipparchos (–134/6/26 1/4) were both accurate within 1⁴/4 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.)

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

³⁸Based without justification upon claimed (§F4) significance of the eq.10 ratio's precision. But how would Pytheas calculate Z_s (thereby giving him his s_e/g via tangent) by subtracting obliquity ϵ from L (MuJHA p.17 middle equation), when he didn't know ϵ without using eq.8, which requires one already to have determined Z_s , the equivalent (via arctan) of the very item supposedly being sought (s_s/g)! So MuJHA has to speculate that 41 4/5 is Hipparchos' calculation, via eq.4 using eq.5's obliquity. Yet Strabo (1.4.5 & 2.5.41) 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 $L = 43^\circ 01'$ — though we multiply demonstrate elsewhere (§H) here that the L upon which MuJHA bases his argument is actually $L = 43^\circ 04'$, which underguts 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 $M = 15^h 1/4$, MuJHA's hypothetical calculator would have (via eq.4) gotten not 41 4/5 but the *Almajest* 2.6 value, 41 2/3 (eq.14). MuJHA p.17 realizes this; so, does MuJHA then responsively dispense with the 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 Waerden 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 recomputation for $\epsilon = 23^\circ 51' 20''$ " — this, though Diller-DR's 14 fits (Tables 1&2) are all within their precision (100 stades) 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 superior³⁹ to FOURTEEN Strabo klimata (several *redundantly*⁴⁰ 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-misinterpreting 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 anciently taken as an accurate placement of Massalia. At *Almajest* 2.6, the original ms reading for Massalia's latitude is not 43°01' (as recently emended⁴¹ 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^h 1/4 klima, via **sph trig**, having no mathematical relation⁴² whatever to *empirical* eq.10.

H2 We next reveal that Massalia's *Almajest* 2.6 latitude $L = 43^\circ 04'$ was in truth elicited by an ancient computation (Eratosthenes'?) with Pytheas' empirical outdoor *transit* datum 41 4/5, as we see from the simple **arithmetic** of standard transit-reduction, using⁴³ eqs.5&10, which produces a perfect hit upon this (previously unexplained?) latitude:

$$\arctan[(41 \ 4/5)/120] + 23^\circ 51' 20'' \doteq 43^\circ 04' \quad (15)$$

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° 1/12, which (in a work whose degree-angles are all Hipparchanly rounded [as also at fn 30]) to the nearest 1°/12) is consistent with empirical eq.15's 43°04', not with calculated eq.13's 43°01'.

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 Muffia-upsetting DR-related achievement is similar to Schaefer's blindered attraction (Schaefer 2001) to depending upon the *least* reliable test (low altitude atmospheric extinction) of all those available for determining the authorship of the Ancient Star Catalog. (Schaefer 2001 was also published in the DR-banishing *JHA*.)

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

⁴¹Our thanks to Toomer 1984 p.86 n.43 for fairly and helpfully pointing out the original's δ (the Greek math symbol for 4) even while arguing against it in favor of α (Greek math for 1), since the latter explains the *Almajest* 2.6 shadow ratios *but only if one rounds to the nearest 1°/12*. (This step also crucial at fn 56.) That is, both $L = 43^\circ 01'$ and the eq.5 obliquity must be so rounded: to 43° & 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, Manitius, & 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.

⁴³Neugebauer 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 (*idem*) uses round $\epsilon = 24^\circ$ (not eq.5) to develop Pytheas' L , thereby missing our eq.15 & getting accurate $L = 43^\circ 12'$ only by chance cancellation of 16' errors (ϵ & *ssd*). (Note: *Almajest* 2.6's three s/g are consistent with $L = 43^\circ 01'$.)

