K Old Turkey: the Mystery of Hipparchos' Roots

K1 We have no direct biographical note indicating when Hipparchos of Nicaea, Bithynia (now northwest Turkey), moved to Rhodos, which is where his extant outdoor[184] celestial observations were made. But we have several clues regarding this and other aspects of Hipparchos' pre-Rhodos career. Before his serious observing began at Rhodos in -146, the only Hipparchos celestial observations cited by Ptolemy are a much-earlier threesome of Autumn Equinoxes -161, -158, -157 (*Almajest* 3.1). One immediate oddity here: why no matching Vernal Equinoxes? Possible explanation (other than better Autumn weather): throughout this era, the AE happened (by chance) to be so close to Thoth 1 of the Egyptian calendar (Rawlins 1991H fn 7) that the AE was naturally of special calendaric interest. A far more striking peculiarity: since Hipparchos was the prime source of Ptolemy's klimata-based latitudes L (*Geogr Dir* 1.4.2, Rawlins 1985G p.261), it is remarkable that the L values for Hipparchos' native Byzantion-Bithynia region are off by about $+1°1/2$ to $+2°$. These are huge errors, and they are impossible to make even with the common crude gnomon. (This instrument consistently misleads[185] the observer, but only by about $-1°/4$ in deduced latitude.) The gnomon was most probably (fn 195) the instrument used to record the systematically late 162-158 BC Autumn Equinoxes reported by Hipparchos. (Each of these equinoxes was late by a fraction of a day. But an observer whose latitude was off by $2°$ would be expected to record an equinox wrong by c.5^d.) Therefore: these 162-158 BC A.Equinoxes were not observed in Bithynia. (Otherwise, Hipparchos would have gotten the L there correct within about $1°/4$, just like the equinoxes.)

K2 Since 1979, DR has proposed that Ptolemy's amazingly large $+2°$ error in the L of Byzantion (modern Istanbul) was initiated by Hipparchos' use of a primitive solstitial ortive amplitude of atn(2/3) = $33°41'$. Exactly this value is anciently attested for Byzantion (Neugebauer 1975 p.983); and, by ancient sph trig formulas,[186] this would lead to longest-day M = $15^h1/4$ and L = c.$43°$ (*Geogr Dir* 8.11.7, 3.11.5 & *Almajest* 2.6, resp). (Actual Byzantion $L = 41°01'$.) Hipparchos' nearby native city, Nicaea (modern Iznik), is listed with $M = 15^h1/8$ & $L = 41°11/12$ (*Geogr Dir* 8.17.7 & 5.1.14, resp; Rawlins 1985G p.262). (Actual Nicaea $L = 40°26'$; error $+1°29'$.) These considerations suggest that Hipparchos never advanced beyond horizontal astronomical observations until after leaving Bithynia. And this almost has to be the case, since the slightest vertical-instrument celestial work by Hipparchos would have revealed his $2°$ error in L.

K3 As for the 162-158 BC equinox trio (so inconsistent with this §K2 error): Muffiosi doubt[187] that this trio was observed by Hipparchos. However,

[a] Hipparchos says (*Almajest* 3.1) that they were observed with great care.[188]

[b] They are uncited to another party.

So, I will suppose (very uncertainly) that they are his own observations, made with a gnomon — before he moved up to a transit circle (used for the equinox observations made from 147 BC to 128 BC: *Almajest* 3.1). If the 162-158 data are his, then (§K1) Hipparchos had left Bithynia by 162 BC. Given the long, data-bare 11^y break between 158 BC & 147 BC, one may speculate that the 162-158 BC trio was taken by Hipparchos (mainly for calendaric

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[184] Neugebauer 1975 p.275 speaks of Hipparchos' observations in Bithynia (Heiberg 1907 p.67), but these are mere weather astrology. They are not evidence that Hipparchos made any serious astronomical observations in Bithynia. See Neugebauer 1975 pp.277 & 928.

[185] The standard ancient vertical gnomon will read the solar altitude high by c.$16'$, the solar semidiameter. This will affect an equinox time by about 2/3 of a day. See, e.g., Manitius 1912-3 1:419-420 or Rawlins 1982G.

[186] Neugebauer 1975 p.37 eqs. 4a, 5a, and-or 6. For probable Bithynian interest in ortive amplitudes at about Hipparchos' time, see *ibid* p.766.

[187] Toomer 1980 p.103 fairly notes that there is no proof, but stresses that, besides the early AE three, there are no Hipparchos observations known from that time. See also Toomer 1978H p.208, Toomer 1984 p.133 n.7, Goldstein & Bowen 1989 p.289 n.1, & Sarton 1959 p.285.

[188] The remarkable classicist-turned-astronomer J.Fotheringham stresses this hint. And Neugebauer 1975 (p.276 n.20) sneers at him for doing so.

purposes: §K1), during a stable 4^y geographical way-station in his career, before he later — presumably sometime not long before the 147 BC Autumn Equinox — settled on Rhodos (for the remaining decades of his life), and started[189] to set up sophisticated instruments there. But, regardless of when it was that Hipparchos commenced making regular outdoor observations, he (a prominent astrologer) would have required possession of a solar theory, in order to compute his extensive eclipse catalog (§M7).

K4 In any case, let us pose a question: if Hipparchos developed a solar orbit early on (before −146), based (in the standard Greek fashion of *Almajest* 3.4) upon VE, SS, & AE observations, then what 3 data would he use? Regardless of our guesses as to his peregrinations, the answers are all easily induced from *Almajest* 3.1, where we learn: [a] Hipparchos' AEs before −146 were explicitly listed by him (*Almajest* 3.1 & §K3). [b] He was a student of the Vernal Equinoxes observed annually on the large bronze krikos in the "Square Stoa" at Alexandria (see Toomer 1984 p.133 n.7). [c] Hipparchos dated all his equinox-solstice observations by the calendar of Kallippos (*Almajest* 3.1) — a calendar in which each year started with a Summer Solstice, an event computed as occurring an integral number of Kallippic years (exactly 365^{d}1/4 each) after the Kallippic Cycle epoch,[190] Kallippos' accurate (near-syzygial) −329/6/28 Summer Solstice observation,[191] which DR has already earlier independently reconstructed as having been recorded (by Kallippos) for dawn. (See Rawlins 1985H, which notes that this solstice occurred, exceptionally, less than an hour before a New Moon — which made it a particularly ideal[192] choice for the foundation of a luni-solar calendar.) The current paper confirms[193] this Rawlins 1985H reconstruction, on the nose.

K5 Items [a]-[b]-[c] of §K4 are about to supply all 3 of the foundation stones of Hipparchos' longlost earliest solar orbit (§K9).

K6 Before −146, we have (*Almajest* 3.1) only 3 Hipparchos solar observations: the AEs of −161, −158, −157. The −161 datum is discordant, and each of these 3 early data disagrees[194] with each of the later AE set (−146 to −142, *Almajest* 3.1). (The likely cause of the disagreement: the 3 early AEs were probably observed via gnomon.)[195] So, for the period from −157 to −146 (the early part of Hipparchos' career, when his adopted solar orbit has previously been unknown to us), let us take the last (−157) of the 3 early AE data as Hipparchos' AE standard. (The −158 & −157 AEs agree.) According to *Almajest* 3.1, this AE (§K4 item [a]) was stated by Hipparchos to have been carefully observed as occurring at −157/9/27 1/2 (noon).

K7 The −145/3/24 Alexandria VE cited at *Almajest* 3.1 is reported to have occurred at 11 AM. The 12^y interval between this date and the −157/3/24 VE is divisible by 4, so (given that the year's length is c.365^{d}1/4) the Alexandria krikos observation of the −157 VE would be made under similar geometric conditions, thus it should have been recorded at roughly the same time (likely c.1 PM). Hipparchos (who always rounded such data calendarically to the nearest 1^d/4) would express this as noon. Thus, we have reconstructed Hipparchos' other equinox datum (§K4 item [b]): −157/3/24 1/2.

K8 Finally, the interval from −157 back to Kallippos' −329/6/28 1/4 epoch is 172^y, which is also divisible by 4. Thus, it is easy to see that, if Hipparchos used the Kallippic calendaric Summer Solstice (§K4 item [c]), it would have been −157/6/28 1/4, a quite inaccurate time. (See §K4 for Kallippos' calendaric year and epoch.)

K9 From these data, we have: Spring = 95^{d}3/4, Summer = 91^{d}1/4. It remains now merely to solve for the orbit which satisfies the 3 data of §K6-§K8. The math method is set forth in *Almajest* 3.4. (It is available, in modern dress, at Neugebauer 1975 p.58 or Rawlins 1991H §C6f.) The resulting orbital elements are given below (for Hipparchos' chosen tabular epoch,[196] which was −323/11/12 noon = Nabonassar 425 or Philip 1, Thoth 1 — as is obvious from *Almajest* 3.1). We will henceforth call this orbit the "Early Hipparchos" solar theory or just the EH orbit. (DR started attacking the *Almajest* 4.11 solar orbit problem on 1991/10/27. He is ashamed to acknowledge that it was all the way to 1991/10/29 before he'd zeroed in on the krikos-Kallippos-Hipparchos-based EH orbit.) The EH elements (taking 1^p = 1/60):

mean-longitude-at-epoch $\epsilon_E = 228°$
mean motion $F_K = 360°/(365^d 1/4)$, adopted from Kallippos
apogee $A_E = 44°$
eccentricity[197] $e_E = 3^p 1/4$

K10 These elements are rounded much as one expects of ancient work (Rawlins 1985K). All 4 elements differ from those of the famous Prime Hipparchos orbit[198] (*Almajest* 3.1-7). For the PH orbit:[199]

mean-longitude-at-epoch $\epsilon_P = 227°2/3$
mean motion $F_J = 360°/(365^d 1/4 - 1^d/300)$
apogee $A_P = 65°$
eccentricity $e_P = 2^p 1/2$

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[189] If we assume, from our ignorance of any site for him except Bithynia & Rhodos, that Hipparchos had no other observation-locale, then the 162-158 BC trio were observed in Rhodos. But this leaves unexplained the 11^y gap (158-147 BC) between equinox observations.

[190] Muffia capo B.Goldstein, U Pitts Dep't Hist & Philos of Sci, denies (as part of the Muffia's downgrading of pre-Ptolemy Greek astronomy: see fn 241) that Kallippos founded a calendar (a bizarre denial, naturally disconfirmed by this paper's findings), suggesting instead that this 4th-century BC Greek's famous 4th-century-epoch calendar (which uses Athenian month-names: *Almajest* 7.3) was perhaps actually invented in *Egypt* in the *3rd* century BC. Goldstein & Bowen 1989 (p.279 emph added; see also p.285) delight in the "possibility, *never before considered*, that the dates are really Egyptian *despite the Athenian names*." (In the previous sentence, the superficial notion that the Athenian names are Athenian is called "a move of desperation": see fn 192.) If only Dr.Velikovsky were alive to enjoy & chirp-in on all this.

[191] The date is correctly induced by van der Waerden 1984-5 p.120, acknowledging Fotheringham's priority. Yet Goldstein & Bowen 1989 (p.279 & nn.34, 36) scoff at van der Waerden's belief that Kallippos observed the −329 S.Solstice, adducing Neugebauer 1975 pp.617-618 in support — a clear case of these co-authors' Collective Amnesia (see also §F1), since, only 10 pp later, Neugebauer 1975 (p.627) rightly states that Kallippos did indeed observe S.Solstices (see *DIO 1.1* ‡5 fn 13).

[192] Ignoring Rawlins 1985H, Goldstein has no inkling of the specialness of the proximity of the S.Solstice to the New Moon in −329. Though Alexander's regnal year is (since he conquered Persia during Nab 417) universally attested as starting −331/11/14 (Toomer 1984 p.11), Goldstein & Bowen 1989 (pp.274, 279, 284) try to convert the nonregnal year −329 into his regnal year anyway! (Velikovsky was also famously gifted in the Revolutionary-Chronology Dep't. See "Worlds in Collusion", in I.Wallach *Hopalong Freud & Other Parodies* Dover ed 1966 p.25.) It was, they plead, "the year in which Alexander assumed the title of Great King." Recall that it is B.Goldstein, of all reputed scholars, who ritualistically resorts to the phrase "move of desperation" (fn 190) whenever he encounters another scholar's inconvenient reality, standing in the way of his desire-fantasies.

[193] §K8, §K11, & §L3.

[194] Discrepancies here refer to comparisons with Kallippic motion F_K (§K9), which is an adequate & useful approximation (to the truth) for a few years of data.

[195] The $+12'$ declination mean error ($+12^h$ time) of the three 162-158 BC Autumn Equinox data agrees with the $+12'$ error of Hipparchos' early obliquity 23°55′ (Rawlins 1982C & Rawlins 1985G). Note: Hipparchos' use of a gnomon for solar altitude, and adoption of a polestars-based geogr latitude L, would explain most of these three AE errors — as well as the obliquity error, if we assume that the gnomon-measured S.Solstice zenith distance was subtracted from this L to find the obliquity. (It appears that adopted L was low by a few arcmin.) Gnomon-use indicates that Hipparchos' early empirical work was subprofessional. The suggestion here is that the first Hipparchan obliquity, 23°55′, was measured by the same party (probably himself) who observed the 162-158 BC equinox data.