I Inconsistencies' Inconsistencies & Hipparchos' Circuli

II Thus, MuJHA p.17's attempt to connect Hipparchos to $23^{\circ}51'20''$ fails both because eq.15 could as easily be (say) Eratosthenes' as Hipparchos' and because MuJHA's eq.13 relation of L & ϵ now (revised here to accord with mss-based eq.15) leads to obliquity:

$$\epsilon = \arctan \frac{-\cos[(15^h 1/4)(15^{\circ}/1^h)/2]}{\tan 43^{\circ}04'} = 23^{\circ}49'25'' \quad (16)$$

which is not Eratosthenes' obliquity. (Such inconsistencies inevitably result from bringing in scraps of disparate data from all over the place to try splatter-strafting solid work — instead of recognizing the merit of a coherent solution to an inter-related **[and uniformly unitized]**: §I2] 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, $23^{\circ}40'$, that yields a close fit to Strabo's stade figures (which are expressed in round hundreds of stades, thus to a precision of $1^{\circ}/7$). Unfortunately [?], there are some inconsistencies⁴⁴ in the numbers reported by Strabo, and one may well suspect that one or two modest changes in the intervals, through either scribal error or deliberate tampering, could⁴⁵ have introduced systematic errors which would affect the value of the obliquity best fitting the data.

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

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

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

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

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

I2 For Hipparchos' klimata, which are expressed by Strabo *entirely in stades*, Jones 2002E n.9 justifies the need for his proposed 100 *stade* shift via three *non-stade* data:

[A] The star α UMi is stated by Marinos (*GD* 1.7.4) to have been placed at north polar distance $NPD = 12^{\circ}2/5$ from the pole, which (by eq.4) for Diller's proposed Hipparchan obliquity $23^{\circ}2/3$ (eq.2) corresponds to 8700 stades, not Table 1's 8800. The Catch: Hipparchos used more than one obliquity (see discussion at §F1), the other one being (Rawlins 1982C pp.367-368 & eq.27) $\epsilon_{H1} = 23^{\circ}11/12$ (eq.6), which, if we compute with it (eq.4) for the Cinnamon klima's $M = 12^h 3/4$ yields $L \doteq 8700$ stades — thereby providing one simple and quite plausible explanation of the discrepancy. (For another, see fn 55.)

[B] Strabo 2.5.36 puts Syene at $L = \epsilon$ & $M = 13^h 1/2$, a common ancient confusion. If taken as precise, both statements were false (§B3), but Jones 2002E n.9 notes that if both are forced to be consistent, then $L \doteq 16700$ stades, 100 stades below Table 1's Syene klima. Catches: [i] If we demand both Strabo statements' consistency, then (by eq.4):

$$\text{Syene } L = \epsilon = \arctan \sqrt{-\cos(7.5 \cdot 13.5)} \doteq 23^{\circ}49'50'' \quad (17)$$

but that is not consistent with the MuJHA prong [a] argument (discussed here at §F1) which claims that Hipparchos' ϵ was eq.5 = $23^{\circ}51'20''$. (And both these values are contradicted by MuJHA's 100 stade-shift argument of §I3, which [implicitly: §I3] finds $\epsilon = 23^{\circ}47'$.) [ii] As with all of MuJHA's justifications for his 100 stade-shift), *none of the Strabo data MuJHA cites against Diller-DR are given in stades by Strabo*, whereas *all* the values accepted & used by Neugebauer 1975 pp.305 & 1313 and fitted by Diller 1934 (& our Table 1 or *DIO* 4.2 p.56) are given *explicitly in stades by Strabo*, an obvious indication that Table 1 is based on a coherent, one-source data-set.

[C] Jones 2002E p.16's 31° Alexandria latitude, derived from Strabo 2.5.38's $s_e/g = 3:5$ for that city, would by eq.1 equal 21700 stades, though (as just above at [B] item [ii]) this is not so stated by Strabo. Since this disagrees by 100 stades with Strabo's 21800 stades for Alexandria (Neugebauer 1975 p.1313), Jones claims another hit for his 100 stade-shift. Catch: Ratio 3:5 is just a *modern alteration of the actual text's* 7:5, which *isn't a shadow/gnomon ratio but a longest-shortest day ratio* (§F4 or Neugebauer 1975 p.336 n.29). Informed of this, Jones now (2009 April) brushes off the whole issue as minor.

I3 Jones 2002E p.17: this 100 stade shift would "affect the value of the obliquity best fitting the data". (Yes, and it would produce [§J3] a much worse rms than Diller-DR, thereby ruling-out the proposed shift.) How genuine & (Schaefer 2002) "premiere" is a journal that would carelessly-lazily publish such deliberate trashing of a precise & epochal discovery, "without bothering to test said fit"? And without asking Jones if he even knew how to run such a least-squares test? (*JHA* attitude: if an author is an archon, why referee him?) For Jones' eq.5 ϵ & 100 stade shift, **8 of 13 klimata fail**, a worse score than Neugebauer's! *Thus Jones produces no table & never tells anyone where to find DR's*. Jones repels unwelcome evidence as amusingly as the kook Doc-Cook Society (see *DIO* 9.3 §C7): e.g., if he rejects the data which Diller-DR have fit, then: *why can't he cite the best-fit table?* If the Strabo data-set (Table 1) is altered by Jones' mere 100 stades (less than $9'$), *no choice of obliquity can satisfy it. That's how hard it is to thread a curve through these data. Yet Diller-DR's solution produces a flawless fit to them*. For Jones 2002E n.9's 100 stade-shifted klimata data-set, the best fit is for $\epsilon = 23^{\circ}.778$ or $23^{\circ}46'.7$, a figure *nowhere stated* by Jones 2002E (perhaps because this prong [b] ϵ contradicts prong [a]'s eqs.5&7: §E7). After all, $\epsilon = 23^{\circ}46'.7$ is: [i] unround; [ii] "has disappeared entirely from the tradition and is not attested" (selectively echoing Neugebauer 1975 p.734's attack on Diller 1934); & [iii] has (unlike Diller's eq.2: §J5) no independent support. And even this best-fitting ϵ value is ruled out statistically (§J3), and will (if used in eq.4) nonetheless fail for four klimata of the

scheme didn't give $M = 12^h$ 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 Hipparchan obliquity (eq.2): see Fig.1.

fourteen that Diller-DR solves all fourteen of. If we try the Eratosthenes obliquity (eq.5) of Jones 2002E's prong [a] attack and compute via eq.4, the results disagree with about 60% of §I1 prong [b]'s proposed 100 stade-shifted klimata data-set. These unevadable 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 $\epsilon = 23^\circ 51' 20''$ & look for the best-fitting A , it's 158 stades (not Jones' 100): impossibly far from the unrestrained best fit we are about to locate (eq.18) in ϵ - A 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^{-9} for 200 stades.

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

J1 But these are trifling odds compared to those against adopting Jones' 2 prongs simultaneously (§I3): $\epsilon = 23^\circ 51' 20''$ (prong [a]) & $A = 100$ stades (prong [b]). For this remote position in ϵ - A space, $P < 10^{-13}$ (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 (§J3) and corrected-for; [2] the L -vs- M curve would still (see Fig.1 & caption) show a suspicious tendency to track the sort of curve produced by sph trig with a Hipparchan & accurate obliquity. Which vindicates Diller, though this important point is (ungenerously: www.dioi.org/biv.htm#ncmf) left unstated by *JHA*; so how is MuJHA a refutation of Diller's essential discovery⁴⁸ 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 §I1.) 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 (§J2) 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 folly because it (unlike sph trig) "failed to show how Hipparchus could have found a sequence matching so accurately the theoretically correct latitudes". (Which Diller 1934 had done, heretofore to Muffiosi's arbitrary non-pleasure.) We run a least-squares fit (no roundings) of the function, $\arctan[-\cos(15M/2)/\tan \epsilon] + A$, upon the M & L data of Table 1, to check the fit of eq.4 simultaneously with Jones' 100-stades-shift proposal, thus treating obliquity ϵ (eq.2) AND Jones fudge-factor A as unknowns. Formal results:

$$\epsilon = 23^\circ 37'.6 \pm 3'.2 \quad \text{and} \quad A = -28 \pm 44 \text{ stades} \quad (18)$$