[196] DR has long proposed (Rawlins 1985K) Phil 1 as the formal epoch of Hipparchos' solar tables, up until his Ultimate (UH) solar theory (Rawlins 1991H). Phil 1 was also presumably the formal epoch of Aristarchos' ephemerides (eq. 8). Likewise for Kallippos, since his work was published very near Phil 1.

[197] The *Almajest*'s usual rounding of eccentricity is to a quarter of 1^p. (Exceptions: Saturn & the Moon.)

[198] I must point out my own stupid, unexamined temporary-belief, at Rawlins 1991H fn 16, that trios A&B were computed from the PH orbit — a footnote based entirely upon my cursory, overCartonesque glance at a table (R.Newton 1977 p.126) which merely listed Ptolemy's recomputed (not Hipparchos' original) longitudes for these eclipses. (The error is wholly mine, not the late RN's.) Some of the latter part of *DIO 1.1* ‡5 fn 7 isn't worth much, either. (Looking for the positive side: perhaps future historians will appreciate the enormity of the gulf between those superficial notes and the current paper, and how little time elapsed between them.)

[199] Ptolemy computes A to the half-degree from Hipparchos' data and gets 65°1/2. (And *Almajest* 3.4 explicitly states that this was Hipparchos' value.) I don't wish to guess whether: Hipparchos or Ptolemy or either was the

K11 The ancients took mean solar anomaly g with respect to the apogee A:

$$g = f - A \quad \text{so} \quad g_{\mathrm{E}} = f_{\mathrm{E}} - 44^\circ \tag{2}$$

where f = mean longitude. (Throughout the remainder of this paper, A will refer to apogee-at-epoch.)[200] The EH orbit's equation-of-center E_{E} is now determined (see Rawlins 1991H eq.22):

$$E = -\arctan\frac{e\cdot\sin g}{e\cdot\cos g + 1} \quad \text{so} \quad E_{\mathrm{E}} = -\arctan\frac{\sin g_{\mathrm{E}}}{\cos g_{\mathrm{E}} + 240/13} \tag{3}$$

and the true longitude ϕ is:

$$\phi = f + E \quad \text{so} \quad \phi_{\mathrm{E}} = f_{\mathrm{E}} - \arctan\frac{\sin g_{\mathrm{E}}}{\cos g_{\mathrm{E}} + 240/13} \tag{4}$$

where

$$f = \epsilon + F\cdot d \quad \text{so} \quad f_{\mathrm{E}} = 228^\circ + 360^\circ\cdot(d/365^{\mathrm{d}}1/4) \tag{5}$$

(ϵ = mean-longitude-at-epoch; d = days since epoch). To a precision of about $1'$, the EH orbit fits the above three proposed foundation-solar-positions. (The appropriate dates are cited at §K6-§K8. The d are given at fn 201.) Longitudes computed[201] from eqs. 2-5 (applied to the elements of §K9) are: $0^\circ 01'$ for VE (-157/3/24 1/2), $90^\circ 00'$ for SS (-157/6/28 1/4 or Kallippos' -329/6/28 1/4), $180^\circ 01'$ for AE (-157/9/27 1/2). This is a more than satisfactory fit.

L Hipparchos' Eclipse Trio B Reveals His Early Solar Orbit

L1 Armed with the EH orbit (trig), we now return to consider the supposedly-impossible (§G4) task of finding a Greek-style (trig) solar orbit that will explain the solar position-intervals Hipparchos used for his analysis of eclipse trio B (*Almajest* 4.11). Will the EH orbit do the trick? The intervals to be satisfied are (*Almajest* 4.11):

B2−B1: $180^\circ 20'$, $178^{\mathrm{d}}06^{\mathrm{h}}$
B3−B2: $168^\circ 33'$, $176^{\mathrm{d}}01^{\mathrm{h}}1/3$

L2 First, we find from these two time-intervals (§L1) and from the *Almajest* 4.11 discussion, the absolute times Hipparchos probably used for the 3 eclipses: B1, B2, B3. These times are:

B1: Nab 547 Mesore [12] 16 07^{h} = -200/09/22-23 19^{h}.
B2: Nab 548 Mechir [06] 09 13^{h} = -199/03/19-20 01^{h}.
B3: Nab 548 Mesore [12] 05 $14^{\mathrm{h}}1/3$ = -199/09/11-12 $02^{\mathrm{h}}1/3$.

party who altered the original $A = 65^\circ$ to $A = 65^\circ 1/2$. Note: both here-reconstructed A values of Hipparchos (EH & UH) are in whole degrees, which is reasonable, given the looseness of A's empirical determination. (Ptolemy's sometimes amusing overprecision is also evident at *Almajest* 3.1 & 4.7.) I note that the math of Neugebauer 1975 p.58, arcsin($2^{\mathrm{p}}16'/2^{\mathrm{p}}30'$), yields 65°, not $65^\circ 1/2$ as Ptolemy gets from arccos($1^{\mathrm{p}}02'/2^{\mathrm{p}}30'$). Without computationally checking a thing, Neugebauer 1975 p.58 simply forced his result to be the "right" answer, anyway — i.e., Ptolemy's $65^\circ 1/2$. For similar Muffia inventiveness, see fn 38. Muffiosi are unarguably the perfect cult to defend astronomy's most notorious faker.

200 Hipparchos-Ptolemy's solar apogee is constant, while the lunar apogee moves at a constant speed.

201 Taken from epoch Phil 1 = -323/11/12 noon, the d are (for the -157 orbit-foundation dates): d_1 = 60398d (VE), d_2 = 60493d3/4 (SS), d_3 = 60585d (AE).

L3 Next, we compute the solar longitude for each of these times. (Note: all times for computing the Sun's place may be rounded to the nearest hour — though that's not necessary for d_1 here — because the worst possible error caused thereby will be barely $1'$.) The process is, effectively: substitute the time-data just given, along with the elements of §K9, into eqs. 2-5. We will start by finding trio B's f_1 from figuring the time d_1 elapsed since epoch Phil 1 = -323/11/12 Alex noon (Toomer 1984 p.11, using $1^{\mathrm{E}} = 365^{\mathrm{d}}$ = 1 Egyptian year): $d_1 = 44875^{\mathrm{d}}7/24 = 122^{\mathrm{E}}\ 345^{\mathrm{d}}\ 07^{\mathrm{h}}$. However, when using eq. 5 to find mean longitude, we may easily forget that ancient computation was tabular, not electronic. And ancient tables (e.g., *Almajest* 3.2) were formed in units of 18^{E}, 1^{E}, 30^{d}, 1^{d}, & 1^{h}. Thus, d_1 must be broken into: 108^{E}, 14^{E}, 330^{d}, 15^{d}, 7^{h}. Tables founded upon mean motion F_{K} (§K9) will provide the corresponding arcs, which we add to ϵ (§K9) in order to find trio B's f_1. & ϵ_{E} of §K9.) The sum: $f_1 = 333^\circ 23'\ (108^{\mathrm{E}}) + 356^\circ 33'\ (14^{\mathrm{E}}) + 325^\circ 15'\ (330^{\mathrm{d}}) + 14^\circ 47'\ (15^{\mathrm{d}}) + 0^\circ 17'\ (7^{\mathrm{h}}) + 228^\circ\ (\epsilon) = 178^\circ 15'$. An experienced ancient mathematician would then save labor (see also §N12) by proceeding henceforth merely differentially: just find the tabular mean-motion arc for the time-interval elapsed from eclipse B1 to eclipse B2, stated (§L1, *Almajest* 4.11) to be $178^{\mathrm{d}}06^{\mathrm{h}}$ or $150^{\mathrm{d}} + 28^{\mathrm{d}} + 6^{\mathrm{h}}$. Similarly for the interval between eclipses B2&B3. Thus, summing F_{K}-based tabular entries, we have: $f_2 - f_1 = 147^\circ 51'\ (150^{\mathrm{d}}) + 27^\circ 36'\ (28^{\mathrm{d}}) + 0^\circ 15'\ (6^{\mathrm{h}}) = 175^\circ 42'$; and $f_3 - f_2 = 147^\circ 51'\ (150^{\mathrm{d}}) + 25^\circ 38'\ (26^{\mathrm{d}}) + 0^\circ 02'\ (1^{\mathrm{h}}) = 173^\circ 31'$. Collecting & summing our results,[202] we have: $f_1 = 178^\circ 15'$; $f_2 = f_1 + 175^\circ 42' = 353^\circ 57'$; $f_3 = f_2 + 173^\circ 31' = 167^\circ 28'$. From this point, the computation is standard (eq. 4): mean longitude f plus eq-ctr E_{E} (eq. 3) = true longitude ϕ; we round to the arcmin as we go, and then express the sum rounded according to normal ancient custom:[203]

B1: $\phi_1 = 178^\circ 15' - 2^\circ 19' = 175^\circ 56' = 175^\circ 11/12$
B2: $\phi_2 = 353^\circ 57' + 2^\circ 18' = 356^\circ 15' = 356^\circ 1/4$
B3: $\phi_3 = 167^\circ 28' - 2^\circ 40' = 164^\circ 48' = 164^\circ 4/5$

The longitude intervals B2−B1 and B3−B2 are, then: $180^\circ 20'$ and $168^\circ 33'$ — which precisely[204] accord with the stated intervals (*Almajest* 4.11, Jones 1991H p.106 Table 1 last column, or above at §L1). A gratifying outcome, since the orbit used has been founded — independently of the *Almajest* 4.11 Hipparchan intervals[205] — from equinox-solstice solar

202 Had we proceeded double-differentially here, as at §M10, f_3 would have come out = $167^\circ 27'$; but this would not have affected the final result at §L3, since $\phi_3 = 164^\circ 47'$ anciently-rounds to $164^\circ 4/5$.

203 For consistency, same rounding procedure is adopted for these calculations as at Rawlins 1991H §D9 — & later here for trio A (§M10).

204 The 3 perfect matches are based on the tiny ancient-convention roundings adopted. (Compare to both of Jones' extremes: fn 209 item [b]!) But, even forgetting the last column, the greatest discrepancy is $1'$, which is about the uncertainty attendant to interpolation from an ancient table for the equation-of-center (e.g., that of *Almajest* 3.6). I.e., all considered, the agreements found in this paper are a good deal closer than are really necessary to establish the propositions broached. See also fn 205. And I would point out the long odds against the neat (ordmag $1'$) explanation of trio B's data by an orbit (EH) founded upon 6h-rounded data! The same remarkable fit occurred (Rawlins 1991H) for the trio of *Almajest* 5.3&5. (The *a priori* odds against these fits remind one of the peculiarity that Ptolemy's four 1h-precision solar fakes are founded upon 6h-precision Hipparchos & Meton data: fn 64.) If one alters merely by an eighth of a day (3h) any one of the EH foundation-times (-157 VE, SS, AE), then the orbit deduced from these will fail to satisfy the trio B intervals by ordmag $0^\circ.1$, which would utterly destroy the match.

205 If one adopts the times of §L2 and the 1st two elements of §K9 ($\epsilon = 228^\circ$ & F_{K}), and solves analytically for the eccentricity e & apogee A, then: the exact-fit result for the *Almajest* 4.11 trio B intervals ($180^\circ 20'$ & $168^\circ 33'$) is: $e_\circ = 3^{\mathrm{p}}10'$ & $A_\circ = 46^\circ 1/5$. I.e., using these elements in Greek trig-based orbit calculations will accomplish just what JHA-Muffia wisdom has called The Impossible: reproducing, on the nose, the *Almajest* 4.11 intervals: $180^\circ 20'$, $168^\circ 33'$, & their sum $-11^\circ 07'$. The square-residual sum S for all 3 intervals is obviously null for this case. (Of course, only 2 of these 3 intervals are independent data.) Defining (with consistent units) $x = e\cdot\Delta A = e\cdot(A - A_\circ)$ and $y = \Delta e = e - e_\circ$ and mapping S on the x-y plane, we find that the foregoing ideal solution is only trivially better than the EH solution (§K9), contrary to what one might at first suppose. For, the near-parallelness of the 3 eclipses' radius vectors produces (in the x-y plane) a long, gradual-curvature paraboloidal minimum (in S), whose constant-S cross-sections are nearly elliptical, with narrowness (ratio of semi-major-axis a to semi-minor-axis b) $\approx$ 23/3 or 7 2/3. The major-axis is virtually a locus or long groove of solutions that adequately satisfy the trio B intervals.

data which are the most reasonable choices we could induce for the young Hipparchos (§K).

L4 But: hold on there! The Editor-for-Life's extremely infallible *JHA*, on the advice of a gaggle of Muffia-circle archons, has decreed (Jones 1991H) that no such trig-based Greek-orbit solution is even *possible* for eclipse trio B: see the irrefutable Muffia analysis quoted above at §G4. (And note: the *JHA* co-Editor most directly responsible for publishing Jones 1991H is one whose name is printed as senior author of the extensive orbit computations published in 1983 as vol.59S of the *Memoirs* of the American Philosophical Society.) So, we must be imagining the foregoing. If Muffiosi, the greatest ancient astronomy experts in the history of the universe (just ask them), say it ain't so, believe them: it ain't so.