Jones 2002E n.9⁴⁹ haggle-adduces disparate Hipparchan data (having nothing to do with Table 1's coherent data-set) to come up with his $A = 100$ stades (which is c.9'). But the foregoing best-solution equation shows that Jones' +100 stades is statistically ruled out, since his A (like even the most helpful ϵ [§I3] adjusted for it) is several standard deviations distant from the A & ϵ (eq.18) which minimize the residual-sum, with probability $P \doteq 1/70$. I.e., we can find A mathematically.⁵⁰ (A non-fictional *JHA* referee would have known

⁴⁸ Were MuJHA's hypothetical data-set actually in Strabo, an uncommitted explorer-scholar would test statistically and would soon find (eq.18) that removing $A = 100$ stades would produce a data-set neatly fitting L values calculated via eq.4. I.e., math-analysis cures corrupt data better than guessing-around. See, e.g., the restored Pliny circuli (fn 50), which (before Rawlins 1985G) had been universally regarded as useless. (A view time-warply echoed at Jones 2002E n.11. Neugebauer 1975 p.748 even fantastically treats the circuli as "a telling illustration for the absence of any scientific organization in antiquity".) Yet, by minimal reconstruction, Rawlins 1985G has shown that the Pliny circuli are a clever, unexpectedly precise linear fit to a sph trig klimata table based on Diller's ϵ (eq.2) and are thus one of more than a half-dozen post-1934 findings (§J5) that back up his $23^\circ 2/3$. (See *DIO* 5 §D3.)

⁴⁹ Jones 2002E n.9 credits Muffia-don Neugebauer with reconstructions actually 1st published in Diller 1934 (cited in MuJHA's previous endnote). Again: alert refereeing would've spotted that.

⁵⁰ The DR solution (fn 47) of Pliny's "circuli" klimata can be similarly grounded in mathematical analysis rather than speculation. If one computes obliquity ϵ for each of Pliny's firm klimata from the

that and tested for A .) By contrast, Diller's solution ($\epsilon = 23^\circ 2/3$ and $A = 0$) 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^y 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 ϵ - A plane, probability⁵³ P is the integrated volume exterior to the locus of points whose pd equals that of the point in question:

$$P = e^{-\frac{S-S_m}{2\sigma^2}} = e^{-(N-2)D/2} = e^{-FD/2} \quad (19)$$

where S = square-residuals sum there; S_m = best-fit S ; σ = single-datum standard deviation; sums' relative difference $D = (S - S_m)/S_m = S/S_m - 1$; N = no. of data; F = degrees of freedom (= N minus the number of unknowns, that being 2 in this case). For the Princetivite 4-dimensional case (fn 7): $P = (1 + FD/2)e^{-FD/2} = 10^{-518}$.

J5 A general observation: the MuJHA paper (which never remotely approaches supplanting Diller's well-founded improvement of our knowledge of antiquity by arriving at a comparably coherent vision: fn 55) omits mentioning any of the five then-known⁵⁴ published post-1934 confirmations (now *seven*: *DIO* 5 §D3) of Diller's ϵ and data-fit.

data he gives, the mean is $24^\circ 07' \pm 5'$, disagreeing with Hipparchan ϵ . But, after shift-restoring (Rawlins 1985G p.263) the M by -1° or -4^m (an amount explained at fn 47), we find the corresponding mean for the reconstructed data is $23^\circ 37' \pm 2'$, statistically consistent with the now-thoroughly-established (fn 48) Hipparchan ϵ of eq.2 — and fitting this ϵ 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 ϵ 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 $14^h 1/2$, the traditional Rhodos klima M . The DR reconstruction also allows us to recover (Rawlins 1985G p.263) the circuli's origin: using eqs.2&4, we find for Pliny's Alexandria klima ($M = 13^h 56^m$) $\tan L = 34^p 17'$; and for his Rhodos klima ($M = 14^h 1/2$) $\tan L = 44^p 00'$. Continued-fraction analysis (or mere familiarity with 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 circuli's original linear equation ($\tan L = [30M - 358]/105$) 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: Alexandria and 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.

⁵¹ The Diller-DR solution is superior *even to the best-fit solution* (eq.18), which fails for one klima: the L for $14^h 1/4$ is a non-match. By contrast: though the Diller solution ($\epsilon = 23^\circ 2/3$ & $A = 0$ stades) 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.**

⁵² 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 longterm-worse than the latter; but, following exile-decree, a shunned scholar's fertility may produce unanticipated ideas, the blanket (knee)jerk-condemnation of which can monotonically evolve into requiring an unexpectedly laborious and complex fear&smear campaign, to maintain perpetual suppression of a heretic — in order that a debate-fleeing cover-operator mogul's decree sticks. See fn 56, Rawlins 1991W §§D4&H2, and www.dioi.org/cot.htm#vskc.

⁵³ The matrix relating ϵ & A 's stdevs & correlation to σ is diagonalized by similarity transformation (50° rotation) ensuring separation of variables. One new variable's sd is 10 times the other's, but normalization creates isotropic pd , simply integrable via standard cartesian→polar transformation to yield eq.19. (Details: www.dioi.org/biv.htm.) The proposed process (applicable to all such Gaussian bivariate problems) is valid because the exterior volume's fraction of the whole is unaltered by the transformations. The rightmost form of eq.19 also equals normalized pd , for any number of unknowns.

⁵⁴ Some sources are cited (n.1), though key evidence discussed here is not. Arguments of Diller 1934 & Rawlins 1982C for Hipparchos' sph trig & $\epsilon = 23^\circ 2/3$ are cited at MuJHA n.8, but there's no mention of $23^\circ 2/3$'s additional confirmation by Nadal & Brunet 1984, 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 (§F1) adopted parameters (e.g., ϵ , 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 (informally) gratuitous. MuJHA can't match Table 2's 14-fold match with anything like, & *the prime datum brought against Diller's obliquity doesn't even fit*, so (§G2) MuJHA p.17 alibis: "tiny errors in [H's] calculation . . . might result from [trig] imprecisions". Wouldn't real reffing note that the Diller-DR Table 1 asked for no such leniency for its then-**dozen** perfect H-trig-calculation fits of eqs.4&2 to Table 1? Does Occam's Razor mean *anything* anymore? (Further at MuJHA p.17: for $\epsilon = 23^\circ 51' 20''$ [sic], the resultant $M = 15^h 1/4$ klima's $L = 30100$ stades, differing by 200 stades [not MuJHA's 100] vs Table 1.)

J6 MuJHA doesn't cite *DIO* at all. Now, since the newly-discovered and very strongest case (here at Table 1) for Diller's matches was published at *DIO* 4.2 p.56 Table 1 (a table in which 15^y of determined, evidently-unanimous Muffia opposition has found no errors), and since the 2002 MuJHA paper's timing suggests that it was concocted specifically to counter 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 DR's

Rawlins 1982C (MuJHA n.8) while flouting the undetailed Pliny-circuli confirmation of $23^\circ 2/3$ at Rawlins 1982C p.368 (ignoring its eq.28's extra evidence for $23^\circ 2/3$), and [b] scoff at Pliny's precise circuli as "crude" (MuJHA n.11, thereby sneering at Longitude Zero-refereed Rawlins 1985G), then competent *JHA* refereeing would require citation of Rawlins 1985G pp.262-263 where the circuli are found (fn 50) consistent with a cleverly&accurately derived linear fit to a klimata table computed by sph trig via $23^\circ 2/3$. Cornered again (as at §I1), Authoritative-in-His-Own-Mind Jones can only effect required degradation of a lovely DR fit by decreeing as unreliable THE DATA, not his own pre-judgement. Jones 2002E's implicit proposal: putatively-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' unfunniest crankprank since his Winter Equinox (Rawlins 1991W §B4).