M Frankensteinorbit Meets Trio A

M1 Applying to trio A the same unerringly insightful Muffia strategy we have (§G4) already giggled our way through for trio B, Jones 1991H of course concludes that trio A must also be solved by Babylonian methods. But, I'm such a hard case that I'll try, yet again, the Greek approach to the Greek, Hipparchos. (No wonder Muffiosi snub DR.)

M2 Hipparchos' given intervals for trio A (*Almajest* 4.11):

A2−A1: 173° − 1°/8, $177^{d}13^{h}3/4$
A3−A2: 175° + 1°/8, $177^{d}01^{h}2/3$

M3 Now, whether we're going Greek or Babylonian here, there is a 1° or 1^{d} discrepancy in the foregoing (vis à vis a $365^{d}1/4$ calendar), as the slightest testing will show. (See fn 162!) It is precisely by refusing to face this (thus taking the intervals literally) that Jones 1991H (p.112) ends up boxed into the risible conclusion that Hipparchos used a 366^{d}calendar (§G3). Understand, Jones 1991H is a gov't-funded paper, by a protégé of gov't-funded Muffiosi, experts whom we trust to discriminate between their Received orthodoxy vs. "garbage" (fn 13) produced by "cranks" who extract "money from the government on false pretenses" (*DIO 1.1* ‡3 §D3). To a low thing like DR, who has not yet attained to the lofty plane of Muffia enlightenment, it would instead seem that there is just a 1° error here in the Hipparchos report of one of the intervals, and thus with either eclipse A1 or A3. After some testing, I have opted for the assumption that the problem is with eclipse A3, thus the A3−A2 interval (§M2) must be (as R.Newton already discovered)[206] enhanced by 1°. Adopting this correction, our revised solar interval-data are:

A2−A1: 173° − 1°/8, $177^{d}13^{h}3/4$
A3−A2: 176° + 1°/8, $177^{d}01^{h}2/3$

The locus-groove's slope in the x-y plane is: $dy/dx = -3/4 = \tan 143°$. The EH solution is $8'2/3$ distant from the minimum-pt, in the direction 146° — i.e., nearly in-the-groove. A similar situation holds for trio A, where the intervals are 173° − 1°/8, 176° + 1°/8, & their sum −11°. The trio A eclipses' radius-vector interrelations are similar to those of trio B; so, in the trio A case, the elliptical groove of valid solutions has about same narrowness (23/3). But the slope $dy/dx = +3 = \tan 72°$. The point on the x-y plane where trio A's S is null is where $e_{\circ} = 3^{p}09'$ & $A_{\circ} = 64°1/3$. Our trio A solution (§M4) is $6'1/7$ distant from this point, in direction 71°: i.e., almost precisely in the locus-groove of solutions that satisfy the intervals of §M3 within ordmag $1'$. For trio B, the EH orbit (§K9) corresponds to residual-sum (for all 3 intervals) S = 8.8 sq-arcmin; for trio A, the solution of §M4 corresponds to S = 2.7 sq-arcmin. This shows that, even without the roundings of §L3 & §M10, the rms of both solutions' non-fits is merely 1-2 arcmin (& even this largely disappears during rounding) — which, given the interpolation-uncertainty noted in fn 204, is fully adequate precision for solving the mystery of the *Almajest* 4.11 intervals.

206 After completing this entire analysis of trio A, DR noted that R.Newton 1977 p.119 (not cited by Jones 1991H) already — 14^{y} ago — independently reached exactly the same conclusion [triggered by realization of the insane fn 162 solar speed required by the uncorrected places]: the A3−A2 longitude interval (175°1/8) must be restored to 176°1/8. (Except for this groundwork point, my analysis here is not much like RN's.) So Hist.sci's Jonestown disaster could have been averted, had the Muffia not anathematized R.Newton 1977.

M4 Solving analytically[207] finds that, for the traditional (§K10) H-P value, ϵ_{P} = 227°2/3 (choice justified by matching lunar positions later on here: §N15), and $Y_{K} = 365^{d}1/4$ (§K9), the elements of the precise-fit orbit solution are (fn 205): $e_{\circ} = 3^{p}09'$, A = 64°1/3. Thus, taking into account [i] the trio B solution, [ii] Hipparchos' known PH orbit elements, [iii] ancient rounding practice, & [iv] the lunar solution to come (eq. 8, also epoch Phil 1), we induce (after some testing & checking) the solar orbit Hipparchos used for trio A:

ϵ_{P} = 227°2/3
mean motion $F_{K} = 360°/(365^{d}1/4)$, adopted from Kallippos
apogee A_{P} = 65°
$e_{E} = 3^{p}1/4$

M5 This presents us with an odd but (as we shall see presently, in §M6) surprisingly informative Frankensteinesque[208] orbit: half[209] of the elements are from EH (§K9); the other half, from PH (§K10)!

M6 But the split is not random. We note a revealing correlation: those elements that are associated with tables (namely, F and e) are EH. Those which are just epoch-constants (ϵ & A) are PH. Simple explanation: the Hipparchan school's calculation of the solar positions of eclipse trio A was done during the transition period when Hipparchos was in the process of adopting his PH orbit.[210] (Contemporary PH epoch −145/9/29 noon = Nab 603 or Pot 1 Thoth 1 noon; Rawlins 1985K.) At this point, he had determined the PH elements (§K10), but had not yet compiled the necessary associated tables. This has to have been around the PH epoch: 146 BC. Thus, by the luck of happening upon transition-period work, we've learned the date of a famous H eclipse calculation. Later (§N3), we'll find that, by the time he performed the associated *Almajest* 4.11 lunar calculations, he'd fully adopted (including contingent, prepared lunisolar tables) the PH solar orbit: §N2. This gives us the essential chronological order of the Hipparchos work described at *Almajest* 4.11.

M7 OK, comic-relief-time again. Thanks to Pliny,[211] it is wellknown that Hipparchos computed 600^{y} of eclipses. In the context of considering this famous *tabular* monument

207 Some details are provided at fn 205. The math of orbit-fitting (see, e.g., Rawlins & Hammerton 1973A or Rawlins 1991H) reminds me of a common alibi for Ptolemy, which is as belovéd of the Muffia as it is irrelevant to Ptolemy's pretensions. Muffia loyalist Peter Huber to *DIO*: Ptolemy never heard of Gauss. (See *DIO 2* ‡2 §H6 & §H14.) Well, has the Muffia? It was not DR talent but Muffia innocence — as to how even to *compute* the EH (or UH) problem — that left DR alone at the portals of the ancient intellectual temples we are now discovering & exploring here: the EH & Frankenstein orbits, Aristarchos' lunisolar elements, and ancient use of the 1000^{a} Astronomical Unit. (My oft-expressed gratitude to the Muffia has never been entirely facetious.)

208 The pieced-together Frankenstein orbit & A's integrality were discovered 1991/10/31-11/4. Realization that Hipparchos had (for both trios) taken all but 1 of his lunar elements from previous work occurred 1991/11/3-6.

209 This may at 1st look *ad hoc*, but I recommend 3 counter-considerations: [a] See the chronological-correlation discussions at §M6 & §M8. [b] The trio B monthly speeds of Jones 1991H (pp.114-115, 121) are unattested *and* ludicrously over-precise — down to the *arcsec*: 30°21′05″ & 27°58′08″. By contrast, his trio A speeds are the traditional values, rounded to *degree*/eighths. And elsewhere in the discussion (Jones 1999A pp.113, 120), roundings of 1°/2 are tossed in at the author's (apparent) whim! (These high-precision trio B speeds have no other basis than Jones' rigging them, in order to fit trio B exactly to his also-unattested equal-arc version of System A.) By contrast, DR's rounding procedures are standard-ancient for all 3 trios discussed (see fn 203), and the EH orbit elements are based on foundation-data which may be easily deduced, independently, from Hipparchos' attested output: §K4. [c] Jones' hypothetical Babylonian schemes both require "modified" parameters (p.104; §F4): two unrelated sets of speeds. (See fn 104. The common factors are merely apogee A & the model: half the circle at one constant speed, another constant speed for the other half.) I.e., the trio A & trio B solutions of Jones 1991H not only lack Babylonian attestation (other than trio A's speeds, which lead to $12.4 \cdot M_{A}$ = 366 day hilarity): they have only one common parameter. (Jones 1991H p.113: "There are reasons for doubting whether Hipparchos's [determinations of trio A e & trio B r] were carried out in a single work.") For contrast: half the 4 elements of DR's trio A solution are shared with DR's trio B solution (§M6), and the other half match the attested PH orbit.

210 Jones 1991M p.446: "Hipparchos could scarcely have composed [solar tables] when he wrote his treatises on the lunar theory; for at this stage he had no confirmed value for any of the fundamental periods . . . of the solar model."

211 Pliny 2.53 called these calculations "predictions". There is a reasonable interpretation by Neugebauer 1975 pp.319-321 & Toomer 1988 p.355, namely, that the Pliny report of Hipparchos' 600^{y} of calculated eclipses referred

(and the precise confirmatory implication of §N14), it is delightful to encounter, in a Pb paper of the History of Science Society's *Isis*, the straightfaced, mindblowing Muffia fantasy that Hipparchos & prior Greek astronomers never computed predictive astronomical tables. (Jones 1991M p.446; see here at §E4. So Hipparchos' 600-year eclipse table was *entirely* backwards? He wanted to predict only *past* eclipses? Well, most of the eclipses in the canons of moderns T.v.Oppolzer and J.Meeus are past ones, but neither stops at his present. Have Muffiosi forgotten that astronomy's historical pre-eminence has been built upon its predictive power?! See Schmales at §N16.) Jonestown's (anti-tabular) conception of ancient astronomy is presumably the sort of special Hist.sci insight which uncomprehending modern scientists are forever denied. Do not lose sight of what is certain: if a frontpage paper in *Isis* proposes this idea, it's got to be sober scholarship. Now, a naïve scholar, unfamiliar with Muffia wisdom & determination, might imagine in his innocence that Jones' revolutionary notion founders upon ancient astrologer Vettius Valens' explicit attestation to [predictive] Hipparchan solar tables & lunar tables by Hipparchos' near-contemporary, Apollonios. But these 2 impediments are brushed aside with ease: Jones just implies that Valens' testimonies may be based on a fake *and* an identity-confusion. This speculation is assisted by the fact that only one of a vast catalog of Hipparchos works is extant. (Guess who else takes advantage of paucity of early records — to let fly his psychoanalytic speculations on colliding worlds?) So Jones 1991M (p.446, emph added) points to: "the apparent absence of predictive tables in Hipparchus's [surviving] work. Only one author, the astrologer Vettius Valens (late second century [AD]), speaks of Hipparchian tables for the sun . . . . [possibly] *a later fabrication* based perhaps on Hipparchus's solar theory . . . ." Same suggestion at Jones 1991H (p.102). Note, too, the related & equally-forced (contra §N16) Jones-Muffia assertion (fn 242) that the also-attested lunar tables of Apollonios (c.200 BC) never existed and that the ancient (Valens) reference must be to Apollinarios (an Apollinarios who lived long after Hipparchos), since *the name is similar to Apollonios*. Just drop the "onios" and add "inarios". Well, the Muffia may be right. I can't prove otherwise.[212] But, since Muffiosi routinely slander dissenters from Muffia wisdom (like §G3) as Velikovsky's mental kin (see *idem* and *DIO 1.1* ‡1 §C7, ‡3 §D2), I cannot help recalling Johns Hopkins religion prof W.Albright's deft comment, cited by M.Gardner, who expresses it thusly:[213] "Velikovsky's historical method . . . is on a level with that of the professor who identified 'Moses' with 'Middlebury' by dropping the '-oses' and adding '-iddlebury'." Is it not inspirational to see similarly creative anagramology given top billing as bigleague scholarship, in the *JHA* and in the History of Science Society's *Isis*?

M8 Presumably, Hipparchos used hired calculators. (Which for the lunar analyses — performed long after the solar calculations — suggests consultation of his own school's §M7-cited 600^y catalog of mideclipse solar longitudes. This, without availability of — or concern to doublecheck — the data earlier used for the original computations, over several years of work on this catalog. See fn 214.) When these men were originally churning out the trio A solar data, they used the most up-to-date constants (ϵ_P & A_P) but found it most convenient to use (§M6) the older EH tables to compute mean longitude motion (since epoch) and equation of center. It was noted above at §M6 that the orbital elements used to

not to 600^y into the future but to the interval from Nabonassar (747 BC) to about the time of Hipparchos. However, the reader is not reminded that gentle Toomer 1967 p.146 called "absurd" P.Tannery's acceptance that Hipparchos had calculated 600^y of eclipses (in either direction). Perhaps we may salvage Toomer's judgement a bit here by supposing (as DR has done, throughout this paper) that Hipparchos' past catalog involved, in many or all cases, merely computing for each eclipse the solar position at an empirically reported time. (Calculating future eclipses required at least a syzygial lunar theory and involved much more work.)

[212] However, a common-sense point here: Valens refers to the Apollonios lunar tables along with those of Sudines & Kidines (both Babylonian), who were much nearer chronologically to Apollonios & Hipparchos than to the particular late Apollinarios whom Jones & Toomer refer to. See Neugebauer 1975 pp.262-263, 306, 574, 601 & n.2, 610-612, 666.

[213] Original from *NY Herald Tribune Book Review* 1952/4/20, cited & relayed in M.Gardner *Fads & Fallacies* NYC 1957 ed, p.327.

compute the trio A solar positions were not randomly distributed. We should re-emphasize that the split is correct, timewise — i.e., the laboriously-computed tables used are for the older solar orbit, EH, while the simple constant epoch-values are from the new solar orbit, PH. (Recognition of such a reasonable chronological pattern lends confirmatory support to the conclusions of this section.) By-product of these researches: we now know that solar trio B was computed before[214] solar trio A.

M9 To check these computations, it helps to start by reconstructing[215] (from the intervals listed at §M3 and from the details given at *Almajest* 4.11) the most likely estimates of Hipparchos' absolute times for the 3 eclipses of trio A.

A1: Nab 366 Thoth [01] 26 $18^h 3/5$ = $-382/12/22$-23 $06^h 3/5$.
A2: Nab 366 Pham [07] 24 $08^h 1/3$ = $-381/06/18$-19 $20^h 1/3$.
A3: Nab 367 Thoth [01] 16 10^h = $-381/12/12$-13 22^h.

M10 We next compute solar longitudes, much as previously for trio B (§L3) — but now using the §M9 times (again rounded to the nearest hour), and not with pure EH elements, but instead from the hybrid Frankenstein elements just discussed (found at §M4). From eq. 5, mean longitude (for time since Phil 1 epoch, $-59^E + 25^d 19^h$) is found: $f_1 = 17°44'$ $(-72^E) + 356°48'\ (13^E) + 24°38'\ (25^d) + 0°47'\ (19^h) + 227°40'\ (\epsilon) = 267°37'$. The interval between this eclipse (A1) and the next eclipse (A2) is $177^d 14^h$ (§M2). Computing the corresponding mean-motion arc, from F_K-based tabular entries: $f_2 - f_1 = 147°51'$ $(150^d) + 26°37'\ (27^d) + 0°34'\ (14^h) = 175°02'$. And, working double-differentially in this simple case: since $t_3 - t_2$ is 12^h less than $t_2 - t_1$, we have $f_3 - f_2 = f_2 - f_1 - 0°30'$ $(-12^h) = 175°02' - 0°30' = 174°32'$. Thus, summing up: $f_2 = f_1 + 175^d 02' = 82°39'$, and $f_3 = f_2 + 174°32' = 257°11'$. The true longitudes ϕ_i now follow (as in §L3) from eq. 4:

A1: $\phi_1 = 267°37' + 1°15' = 268°52' = 268°7/8$
A2: $\phi_2 = 082°39' - 0°54' = 081°45' = 081°3/4$
A3: $\phi_3 = 257°11' + 0°41' = 257°52' = 257°7/8$

The longitude intervals A2−A1 and A3−A2 are then 172°53′ & 176°07′, or: 173° − 1°/8 and 176° + 1°/8. These are just the trio A solar longitude intervals we set out (§M3) to track down. Restoring the 1° scribal error (*idem*) for our 2nd interval, we have recovered the attested intervals of *Almajest* 4.11 (§M2), and our precise reconstructed A3−A2 interval will be: 175°07′.

N From Hipparchos' Sham Emerges: Aristarchos' Lunar Apogee

N1 Thus far, we have dealt almost entirely with Hipparchos' solar model. We now turn to his syzygial lunar model. As Toomer 1973 (n.10) & Jones 1991H (nn.20&25) have helpfully & correctly pointed out: the *Almajest* 4.11 times, cited as those used by Hipparchos (in his lunar deductions), lack correction for equation-of-time. (By contrast, Ptolemy rightly uses eq.time for the Moon, though not for other bodies, where the effect is less critical.)

[214] Hipparchos performed his lunar computations later than his solar work (§N3), so we cannot tell their chronological order from that of the corresponding solar calculations. (Indeed, the reverse-order turns out to be the case: §N3.) But I expect that Hipparchos' 600 years of computations (mostly of past lunar eclipses' solar longitudes, naturally) occurred roughly backwards during his career, not forwards. This is our finding here for trios A&B, and it is obviously the most likely order if his eclipse-catalog project wasn't initially so ambitious as it eventually became.

[215] A helpful clue (to the exact original times) is provided by the disparate endings of the trio A intervals (§M3). Those familiar with ancient rounding practice will quickly see what I mean. (DR determined the absolute times for all 3 mideclipses, of both trios A&B, before proceeding with his analysis. I.e., eventual agreements of hypothesis with attested data were not effected by post-hoc re-adjustments of these times.) Curiously, the absolute values of times and longitudes have not previously been induced; e.g., Britton 1967 p.47: "Since the actual longitudes of the sun are not given, it is impossible to deduce the values of the solar equation [eq.ctr] which would account for the observed discrepancy." See §G8. [Note added 1993: Reference here is to pp.38-39 of the 1992 edition of Britton.]

The times for trio B will be those of §L2, and the times for trio A will be those of §M9. (While computing the foregoing solar places, we ignored fractions of hours. This is not advisable for the Moon, whose mean motion is rapid: $33'$/hr sidereal, $30'$/hr synodic.)

N2 Each trio establishes 3 equations of condition, from which one may solve for 3 lunar elements: [1] mean-longitude-at-epoch ϵ, [2] apogee-at-epoch A or mean-anomaly-at-epoch $g_\circ$, & [3] the lunar epicycle-radius r (for trio B) or eccentricity e (for trio A). Also required are the mean motions[216] of the Moon (& Sun) and the lunar apogee. The standard ancient length of the synodic month is the admirably accurate "Babylonian" value,

$$M_{\mathrm{A}} = 29^{\mathrm{d}}31'50''08'''20'''' = 29^{\mathrm{d}}.5305941 \qquad (6)$$

(*Almajest* 4.2-4), which DR has mathematically traced to Aristarchos (Rawlins 1985S, Rawlins 1991H §B10 & fn 1; see also here at fn 81 & §N11). This and the relation (also Aristarchan: Rawlins 1985S)

$$269 \text{ anomalistic months} = 251 \text{ synodic months} \qquad (7)$$

are both associated with Hipparchos (*Almajest* 4.2). (His adopted anomaly epoch value will be induced below: eq. 9.) Thus, at the outset of our search for the 3 unknowns noted above (ϵ, A, & r or e), we may tentatively assume that these 2 motions (eqs. 6 & 7) were adopted by him.

N3 Finally: since the Moon's motion is the sum of its synodic motion and the Sun's motion, choice of yearlength will affect ϵ — which allows us to test for the yearlength Hipparchos adopted at the time of the lunar calculations. This turns out to be his traditional PH orbit value (§K10). Which tells us what is already selfevident (since the solar longitudes were used in the lunar deductions): both lunar calculations were performed after the latter of the two solar calculations (§L3 & §M4), both of which used Kallippos' yearlength. However (as noted at fn 214), none of this tells us which lunar calculation came first. But, as already noted by Jones 1991H p.113 (which via n.31 refers to Toomer 1967), the Pappos & Ptolemy (*Almajest* 4.11) accounts both indicate that the trio A lunar deduction preceded that of trio B.

N4 Since the *Almajest* 4.11 data are actually just (for each eclipse trio) 2 intervals instead of 3 independent data, we may simplify (& make surer) our search by just using these 2 intervals. Moreover, if one uses just the intervals, ϵ cancels[217] out of the equations of condition; thus, we have merely 2 equations for finding 2 unknowns: A and e (or r). (Note: the 1° correction to the solar intervals has no effect here, since the 1° error occurred between[218] the solar and lunar calculations. Thus, to reconstruct Hipparchos' trio A work,

[216] Different likely (non-Jonestown) ancient values for the necessary mean motions (2 lunar, 1 solar) will have little effect on a 3-unknown solution for the time of the chosen trio — though of course they will affect the solutions for ϵ & A at Hipparchos' remote tabular epoch (Phil 1).

[217] See fn 161. An alteration in ϵ will alter the deduced A by the same amount. However, in effect, the 2 unknowns here are actually [a] the lunar anomaly for any one of the 3 observations (the other 2 anomalies then follow, once even a crude anomalistic motion is adopted), and [b] the lunar e (or r). Using A for the 1st unknown (instead of anomaly) introduces a dependence upon ϵ and the precise anomalistic motion; but (in this case) these be turn out to be merely traditional constants, so: treating A as an unknown causes no real loss of flexibility in the solution process here. However, my modern preference for using the (more slowly varying) apogee is probably unhistorical. (See §N16.) I am converted at eq. 9.

[218] As a matter of interest: had the 1° slip not occurred in trio A, Hipparchos would have found $e = 5^{\mathrm{p}}1/2$ & $A = 89^\circ$, by treating both as unknowns (which he didn't: §N7). Both elements are more accurate than those he actually got (*Almajest* 4.11) for either trio A or trio B. (Averaging these with trio B's results produces elements not far from Ptolemy's.) With some justice, the Muffia has criticized R.Newton (e.g., Swerdlow 1979) for his shaky speculation that the *Almajest* lunar epicycle radius $r = 5^{\mathrm{p}}1/4$ came from Hipparchos. Since both Ptolemy's proofs of this value are the usual superneat fabrications, we ought to look elsewhere for its origin. Note: Ptolemy scorns Hipparchos' two discrepant values, but fairminded scholars ought at least to credit Hipparchos with nonfudgery, while instead scorning Ptolemy's own laughably overprecise agreements: $r = 5^{\mathrm{p}}13'$ & $5^{\mathrm{p}}14'$, both at *Almajest* 4.6. See R.Newton 1977 pp.122-123, which finds that an error of merely 15^{m} in 1 eclipse-time would affect deduced r by $9'$, and $g_\circ$ by $43'$! See fn 237.

we will use here the intervals of §M2, not §M3.) In order to arrange[219] that lunar & solar positions will be 180° apart at the given mid-eclipse times, I have, using §K10, set (§N10 & fn 237):

$$\epsilon = 178^\circ \qquad \Delta\epsilon \equiv \epsilon - \epsilon_{\mathrm{P}} = 178^\circ - 227^\circ 2/3 = 310^\circ 1/3 \qquad (8)$$

(Note that both 178° and $310^\circ 1/3$ are merely the *Almajest* 4.2 values transposed to Phil 1.) I believe mean-elongation-at-epoch (or mean-synodic-longitude-at-epoch) $\Delta\epsilon = 310^\circ 1/3$ was Aristarchos'. (See Rawlins 1985K. Since eqs. 23&24 and *Almajest* 3.1 all establish a connection of Hipparchos to Aristarchos' work, I will use Aristarchos' name for the pre-Hipparchos epoch values, from c.300 BC, which we are recovering here; however, one cannot really be sure who was responsible for much of this work: Kallippos 330 BC, Timocharis c.300 BC, Aristarchos 280 BC, Aristyllos c.260 BC — perhaps Apollonios c.200 BC, who is actually credited in antiquity with lunar tables: §M7 & fn 242. Or anonymous.)[220] Any value for ϵ will suffice at this point, though it turns out that, here and below, eq. 8 will provide the ϵ which Hipparchos used. From the intervals (§M2 & §L1), I have solved for the other Hipparchan elements, with the following results:[221]

The trio A solution: $A = 96^\circ.136$ & $e = 6^{\mathrm{p}}.403$ = (335 1/2)/(3144).
The trio B solution: $A = 95^\circ.410$ & $r = 4^{\mathrm{p}}.773$ = (248 2/5)/(3122 1/2).

N5 Now, the most striking aspects of these solutions are: [a] The e & r results are not equal to the values cited in *Almajest* 4.11, namely, $6^{\mathrm{p}}.253$ = (327 2/3)/3144 & $4^{\mathrm{p}}.756$ = (247 1/2)/(3122 1/2), respectively. [b] Both values for A are around[222] 96°, which is far from the correct value ($91^\circ.4$) or the *Almajest* value ($92^\circ 43'$) for epoch Phil 1.

N6 In our earlier investigations of Hipparchos' solar work, we were seeking unattested orbital elements that would produce his given intervals within standard ancient rounding precision. But here in the lunar case, we are examining a more precise (and inverse) situation: certain given intervals (§M2 & §L1) are the basis of Hipparchan *Almajest* 4.11 calculations which will produce partly *attested* elements (§D1 item [b]). So our demands on the precision can be higher, which should make our findings more revealing.

[219] This is easy to accomplish, since, when using eclipse intervals (differential data) for searching out Hipparchos' A & e (or r), ϵ cancels out of the problem. (See fn 217.) It may thus be adjusted later, at one's leisure, to ensure 180° elongations at mideclipse times.