⁵⁵ Hipparchos' observed α UMI $NPD = 12^\circ 2/5$, Jones converts to 8700 stades. *But neither Hipparchos nor Strabo did so.* MuJHA complains that a star's 8700 doesn't equal the Cinnamon klima's 8800 (Strabo 2.5.7&35). (He thinks Hipparchos believed each klima had a bright star's NPD sitting right on it?!) *This* is what co-triggers MuJHA n.9 to *urge lowering all Strabo L by 100 stades?* When MuJHA appeared, DR phoned Jones to stress (§I3): *no ϵ satisfies this hypothetical new set.* Unlike Diller's $23^\circ 2/3$ (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). Sad to find in the work of one who, despite erratic judgement (prior Jones Muffiose mess: Rawlins 1991W), has made *additions* to said sum (e.g., *DIO* 11.1 ‡1 §D1, *DIO* 11.2 ‡2 p.30, Rawlins 2008S fn 23 & p.58, *DIO* 9.1 p.2); as has *JHA* (§I1 §E1; www.dioi.org/fff.htm#cskv); & Evans (www.dioi.org/cot.htm#gjne, ggg.htm#vppp). MuJHA yields nought but chaotic (e.g., §E6 item [c]) *muddying of others' achievements.* Was this its cultish destructive aim? **Unrefereed** Jones 2002E *deems worthless all DR refereed finds touched-on:* $L = 31^\circ 04'$ (§F2), circuli (fn 54), klimata (fn 44), refereed by *Isis* (1982), Greenwich (1985), & *Isis* (2002), resp. *DIO* 1.2 §H2 [g] 1991 prediction of Muffia 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 (eqs.2&4 here) Diller's klimata scheme (Table 2), *though unnoticed by Diller.* Textbook fruitfulness. Yet MuJHA n.9 tries adducing 8800 *against* the Neugebauer-Diller data-set (above & §I2: $8700 \neq 8800$) while following Neugebauer 1975 pp.305 & 335 n.23 in nonciting attested 8800's *exact confirmation of Diller.* Though DR was 1st to publish the 8800 match to Diller, 8800's possible relation to $12^h 3/4$ was initially pondered by Neugebauer 1975 p.335 n.23.

⁵⁶ Curious practice: try refuting a discovery (Diller 1934) that's been updated with a remarkably better confirmatory 1994 hit-score (*DIO* 4.2 p.56 Table 1, or Table 1 here) *without citing the update.* Likewise, *JHA* Assoc.Ed Evans 1998 cited Rawlins 1982C, but not the revealing later *DIO* update's new clincher-evidence at Rawlins 1994L §C. (Our comments: www.dioi.org/w80.pdf p.2; & here at ‡1

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

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

K1 Jones 2002E resembles a try-anything-even-if-it-contradicts-yesterday's-dodge routine, of an anti-Occam brand already spoofed at *DIO* 2.3 ‡8 §C31. And the next Muffioso into the lists to degrade Diller will offer a different joke-defense, casting Jones' aside (just as he dumped Neugebauer's), but the rigidly prescribed common thread will be: Diller hasn't established⁵⁷ anything. Xerxes' "Immortal Ten Thousand" army faked eternalness by replacing each slain soldier with another, so we should admire the Muffia's "Immortal Ten Thousand" degrade-brigade as granting to Xerxes' eternal-life idea its own eternal life. (See perverse analogy at www.dioi.org/epi.htm#dvuv.) And so we observe (yet again) a familiar cardiac-Xerxesure at the spectacle of anti-imperial rebellion and heresy being Taken-Seriously (*Isis*: Thurston 2002 in this instance), leading to (yet again) a serial stoat-wriggle attempt to rescue (yet again) an exalted sacred cow from the jaws of the mundane spring-trap of mere evidence (*DIO* 11.3 ‡6 preface). But the Diller case is (like archoncal cows' heads) too big & too visible for escape. (And is invaluablely unambiguous: Table 1 can show even 8^y old kids [see *DIO* 4.2 pp.55-57] exactly how honest the evidence-ducking & debate-averse Muffia is.) I.e., bigfat data-trapped sacred cows just can't make or fake it as wriggle-out mink. They can only turn purple trying.