[220] Against Kallippos' rôle: his rough monthlength was supplanted by Aristarchos' later-canonical accurate value (eq. 6). But this improvement (fn 81) of the lunar speed's accuracy would not prevent Kallippos (contemporary with Phil 1) from being the source of the $310^\circ 1/3$ Phil 1 epoch value in eq. 8. Note that Timocharis made observations both before and after the decade during which were taken the Aristarchan-era observations proposed by Rawlins 1985S as underlying various attested ancient solar, lunar, & planetary period relations. (Perhaps Timocharis played Flamsteed to Aristarchos' I.Newton. If so, one hopes the relationship was more amicable than the later rendition.) I must note that some of the *DIO 1.1* ‡7 fn 6 problems with Aristarchos' sole alleged extant work ("On the Sizes & Distances of the Sun & Moon") were anticipated by Neugebauer 1975 p.642. Moreover, Neugebauer 1975 (p.636 — especially n.4 — & p.643) makes the telling point that the grossly false distances of the Sun & Moon (which would result from the work's terribly erroneous 2° lunar diameter) are never presented — this despite the fact that these distances are part of the work's very title! That is, "Sizes & Distances" gives the Sun's & Moon's sizes but not distances; but the former are virtually unaffected by the infamous semidiameter-error, while the latter are much affected. This leads Neugebauer to doubt the work's empirical seriousness or sincerity (p.643), while DR takes it merely as further (see also §R10) indication that the author was not Aristarchos but was just a tedious developer of Aristarchos' six hypotheses (who stopped short when confronted with his deduced distances — which disagreed violently with those presumably well known to be Aristarchos'). Another possibility: the author was a pure mathematician, a posthumous devotee, whose innocence of the outdoor sky caused his inadvertent mangling of hypothesis #6, as explained at *DIO 1.1* (*loc cit*). In any case, the question (on which I remain flexible) of the botcher's identity should not divert one from the main point of *idem*: we now have the explanation of the long-mysterious "Aristarchan" lunar diameter error (by a factor of 4). There is no doubt that some of "Sizes & Distances" (not the part involving the factor-of-4 error) was cited by Archimedes only a few generations later (Archimedes p.223). And the peculiarity of pseudo-Aristarchos' 2° value was noted by Pappos, so we know that the whole pseudo-A work was (in some form) already accepted as genuine by c.300 AD at the latest.

[221] The lunar anomaly configuration of the eclipses is far more advantageous for solution than the solar anomaly configuration. Thus, the results of analysis are fortunately tight; e.g., one has no long groove (as at fn 205) of possible solutions for the lunar elements.

[222] First noted 1991/11/3.

N7 Suppose the 2 peculiarities noted at §N5 are related. Realization of the implications of overdetermination here suggests a simple hypothesis which solves both oddities simultaneously: Hipparchos did not use the full trio to solve for 3 elements; instead, he merely, in each case (trio A & trio B), used one interval (between one pair of eclipses) to solve for *one* element: e for trio A and r for trio B. The full 3-unknown calculation for a trio is extremely laborious (see *Almajest* 4.6-8 or, e.g., Toomer 1973 or Pedersen 1974 pp.172f), so Hipparchos took a shortcut, which we will now examine. (If this ploy was that of hirelings, did Hipparchos even know of it? See fn 253.) For reconstructing his shortcut, we assume that he used the intervals A3-A2 and B2-B1. The four retained eclipses[223] — A2, A3, B1, B2 —- will (§N10 & eq. 9) prove to be consistent with the same value of A (or $g_\circ$). Moreover, this value is integral; such rounded parameters are typical of antiquity. (See Rawlins 1985K, which long predated the current findings, and whose speculations are now being confirmed here by repeated neat success: §K10, fn 196, §N4, §N7.) Our reconstruction will culminate swiftly & beautifully below, at §N14.

N8 Pedersen 1974 says (p.174 n.5) that O.Neugebauer suggested (as does Toomer 1973 p.8) Hipparchos as inventor of the eclipse trio method of *Almajest* 4.6. The results of the current paper show otherwise — and leave us with no direct evidence of the method's use before Ptolemy. (Same for equation of time: Jones 1991H n.25. Ptolemy's math at *Almajest* 4.6 is far superior to what we are here revealing to be behind the Hipparchan work preserved at *Almajest* 4.11.) Comments: [a] There is no evidence that Ptolemy originated it (though he seems admirably adept at it). In fact, Hipparchos' mention of ***trios*** in both cases A&B suggests that the triad technique (*Almajest* 4.6) predates him (thus his pretense to being up to the mathematician-standards of his day). [b] The triad method involves lovely math but is impractical because highly sensitive to small observational errors (fn 218 & §P1). [c] The actual ancient determiner of the *Almajest* lunar elements was much more likely[224] to have accomplished his purpose by examining fits of dozens of eclipses.

[223] [Revised 1997.] The unreduced reports (seasonal hours, apparent time) of the trio B eclipses (Alexandria) are creditably accurate; trio A (observed at Babylon, but dated by Athenian calendar), curiously poor. (The eye can discern a total eclipse's mid-time to ordmag 1 timemin; so the main sources of ancient eclipse times' errors may be: [a] lunar hr-angle via sundial for time, & [b] reporters' roundings.) Comparing to modern calculations, we find equinoctial-time O−C errors for each observed eclipse bound: $-46^{\rm m}$ (A1), $-40^{\rm m}$ (A2), $+10^{\rm m}$ (A3), $-7^{\rm m}$ (B1), $+10^{\rm m}$ (B2), $-10^{\rm m}$ (B3). (Trio A rms error = 0h.6; trio B, merely 0h.2. So, I do not agree with R.Newton 1977 pp.122f & 345 that the trio B raw observational data were fabricated.) Note, at *Almajest* 4.11: most trio A eclipse-times are given in whole hrs; trio B, thirds of hrs. (Halving these figures produces roughly the above-cited rms errors — which should be the case if the data are genuine.) This presumably reflects the (real) greater precision-needs of 200 BC Greek theorists vs. 382 BC Babylonian theorists. And Greek borrowing of the trio A Babylon observations hints that 382 BC Greek astronomical observers were inferior to Babylon's. Ptolemy's fudged reductions (to Alexandria mean time of mid-eclipse) infected the data with serious additional errors: $+11^{\rm m}$ (A1), $+24^{\rm m}$ (A2), $-13^{\rm m}$ (A3), $-6^{\rm m}$ (B1), $+22^{\rm m}$ (B2), $-21^{\rm m}$ (B3). Thus, Ptol−C errors of his *Almajest* 4.11 intervals: $+20^{\rm m}$ (A2−A1), $+12^{\rm m}$ (A3−A2), $+45^{\rm m}$ (B2−B1), $-63^{\rm m}$ (B3−B2). Ptolemy's lunar A and r were well chosen, but his acceptance of the Hipparchos PH lunisolar theory introduced a large annual periodic error (amplitude c.$20^{\rm m}$ after accounting for then-unknown lunar theory annual equation): effect much worse for trio B (Mar-Sep) than for trio A (Jun-Dec). Thus, his intervals' errors (above) **had to be** larger for trio B than for trio A. Such conveniently scaled errors are typical of "The Greatest Astronomer of Antiquity". I believe that the first clear enunciation of this crucial point (in another *Almajest* context) is due to Gingerich 1980 p.262 & Fig.3. (Ptolemy was fudging towards a theory that happened to give nearly correct times for trio A; but for trio B, where his theory happened to be very wrong, the same habit led to disaster. I.e., his improvement of trio A times' accuracy doesn't undo RN's correct conclusion that both trios are fudged.) Bottom lines: [a] Ptolemy pretended that his theories fit outdoor eclipses to ordmag a timemin, though his adopted trio B eclipse time-intervals were off by ordmag an *hour*. [b] Trio B's 3 observed times were (rms) thrice as accurate as trio A's; but, after The-Greatest's massages, trio B's 2 intervals were (rms) thrice as ***in***accurate as trio A's.

[224] This elementary point was never understood by Ptolemy, nor is it grasped by most modern commentators on him — few of whom have experience at fitting orbits to data (a point which needs no further defense after the current paper's revelations). An exception is Muffia-friend P.Huber (1991/10/1 to *DIO*, quoted more fully at *DIO 2.1* ‡2 §H22): "I am pretty sure that the ancient astronomers . . . must have derived their parameters by trial and error from rather inadequate sets of observations." Compare to DR's comments (1986/2/28 to van der Waerden): "A . . . general thought about our remnants of ancient astronomy. Real evolution of empirical astronomical theories has nothing to do with the sort of artificial math we find in the [*Almajest*]. The orbit of a planet is not based on a handful of observations which are then treated by Euclidean postulates in a neat formal manner. No, ancient orbits obviously evolved like modern ones: through decades of incremental adjustment, in response to scrupulously observed & analyzed failures of theory. Thus, the very format of the [*Almajest*], which modern Ptolemists . . . worship above all, is ironically one of its most egregiously phony aspects." (Neugebauer 1957 p.191 rates the *Almajest* "one of the greatest masterpieces of scientific analysis ever written".) Jones 1991M p.445 (emph added) innocently lauds "Hipparchus's use of *small* numbers of carefully selected observations to determine the parameters of his models". Of course, no one knew just *how* small was the actual number of data Hipparchos used — until the present discoveries, starting at §N7. These findings thus dramatically demonstrate (since no real astronomer would compute r from 2 data!) that Hipparchos was not at all a serious contributor to ancient astronomy's development of the lunar theory, e.g., the excellent value of r ($5^{\rm p}$ 1/4) preserved in the *Almajest* — which I suspect was that of Aristarchos and-or Apollonios. (Trio B occurred at the very time Apollonios flourished. But we do not possess evidence that Hipparchos used his work, while the connection of Hipparchos to Aristarchos is undeniable. See *Almajest* 3.1; also above at §N2.) Further: given the number of small but nontrivial then-unknown perturbations in the lunar motion, it was not possible in antiquity to mathematically elicit a highly reliable estimate of r from an eclipse triad (no matter if the 3 observations were perfectly accurate), nor could extremely consistent values be obtained even from a string of perfectly observed eclipse triads. (In *Almajest* 4.6 & 4.11, Ptolemy outrageously pretends that all 4 of his adduced triads are consistent, to ordmag *one arcmin*, with the same eq.ctr & thus r: see R.Newton 1977 pp.122-123.) An astronomer with genuine experience in such matters would have known that.

N9 Another note on ancient procedure: if Ptolemy's approach (*Almajest* 4.2 f) is any guide, Hipparchos' math formally used anomalistic motion (eq. 7) rather than apogee motion (fn 217). Thus, recalling that adoption of $\epsilon = 178°$ (eq. 8) produced A nearly equal to 96° (§N5), we see that the corresponding Phil 1 epoch value for mean anomaly $g_\circ$ would have been:

$$g_\circ = \epsilon - A = 178° - 96° = 82° \qquad (9)$$

— just as integral a parameter as input ϵ & A, of course. We will now find out if this was indeed Hipparchos' adopted value.

N10 To test the intriguing hypothesis of §N7, we need only set e or r equal to the value which Hipparchos calculated (reported at *Almajest* 4.11) and then work backwards to solve for $g_\circ$. The results for $g_\circ$ are given below, along with the input data:

Reported Interval	Assumed	Deduced
A3−A2 = 175°07′	e = (327′2/3)/(3144′)	$g_\circ$ = 81°59′
B2−B1 = 180°20′	r = (247′1/2)/(3122′1/2)	$g_\circ$ = 81°58′

N11 The $g_\circ$ found here are strikingly close to each other and to the already-suspected (eq. 9) integral value, $g_\circ$ = 82°. (The odds are ordmag 1000-to-1 that both $g_\circ$ would hit by chance so close to the same integral value.) As just noted, the associated Aristarchos-EH ϵ = 178° (eq. 8) is also integral. So I am proposing (§N7) that Hipparchos adopted these two integral values for ϵ & $g_\circ$ at the outset — and thus he solved only for a single unknown (e or r) from a single interval, for each eclipse trio. Since it is clear[225] that he pretended otherwise, this is a double mathematical hoax.[226] (Comment: Though I have sometimes been critical of Hipparchos — whose scientific acumen was rather overrated by later ancients — neither I nor any other scholar ever previously suspected him of any sort of dishonesty.) As noted, the single-interval math would be relatively easy. Two true longitudes form the given interval. And we could find the corresponding two mean anomalies by computing from eqs. 6, 7, & 9; however, the first step (§N12) of my reconstruction is simpler than this. That reconstruction's ultimate success (§N14) therefore evidences, on the ancient computer's part, a subtle feel for the problem. He understood 2 related points: [a] the anomaly-differences needed to be more accurate than the absolute anomalies; [b] after computing the 1st eclipse's anomaly from the epoch value ($g_\circ$, eq. 9) and his anomaly tables (founded upon eqs. 6 & 7), the remaining required eclipses' anomalies are most easily found differentially (much as at §L3).

[225] See *Almajest* 4.5 & 11 and Toomer 1984 p.181 n.24.