K2 **Runnin-round like Chickens with — B-But, their Heads Are Enormously ON!** Given frantic Browner-motion Muffia inconsistencies (§E6) *vis-à-vis* Strabo's klimata, we're not trying hard to resist recalling yet again the old vaudeville-comic rape-defense routine (already vainly thrown at ineducable historians-of-astronomy back in 1991's *DIO* 1.2 §I9): But I don't even know the girl; & I was nowhere near Judy that night; anyway, she consented.

K3 Every reader should consult FOR HIMSELF the stark truth of the state of the florid-visaged history-of-ancient-astronomy community's purple-cowards, as revealed by its forums' predictable (and predicted) totalitarian revulsion at our 1994 *DIO* 4.2 p.56 Table 1. The table is so devastating to said clique's insultingly stolid pretense (that Diller's finding cannot be Received by Accepted Society), that *DIO* is for the 3rd time publishing⁵⁸

fn 2&7.) But, then, few (if any) *JHA* papers have ever cited *DIO* except to attack it, since Editor-for-Life M.Hoskin's rage at the sight or mention of DR's name is well known. (This, though *DIO*'s board is patently more scientifically qualified than Hoskin's.) Among said rage's more transparent playings-out: *DIO* 6 ‡3 §G2. (Outré? No, just everyday *JHA* sanity&integrity. As here at fn 24 & ‡1 fn 7.) Compare to *DIO*'s hugely different citation-record, error-admissions, self-criticisms, and approach to dissent: *ibid* §B3, Rawlins 1991W §C11 [d], *DIO* 11.1 ‡2 preface ("Gratitude to Opposites"), *DIO* 11.2 cover, & above at ‡1 fn 10 & §C3. In a careerist world, does anyone even care whether journals are honest? **Except to steer clear of the danger of being associated with those that are.**

⁵⁷No one's demanding 100% assent, but the Muffia deliberately, cohesively ducks owning that Diller's discovery has ANY merit. Why would a cult risk its putative reputation for integrity by continuing such transparent (and transparently grabby: p.2 fn 4) dodges in such an ultimately farcical crusade?

⁵⁸Original shirt-unstuffer 1994 publication [augmented 2002]: online at www.dioi.org/vols/w42.pdf. (Republications in 2009, with Meroë resolved: *DIO* 5 Table 0 at www.dioi.org/vols/w50.pdf; detailed table and odds: www.dioi.org/biv.htm.) Don't blame DR's sardonic style for the field's pathology. Princetituter & Muffia godpop Neugebauer's possessive shunning and Babylonianist abuses of Diller 1934 had been going on since before DR was born (indeed, for 45^y 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 wee 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 $L = 0^\circ$, & eq.3's fresh discovery of the Meroë klima's actual ancient value.) The long-overdue detailed *DIO* counter-attack against Muffiosi's 75^y 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 Princetute. As we asked at *DIO* 4.3 ‡14 regarding the now-mercifully-dead Ancient Star Catalog controversy: "To yet continue stubbornly flying in the face of [in the present instance a long-accumulating multiplicity of consistent evidences: Diller 1934 → *DIO* 5 §D3 [1]-[7] → Table 2 here] is to carry unfalsifiability to kook dimensions — and to raise the question of whether it is worth discussing historical issues at all. (Of course, one may easily understand why certain moguls might wish to render reason and competence irrelevant to the evolution of ideas in [the hist.astron] field.) For, if even the most logically & evidentially one-sided controversies are to be decreed [see NCS at *DIO* 2.3 ‡8 §§C20&C25] as indefinitely irresolvable, then — why investigate anything?" When this quote was applied to a few Velikovskians at *DIO* 7.1 ‡5 fn 40, no objection was made by historians. But, can they show equanimity when the same principle is found applicable to eminent personages of their own profession?

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

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