[226] Some may aver that this charge evidences an overcritical nature in DR. Comment: this is a serious mathematical pretense, which misled numerous scholars (in various ways) for over $2000^{\rm y}$; indeed, the most tragic victim of the imposition is a living scholar, Hipparchos' modern biographer, G.Toomer. Thus, one is justified in plainly calling it fakery (on the part of someone in Hipparchos' school).

N12 For trio A, we use the 3 eqs. just cited, following the tabular method of ancient calculation. (The tabular format was obviously that of *Almajest* 4.4, where the anomalistic tables are almost exactly equal those of Hipparchos, which were based upon eqs. 6 & 7. We again adopt the notation $1^{\mathrm{E}} = 365^{\mathrm{d}}$ = 1 Eg.yr.) For finding g_1 (using the absolute time[227] t_1 of §M9), the tabular addition is as follows: $g_1 = 82^\circ\ (g_\circ) + 92^\circ 14'\ (-72^{\mathrm{E}}) + 73^\circ 21'\ (13^{\mathrm{E}}) + 326^\circ 37'\ (25^{\mathrm{d}}) + 9^\circ 48'\ (18^{\mathrm{h}}) + 0^\circ 20'\ (3^{\mathrm{h}}/5) = 224^\circ 20'$. Next, from the same tabular method, the anomalistic motion is found for the interval $t_2 - t_1$ ($177^{\mathrm{d}}13^{\mathrm{h}}3/4$, §M2): $g_2 - g_1 = 159^\circ 45'\ (150^{\mathrm{d}}) + 352^\circ 45'\ (27^{\mathrm{d}}) + 7^\circ 05'\ (13^{\mathrm{h}}) + 0^\circ 25'\ (3^{\mathrm{h}}/4)$[228] $= 160^\circ 00'$. Likewise, for the interval $t_3 - t_2$ ($177^{\mathrm{d}}01^{\mathrm{h}}2/3$, §M2): $g_3 - g_2 = 152^\circ 30'\ (177^{\mathrm{d}}) + 1^\circ 05'\ (2^{\mathrm{h}}) - 0^\circ 11'\ (-1^{\mathrm{h}}/3) = 153^\circ 24'$. Toting up these results, we have: $g_2 = 224^\circ 20' + 160^\circ 00' = 24^\circ 20'$, and $g_3 = 24^\circ 20' + 153^\circ 24' = 177^\circ 44'$.

For trio B, we compute very similarly but not quite identically. (This calculation is generally regarded as having been computed separately from that of trio A: §N3.) In this case, [i] we won't need g_3, and [ii] we express our sums (given the hint at §O3, regarding the trio B computer's predilections) in standard-ancient-rounded fractions of degrees instead of arcmin. Thus (using the §L2-§L3 absolute time[229] $t_1 = 122^{\mathrm{E}}\ 345^{\mathrm{d}}\ 07^{\mathrm{h}}$), we have $g_1 = 82^\circ\ (g_\circ) + 221^\circ 40'\ (108^{\mathrm{E}}) + 162^\circ 04'\ (14^{\mathrm{E}}) + 351^\circ 27'\ (330^{\mathrm{d}}) + 195^\circ 58'\ (15^{\mathrm{d}}) + 3^\circ 49'\ (7^{\mathrm{h}}) = 296^\circ 58'$ which nearly[230] equals 297°. For the interval $t_2 - t_1$ ($178^{\mathrm{d}}1/4$, §L1), the anomalistic motion $g_2 - g_1 = 159^\circ 45'\ (150^{\mathrm{d}}) + 5^\circ 49'\ (28^{\mathrm{d}}) + 3^\circ 16'\ (6^{\mathrm{h}}) = 168^\circ 50'$ or $168^\circ 5/6$. So $g_2 = 297^\circ + 168^\circ 5/6 = 105^\circ 5/6$.

Next, for each trio, to enhance notational facility, we will set the two consecutive g_i used by Hipparchos equal to, respectively, α & β. For trio A:

$$\beta_{\mathrm{A}} = g_2 = 024^\circ 20' \qquad \alpha_{\mathrm{A}} = g_3 = 177^\circ 44' \tag{10}$$

For trio B:

$$\beta_{\mathrm{B}} = g_1 = 297^\circ 00' \qquad \alpha_{\mathrm{B}} = g_2 = 105^\circ 50' \tag{11}$$

The anomalistic differences, $\alpha - \beta$, are: $153^\circ 24'$ (trio A) & $168^\circ 50'$ (trio B). (Toomer 1973 pp.9 & 13 has: $153^\circ 25'$ & $168^\circ 50'$.) We then compute difference δ between the true & mean longitudinal motion over the stated time-interval, for trio A (§M2) & trio B (§L1). Now, each subsequent calculation (of e & r) will be exceedingly sensitive to tiny errors in δ. (This is especially so for trio A, where even a lapse of $1''$ is critical: §N14.) Thus, we will compute the mean longitudinal motion precisely — to the arcsec — from the lunar longitude tables of *Almajest* 4.4. (These tables are merely the precise sum of: the solar tables of *Almajest* 3.1 added to the much older lunar elongation tables of *Almajest* 4.4. The former are precisely based upon Hipparchos' mean solar motion F_{J}: §G10. The latter tables are precisely based upon 360° divided by the standard eq. 6 ancient monthlength used by Hipparchos, M_{A} — which indisputably goes way back (to Aristarchos, DR asserts: §N2). It would thus seem reasonable — to anyone outside the Muffia — to suppose that both tables[231] were available to Hipparchos. A byproduct of the development below is: new confirmatory evidence at §N14 that this idea is not only unshocking but, better yet, true. See contrary arguments at Jones 1991H pp.103, 113: here at §F1 & §G2.)

[227] Phil 1 is Nab 425, thus Nab 366 (§M9) is -59^{E} after epoch Phil 1. (Ancient lunar tables used 18^{E} macro-intervals; see *Almajest* 4.4; thus the split of −59 yrs into intervals of −72 yrs & +13 yrs.)

[228] The motion for 3h/4 is almost exactly $24'30''$, very slightly ($0''.2$) under. But my guess (naturally influenced by the resultant perfect match!) is that the ancient computer just quickly reasoned mentally: $33'$ for an hour, so $8'$ for a quarter hour; thus, $25'$ (the difference) for three quarters of an hour. (Had the computer used 14h minus 1h/4, the result would've been $159^\circ 59'$, which would probably have just been rounded anyway to 160°.)

[229] Phil 1 is Nab 425, so Nab 547 (§L2) is 122^{E} after the Phil 1 epoch. (Similarly at fn 227.)

[230] If this (rather conventional ancient) rounding is discarded, then eq. 20 (below) would produce $r = 247'27''$, which would of course be rounded to $247'1/2$; thus, our deduced r will agree with Hipparchos' value (*Almajest* 4.11), regardless.

[231] The realization (1991/11/30) that arcsec-precision tabular calculation produced precisely the e & r of trios A&B provided welcome confirmation of this independently obvious point.

N13 Trio A's attested time-interval is (*Almajest* 4.11, §M2): $177^{\mathrm{d}}01^{\mathrm{h}}2/3$. For entering the *Almajest* 4.4 lunar mean longitude table, this must be broken into 5 months (150^{d}), 27^{d}, 1^{h}, plus 2/3 of 1^{h}; adding the corresponding tabular entries, we have[232] the computed mean longitude interval:

$$\Delta f_{\mathrm{A}} = 176^\circ 27'26'' + 355^\circ 45'44'' + 0^\circ 32'56'' + 0^\circ 21'58'' = 173^\circ 08'04'' \tag{12}$$

Thus, since the corresponding attested true longitude interval (*Almajest* 4.11, §M2) is $\Delta\phi_{\mathrm{A}} = 175^\circ 1/8$ or (as above)[233] $\Delta\phi_{\mathrm{A}} = 175^\circ 07'$, we have:

$$\delta_{\mathrm{A}} = \Delta\phi_{\mathrm{A}} - \Delta f_{\mathrm{A}} = 175^\circ 07' - 173^\circ 08'04'' = 1^\circ 58'56'' \tag{13}$$

(Toomer 1973 p.13 has $1^\circ 59'1/2$, for he uses the exact attested expression $\Delta\phi_{\mathrm{A}} = 175^\circ 1/8$, instead of our arcmin-rounded figure $175^\circ 07'$.) We next establish the same groundwork for trio B. The attested time interval is (*Almajest* 4.11, §L1): $178^{\mathrm{d}}06^{\mathrm{h}}$. We break this down into 5 months, 28^{d}, & 6^{h}, so (from the *Almajest* 4.4 tables):

$$\Delta f_{\mathrm{B}} = 176^\circ 27'26'' + 8^\circ 56'19'' + 3^\circ 17'39'' = 188^\circ 41'24'' \tag{14}$$

The corresponding attested true longitude interval (*Almajest* 4.11, §L1, §N10) is $\Delta\phi_{\mathrm{B}} = 180^\circ 20'$; thus:

$$\delta_{\mathrm{B}} = \Delta\phi_{\mathrm{B}} - \Delta f_{\mathrm{B}} = 180^\circ 20' - 188^\circ 41'24'' = -8^\circ 21'24'' \tag{15}$$

(Toomer 1973 p.9 has $-8^\circ 22'$.) At this point, instead of finding 2 more δ values for each trio (as in *Almajest* 4.6 & Toomer 1973), we veer off into the Hipparchos shortcut[234] proposed at §N7. Taking

$$U = -[(\cos\alpha + \cos\beta) + \cot\delta(\sin\alpha - \sin\beta)]/2 \tag{16}$$

and

$$V = \cos(\alpha - \beta) + \cot\delta\sin(\alpha - \beta) \tag{17}$$

we can find e (or r) immediately from:

$$e = r_{\mathrm{M}}/(U + \sqrt{U^2 - V}) \tag{18}$$

[232] That Δf is expressed to greater precision than $\Delta\phi$, in both eq. 13 & eq. 15, is partly due to the fact that, by the time the lunar investigations were made, the solar longitudes of the eclipses (on which the true longitude intervals $\Delta\phi$ were based) had been computed years previously (fn 214), and probably already published in Hipparchos' famous 600^{y} eclipse catalog, presumably already rounded in the fashion indicated at §M10 for trio A & §L3 for trio B.

[233] The $07'$ ending here is, of course, precisely that of §M10 (restored) or §N10 for the A3−A2 interval.

[234] Mathematicians will quickly see that eqs. 16-18 are based upon [a] the standard angle-sum trig equation of "Ptolemy's Theorem" (*Almajest* 1.10), [b] our eq. 3, and [c] the ability to solve quadratic equations. (Though never sophisticated in math astronomy, even the early Babylonians could handle quadratic — and cubic! — equations: van der Waerden 1963 pp.69-71 or van der Waerden 1978 pp.668-670; van der Waerden 1963 p.71 also shows incidentally that these old Babylonian mathematicians were more adept at such math than O.Neugebauer.) Toomer's lifelong promotion of a wobbly fantasized Hipparchan trig table (§D1), built up merely from the half-angle theorem (thus using a $7^\circ 1/2$ interval), has been based upon Toomer's disbelief that Ptolemy's Theorem existed as early as Hipparchos. (See Toomer 1973 pp.6, 8, 18, Toomer 1984 p.50 n.59.) The high-precision success of the present reconstruction suggests (as does fn 283) that both the theorem and resultant highly accurate trig tables predated Hipparchos. Of course, one may safely predict that an enraged Toomer will attempt to discredit this DR reconstruction's support for the theorem's existence in Hipparchos' day by pretending that everything-we-know (which may be translated as: nothing; fn 100) tells us the theorem didn't exist then. It's hard to draw a circle that's rounder than Muffialogic. (See fn 99.)

N14 For trio A, we have α & β from eqs. 10 and δ from eq. 13, so these are substituted into eqs. 16 & 17 to obtain U = 5.4253 & V = 12.043. Substituting $r_{\rm M}$ = 3144′ (*Almajest* 4.11, §D1, eq. 23, §N10) into eq. 18, along with the U & V just found, yields:

$$e = 3144'/9.59554 = 327'39'' \doteq 327'2/3 \qquad (19)$$

in accord with the attested e of *Almajest* 4.11 (§D1, §N10). Relative to the needlessly-contended question (e.g., §M7, §N11) as to whether Hipparchos possessed the *Almajest* tables for solar mean longitude & lunar mean elongation (& thus those for lunar mean longitude: §N11), it should be pointed out here that exact — not tabular — calculation in eq. 12 would have produced 173°08′05″, which would have seriously degraded the agreement of the above-deduced e with the attested trio A value 327′2/3, since the outcome of eq. 19 would have been 327′36″ = 327′3/5. For trio B, substituting $r_{\rm M}$ = 3122′1/2 (*Almajest* 4.11, eq. 24) into eq. 18, along with U = 6.2169 & V = −2.30 (computed by substituting eqs. 11 & 15 into eqs. 16 & 17), we have:

$$r = 3122'30''/12.6161 = 247'30'' = 247'1/2 \qquad (20)$$

which agrees perfectly with the Hipparchos value at *Almajest* 4.11 (§D1, §N10).

N15 Before publishing his results, Hipparchos evidently failed to review how well his deduced elements checked with his lunar positions, a check we will now perform. Starting with trio A, we use standard ancient Greek eqs. 3-5 — but with the appropriate lunar theory elements of eq. 19 as well as ϵ = 178° (eq. 8), and with g found from eqs. 6-7&9 (d values at §M9). The results (rounding to 1′ as we go) would have been, for trio A:

A1: ϕ = 084°14′+ 4°30′ = 088°44′ = 088°3/4
A2: ϕ = 263°59′− 2°15′ = 261°44′ = 261°3/4
A3: ϕ = 077°07′− 0°16′ = 076°51′ = 076°7/8

(rounding 51′.5− to 7°/8). Computing trio B likewise, using eq. 20 (& §L2's d values):

B1: ϕ = 352°00′ + 3°54′ = 355°54′ = 355°11/12
B2: ϕ = 180°41′ − 4°27′ = 176°14′ = 176°1/4
B3: ϕ = 340°28′ + 4°17′ = 344°45′ = 344°3/4

Most of these six opposition-matches to the corresponding solar positions (trio A §M10, trio B §L3) are as good (i.e., within a few arcmin of exact 180° elongation) as are comparable Sun-Moon longitude matches for the eclipses calculated in the *Almajest*. Except for the A1 lunar longitude (above vs. §M10). (And, of course, the 1° discrepancy in A3: §M3.) The A1 lunar longitude exhibits a serious disagreement with both the stated & computed A1 solar longitude — i.e., with one of the very longitudes which Hipparchos' eclipse calculation was supposed to match (but didn't, because A1 was not used in the calculation: eqs. 12 & 13). The original 1° slip for trio A (§M3, fn 162) probably occurred (from a needless borrowing) during the simple longitudinal subtraction, A3−A2. Note: had Hipparchos genuinely tried to extract (from trio A) all 3 parameters (ϵ, $g_\circ$, e), which Ptolemy & Toomer have previously understood was the case, Hipparchos would probably have noticed the 1° error in this work, since some longitude-comparison is necessary to find ϵ. (*Almajest* 4.6 finds the middle eclipse's mean longitude for 2 other trios, of Ptolemy's choice.) One presumes Hipparchos later noticed A3's 1° discrepancy and A1's 1°/8 mismatch. These problems (and the accidentally not-bad match of unused B3's computed solar & lunar longitudes) may help explain why Hipparchos' later work neglected the trio A-deduced e and instead used (Toomer 1967) the r gleaned from trio B. However, it appears (since Ptolemy mentions no explicit retraction by Hipparchos) that the trio A error was not publicly admitted, and both pretenses (to mathematical exploitation of *all* data of eclipse-trios A & B) were allowed to stand. This constitutes mathematical fraud. Ameliorating factors:

[a] The basis for our induction is a secondary account. [b] The incidents are twofold but otherwise isolated (as far as we know). (Compare items [a] &[b] to Ptolemy's massive frauds: *DIO 1.1* ‡5 fn 18.) [c] Hipparchos' fakes are mathematical, not empirical. Expanding on the last point: [i] Hipparchos did not (unlike a certain later devotee) fabricate the A1 ϕ to make it fit his A2&A3-based orbit. (This suggests that the main pretense on Hipparchos' part may only have been that he put his name on math work actually carried out by others. This is not the same as faking math, but: it is not a trifle. And his failure later to expose the 1° error, even after jettisoning trio A's e, suggests Hipparchos at least knew of the error but said nothing of it publicly; so he cannot be held unaccountable.)[235] [ii] His *Almajest* 4.11 theories (whatever their faults) *were based upon real observations*. And [iii] he kept trying to improve upon them — which virtue is just what led to the temporary confusion-inconvenience of his having 2 disparate lunar theories. The Muffia fails to understand an obvious related item: if we speak (as, e.g., at [Muffia 1990] p.216) of superior-intellect Ptolemy's preference for Hipparchos' star data over his own, we must ask what the basis for this preference was. Ptolemy's observations? *What* observations? (Same for non-existent Babylonian transit-circle observations for seasonlengths. See §E3.) The rockbottom fact is: *some* parties had to make founding observations in order to have any sort of empirical basis for ancient astronomy. But Ptolemy didn't make outdoor observations. (See, e.g., Rawlins 1985G §10.) That's the Muffia's "Greatest Astronomer of Antiquity" for you (§N16).

N16 And Hipparchos' own self-evident heavy dependence upon previous astronomers' work (revealed by his adoption of their lunar epoch-position values, as just demonstrated: §N11f) is consistent with his being an energetic but far from primary figure in the development of Greek astronomy. This is an apt moment to sample a typically unsupported recent effusion of one of the most academically powerful of the genii[236] that now bestride (politically) the field of ancient astronomy history (Toomer 1988 p.361, implying that Hipparchos' major debts were to Babylonian not Greek predecessors): "I have no doubt whatever that it was Hipparchos who changed Greek astronomy from a largely theoretical to a predictive[237] science. In this his originality and inventiveness are beyond question."

[235] We thus have perhaps 5 cases of questionable ethics here: [1] faked math for trio A, [2] same for trio B, [3] pretended (double) confirmation of previous astronomers' integral ϵ & $g_\circ$ (this sham is depressingly redolent of Ptolemy's M.O.), [4] probable intra-school semi-plagiarism (fn 253), & [5] failure to acknowledge 1° error in trio A after its later (probable) apprehension. These sins are inferior in rank to Ptolemy's; but they are not to be admired, either. The picture emerging here of Hipparchos' school is of a sloppier and more poorly supervised group of astronomers than was my previous impression. (Though, van der Waerden & DR have long held that Hipparchos was not himself a remarkably able mathematician: fn 287 — contra, e.g., Sarton 1959 p.65.)

[236] *DIO* jesteth not. As a MacArthur Fellow, Swerdlow is even certified. One recalls the touchingly hopeful 1979 popular coverage of the inception of the MacArthur Foundation, e.g., *Newsweek* 1979/8/13 p.50: "A Fund for Geniuses." The original MacArthur aim was to help bright & productive unfunded scholars outside the establishment. (E.g., Ted Heckathorn & Keith Pickering would be apt current choices.) Thus, a MacArthur grant to an already-generously-salaried centrist (a prime Muffia mout'piece for the [2nd] Greatest Faker of Antiquity) represents nearly as far & as ironic a fall as possible, from the original MacArthur ideal. The only thing more perverse would be to grant a MacArthur to an establishment hatchetman, who has cemented his exalted social position by defaming the very sorts of persons (e.g., R.Newton) whom the MacArthur grants were initially intended for. Fortunately, Swerdlow has never done anything like that.

[237] Since the very next page of Toomer 1988 agrees with Rawlins 1984A (see §G2 here) that the predictive hokum of astrology was not much of a factor with Greek astronomers before Hipparchos, Toomer's typically insightful conception of ancient astronomy presents a chronology that must inevitably encourage the too-popular notion that astrology's fascination with predictivity helped launch astronomy's. Rawlins 1984A tentatively argued the reverse, mainly on the ground that 3rd century BC Greek astronomers (with no known connection to astrology) did fine orbital work. This is obvious from, e.g., Rawlins 1985S, *DIO 1.1* ‡6 fn 1 & §B11, ‡7 fn 8 — as well as the current paper's finding that Hipparchos' adopted epoch values for both synodic & anomalistic lunar mean motions predate him. Indeed, one of them, $\Delta\epsilon$ = 310°1/3, introduced here at eq. 8, was carried unaltered, 3 centuries later, into Ptolemy (*Almajest* 4.2-4), pseudo-empirically "proved" by him from the usual suitably-fudged observations at *Almajest* 4.6-7. (Note R.Newton 1977 pp.122-123, and here at fn 218 & fn 223.) Ptolemy gets perfect fits to accurate pre-known elements, even while using Hipparchos' PH solar theory, which throws most of the eclipse intervals (for trios A&B) off by ordmag 1 hr! [Note added 1993: See, e.g., Britton 1992 pp.61-67.]

Thales? Schmales.[238] (Toomer 1988 n.48 says he is preparing a whole book[239] on Hipparchos, which will doubtless be founded upon similarly accurate speculations, posing as the intuitions of "**the** expert"[240] in this area.) Toomer's astounding misperception (perhaps original with the Muffia) of the state of pre-Hipparchan astronomy cannot be blamed on a recent stroke. He's been this smart all along. Toomer 1974D p.137: "I do not believe in the myth of a 'school of astronomy' at Alexandria in the Ptolemaic period [3rd century BC]. We can name some (very few) people who were at Alexandria at various times during that period and who made astronomical observations or calculations. But the notion that this activity was in any sense continuous is without supporting evidence." (Viewpoint loyally echoed at [Muffia 1990] p.6, with credit to Toomer 1978H.) A "few" observing astronomers in the 3rd century BC? If we wish to explore the lower limits of the word "few", let's ask: how many observers can we name from Ptolemy's time? (And: why must Ptolemy's Mercury theory depend so heavily upon observations of the 3rd century BC? — made by a "mythical" group of astronomers that was so organized that it even had its own "Dionysios" calendar: van der Waerden 1984-5 pp.125f.) Muffia disbelief-rejection of high astronomy (in the nonBabylonian world, at any rate) in the 3rd century BC — a fantasy also promoted by O.Gingerich — is one of the more amusing byproducts of worship of hyperhero C.Ptolemy, the Muffia's & OG's "Greatest Astronomer of Antiquity" (*DIO 2.1* ‡3 fn 28, *DIO 1.1* ‡6 §H7 & ‡7 §B2) and is typical of the warp & damage which that reverence has visited upon modern Hist.sci specialists' perspective on the state and evolution (and devolution) of ancient astronomy. (Students of the Darwinist debate will find Toomer's conveniently-selective "missing links" faucet-agnosticism all-too-familiar: similar reasoning infects most Muffia writing on ancient astronomy.) Such comedy is yet another benefit of Muffia-watching: the spectacle of a leading power in contemporary Hist.sci seriously proposing that astronomical predictivity was a novelty for 2nd century BC Greek astronomers! — as if the idea of ephemerides had never occurred to Kallippos,[241] Timocharis, Aristarchos, Aristyllos, Archimedes, Eratosthenes, Apollonios.[242] The complaints (of planet motion untamability) in Plato's *Republic* (7.4:528-530) suggest that Greek attempts at planetary tables went back at least to his time (4th century BC).

N17 The upshot of the foregoing math: we have at last, by the remarkable good fortune of the Hipparchan lunar data's survival (embedded in Ptolemy's criticisms at *Almajest* 4.11), discovered the Aristarchos lunisolar theory's lunar apogee-at-epoch: $A = 96°$.

[238] I believe this faithfully reflects the attitude of Neugebauer 1975 p.604. (However, see A.Aaboe *JHA 3.2*:105, 1972, & the sources cited at his n.3.) There is no question that the ability, to predict the detailed circumstances of a solar eclipse, did not yet exist in the 6th century BC. Our main information from Herodotos (1.74) is implicit: astronomical prediction was valued in Greece as early as his day (5th century BC).

[239] See Muffia-salesman O.Gingerich's pre-publication raview, which perfectly exemplifies the hypercritical attitude which seizes OG whenever he is publicly appraising the academic output of the powerful (*JHA 22.2*:187; 1991/5, same issue as Jones 1991H): Toomer 1988 is "brilliantly concise . . . . Toomer leaves us in a state of enthusiastic expectation, waiting for his promised book on Hipparchus." (See also *DIO 2.1* ‡6 §F1 & fn 18.)

[240] P.Huber to *DIO* 1991/10/1 (*DIO 2.1* ‡2 §H21). Boldface in original. (Huber's work appears regularly in Muffia collections, e.g., Berggren & Goldstein 1987 & Leichty, Ellis, Gerardi 1988.) But Huber omits to comment on Toomer's inventiveness. I am proud to point out that *DIO* cannot be accused of similar neglect: see, e.g., fn 24.

[241] B.Goldstein (Muffioso) even attacks the idea that Kallippos authored a calendar, asserting that his discovery was merely a cycle: Goldstein & Bowen 1989 p.285 & fn 190. Compare to Neugebauer 1975 p.627 & *DIO 1.1* ‡5 fn 13.

[242] Lunar tables due to Apollonios (c.200 BC) are cited by Vettius Valens (Neugebauer 1975 p.306 n.1). Toomer 1988 n.43 says that this reference "has been shown by [Jones 1983 p.31] to be due to a manuscript corruption of the name of the much later astronomer Apollinarius." (See §M7.) Consultation of the source (Jones 1983 pp.30-33) reveals not demonstration but speculation "with trepidation" — i.e., a once-explicitly-shaky speculation which has now been transformed (into Toomer's confident assertion), as the Muffia has gotten into aggressive public salesmanship of the grant-generating (fn 266) if magnificently improbable theory that the practice of making predictive astronomical tables was a late Babylonian legacy to the dreamy, previously untabulating Hipparchos-era Greeks! — a legacy suddenly absorbed 2 centuries *after* the Greek conquest of Babylon. . . .

I never expected to know this datum, which I have long wondered about.[243] It is not a bad value, but, as noted above (§N5), it is not remarkably near the truth, either. The pinnacle of accurate ancient lunar theory came later in antiquity. (See Rawlins 1985G §5 ¶3; also Rawlins 1985K: Venus & Mars.) Note that the errors in true longitude, caused by the ancient apogee-at-epoch A being c.5° in error, will only affect a monthlength estimate (based on eclipses 345 yrs apart: *Almajest* 4.2, Rawlins 1985S) by a fraction of a timesec. Aristarchos' $M_{\rm A}$ (eq. 6) is about this accurate, so the error in his A (recovered here) would not have noticeably affected his admirably correct deduction of $M_{\rm A}$ (Rawlins 1991H fn 1). A final reflection: had Hipparchos not shortcut-faked his eclipse math by adopting (and thus inadvertently preserving for us) Aristarchos' lunar apogee-at-epoch ($A = 96°$), that datum would have disappeared forever. So, since Aristarchos was a far more critical figure (than Hipparchos) in scientific history, Hipparchos' pretense here has the fortunate accidental rôle of being the cobweb-thin thread that now connects us to one of the most important of the hard-earned astronomical parameters of a great & genuine intellectual pioneer. This is among the strangest ironies of a history overflowing with them.

O Ancient Heliocentrists' Adoption of the Astronomical Unit

O1 It now remains only to explain the peculiar numbers which Hipparchos gave for the distance to the Moon, $r_{\rm M}$ (*Almajest* 4.11 & §N10): 3144′ (trio A) and 3122′1/2 (trio B). These values we took as already existing for Hipparchos when he computed eqs. 19 & 20. On the other hand, Toomer 1973 (p.16 & p.27 n.14) claimed that he had "conclusively" & "inevitably" established a very different explanation, from which (for each trio) these $r_{\rm M}$ values emerge during the calculation of e (or r). Toomer 1973 also proposes that, since his theory involves assumption of a relatively crude chord table (suggesting nascent trig), he has cast light into the murky origins of this wonderful mathematical device. His proposal is intelligent and intriguing — and sufficiently attractive that it took me a few days of stubborn testing before I abandoned it. (The chord table theory has a solid parallel, as convincingly shown by Toomer 1973 n.4 & p.24.) But one of this paper's prime bases has utterly collapsed (§G2 & §P1). (That inconvenient trifle does not, however, prevent the paper's unqualified citation — by Toomer 1988 n.44 & Jones 1991M p.443 n.5 — as evidence of Hipparchos' alleged pioneering of primitive early trig tables! We will return to examine this shambles at §P1. Meantime, we simply note that P.Tannery & B.van der Waerden have already persuasively argued against Hipparchos' invention of trig: fn 287.) After naïvely hopeful effort to salvage[244] the ingenious theory of Toomer 1973 (thinking such an outcome would give both of us a pleasant shock), I finally turned to search in other directions and almost instantly (same day: 1991/11/9) hit upon the solution, a crucial and amazingly fortunate relic of ancient heliocentrism's vitality — outside[245] the Hipparchos-Ptolemy succession of geocentrist-astrologers, those useful conduits (note innocent irony at Neugebauer 1975 p.943), whom the kneejerk anti-whiggists of modern Hist.sci consistently

[243] See Rawlins 1991H fn 1, which (implicitly) somewhat overpresumes the need for accurate A when determining accurate M. Whether $A = 96°$ is due to Aristarchos or Apollonios (or both or neither) is not now provable. However, we know that Hipparchos used Aristarchan material, and trio B is from Apollonios' era. Both Aristarchos & Apollonios worked on lunar theory, and the existence of the latter's lunar tables is directly attested: fn 242.

[244] Only by stepping back from the math details can one see an inherent weakness of the Toomer 1973 theory (as against DR's §O3 proposal of a small scribal error) for explaining *Almajest* 4.11's large $r_{\rm M}$ numbers, 3144 & 3122 1/2: these numbers differ by less than 1%, which Toomer 1973 *implicitly regards as just an amazing coincidence*. After abandoning the theory of Toomer 1973, my first realization was this oddity, which led right on to the watershed question: did these numbers perhaps *precede* Hipparchos' math, as against being (as Toomer has supposed) a product thereof. Now that the answer is found, I expect we'll all eventually realize the *a priori* improbability of the idea that *both* e (or r) and $r_{\rm M}$ were products of Hipparchos' math. Obviously, normal procedure would be to set some value for $r_{\rm M}$ at the outset (as Ptolemy himself does at *Almajest* 4.6, where $r_{\rm M} \equiv 60^{\rm p}$), and then express e or r in the units thus established.

[245] Actually, eqs. 23 & 24 show that heliocentrism's data were absorbed into Hipparchos' work. For a similar situation with Ptolemy, see §H3 & §O2.

mistake for the real scientists of antiquity. I should add that the import of heliocentrism in serious ancient astronomy has, from the outset (1976), been a theme[246] of DR's studies in this area. The idea of heliocentrists' rôle in 3rd century BC astronomy (establishing a tradition that carried down through Seleukos the Chaldaean[247] and into eastern astronomy) was earlier broached by van der Waerden 1970 (whose proposals were attacked in *Isis*, with classic Muffia surety & haughtiness, by Swerdlow 1973: see Rawlins 1991H fns 6 & 36). So the following surprise developments (eq. 23 & eq. 24) represent a remarkable — and wholly novel — double-success for this Muffia-resented, flagrantly whiggist viewpoint.

O2 We know that the Poseidonios school's distance[248] to the Sun was 10,000 Earth-radii, or:

$$r_S = 10{,}000^{e} \tag{21}$$

So, suppose heliocentrists such as Aristarchos scaled the universe (perhaps very much the way moderns use the Astronomical Unit) similarly, using 1000 units (not necessarily Earth-radii) = 1000^{a} for the solar distance:

$$\text{Astronomical Unit (Sun to Earth distance)} = r_S = 1000^{a} \tag{22}$$

We will call 1^{a} a milli-AU. Now, the traditional Aristarchan ratio of the Sun/Moon distance (also adopted by the geocentrist tradition: Delambre *Histoire de l'Astronomie Ancienne* 1817 vol.2 p.207, R.Newton 1977 p.199) is based on the half-Moon being 3° from quadrature — i.e., the transverse Earth-Moon line subtends[249] 3° as seen from the Sun. So, if the Solar System was based on this scale (eq. 22), then in the same milli-AU units, the Moon's distance r_M will be

$$r_M(\text{trio A}) = 1000^{a}\tan 3^\circ = 52^{a}24' = 3144' \tag{23}$$

This recovers[250] (to a precision of ordmag 10^{-4}) the precise Hipparchan number (3144 for trio A's e) cited at *Almajest* 4.11 (§D1, §N10) for the Moon's distance from the Earth.

O3 A hypothetical Muffioso, secretly reading *DIO*, is now about to (indeed is *required* to) protest that the eq. 23 shocker just unveiled: [a] is mere-coincidence (albeit a *very* long longshot) and [b] anyway-doesn't-fit-trio B. So, let's next unleash the remarkable clincher, as we now use *the same theory* to solve also trio B's superficially discordant Hipparchan r_M. (Compare this classic success, by the fruitfulness criterion of fn 85, to the earlier failures of DR, Toomer, & Jones: §D1, §D3, §F4.) One of the best known & peskiest types of scribal errors in Greek astronomy & geography is the confusion[251] of arcmins with

[246] See, e.g., *DIO 1.1* ‡7 §F3 (from 1976), DR to *DSB* 1978/5/18, Rawlins 1984A.

[247] Neugebauer 1975 pp.610-611, 697-698. In a 1985/4/13 letter to DR, van der Waerden stresses the often-overlooked point that the name "Seleukos" is Greek, not Babylonian.

[248] Kleomedes 79 (Neugebauer 1975 p.656 eq.16; R.Newton 1977 pp.179&181). [Heath 1913 p.348 suggests an Archimedean origin.] This figure entails (eq. 33) a solar radius of about $R_S = 44^{e}$, which has the right ordmag. Another (tenuous) suggestion, of high accuracy astronomy here, is the hint of ancient use of 60 stades/1°: Neugebauer 1975 p.656.

[249] The actual Aristarchos Experiment (*DIO 1.1* ‡7 fnn 5-6) would be most accurately described using sin 3°, not tan 3°. (See, e.g., Van Helden 1985 p.6 Fig.2.) But the standard ancient lunisolar diagram (*ibid* p.7 Fig.3 or *Almajest* 5.15) draws & computes similar situations using transverse lines shown falsely perpendicular to the Sun-Earth axis (instead of the correct way, e.g., Heath 1913 p.404, Neugebauer 1975 pp.641, 1354 Fig.13), thus effectively replacing sine with tangent. Swerdlow 1968 p.60 n.30 & Swerdlow 1969 p.293 nn.10-11 both regard as erroneous (rightly remarking that the difference is trivial anyway) the replacement of tangent with sine for the lunar angular semidiameter in such diagrams (see, e.g., S.Newcomb *Compendium of Spherical Astronomy* 1906 p.158 eq.20), even though sine is actually the correct choice. Which suggests that eq. 23 is a follower's version of Aristarchos' scheme, not the original. (This can be taken as evidence against Aristarchos' use of trig.)

[250] The mixture here of decimal & sexagesimal systems creates no difficulty, since such a hybrid system is used, e.g., throughout the *Almajest*. For 3rd century BC Greek celestial distances expressed in simple powers of 10, see Archimedes ("Sandreckoner" pp.226f).

[251] The least damaging example was the ubiquitous confusion of 1°/8 with 8′. This was virtually an ancient identity: two examples at §M10 here.

degree-fraction; e.g., Swerdlow 1979 (pp.527-528) savages[252] R.Newton 1973-4 (pp.112-113) for being misled by just such an error (a longago misreading of 344° 1/12 as 344° 12′). [Neugebauer 1975 p.166 n.3 requires such an ancient scribal error. Same error: *ibid* p.729 n.15.] Now suppose a member of Hipparchos' school, deputed[253] to calculate r from eclipse trio B (for which he would need r_M for use in eq. 18), started by misreading the eq. 23 distance $r_M = 52^{a}24'$ as: $52^{a}1/24$. This would transform distance r_M into:

$$r_M(\text{trio B}) = 52^{a}1/24 = 52^{a}02'1/2 = 3122'1/2 \tag{24}$$

Which recovers the *Almajest* 4.11 trio B value (§D1, §N10) for r_M, on the nose.

O4 Note that, without the good luck (for us, anyway) of Hipparchos' computer having made his fateful scribal slip, the above heliocentrist explanation (§O2) of the origin of 3144′ would not be utterly unevadable. Even though true. Fellow explorers will better empathize with my pleasure at discovering (1991/11/28) the confirmatory scribal miscue, if they realize that I had already (since 1991/11/9) come upon eq. 23 by theorizing that Hipparchos was (at this point in his career) drawing his astronomical scale (eq. 22) from real contemporary astronomers, who were, naturally, heliocentrists. Though I was confident[254] of heliocentrist eq. 23 anyway, the moment of confirmation — the finding of eq. 24 — was exquisite. I then knew positively that the hypothesis behind eq. 23, namely that heliocentrist work underlay all high ancient astronomy, was (§D2): "not just a beautiful dream. It was reality."

O5 Since scaling the Moon's distance by the Earth-Sun distance implies realization that the Sun was the central body for astronomers, the foregoing two astonishing matches (eqs. 23 & 24 vs. §P2) leave no doubt that genuine ancient astronomers: [a] used the Astronomical Unit, and [b] were heliocentrists. (To scholars of scientific history, it should be self-evident that geocentrism was as nutty to competent ancient scientists as to moderns — a point demonstrated in detail by "Figleaf Salad": *DIO 1.1* ‡7.) While it was perfectly natural

[252] Fn 169. To the open & detailed retraction at R.Newton 1977 p.130, Capt.Captious NCSwerdlow responds, by attempting to portray such refreshing behavior as dishonest! [Note added 1993: Britton 1992 p.xvi repeats this hilariously inverted slander. As a historical & probably verifiable fact, I can report that (contra Britton's unsubstantiated claim of "errors scattered throughout Newton's work" allegedly caused by his use of the Halma & Taliaferro editions of the *Almajest*): R.Newton had both the Manitius & Heiberg editions of the *Almajest* out of the JHU library for years on end. (At the start of his researches, RN indeed openly used primarily the Halma edition; but he henceforth consulted all available versions of the text.) Britton *loc cit* offers the usual Muffia capo gotta-go-now excuses for not providing "extensive references" to RN's allegedly copious resultant errors. The same Muffia pretense to apprehension of scores of serious RN mistakes has been going on for over 20^{y}: Swerdlow 1979 p.530 says the very few RN errors he alleges "could be multiplied twentyfold". DR's 1979/10/26 letter forcefully challenged NCS to prove it. NCS instead just repeated himself unquantitatively at HamSwerdlow 1981 p.61, still refusing to substantiate the big lie. See *DIO 1.1* ‡5 fn 6; also *DIO 2.3* ‡8 fn 61, which directly charges that "the Muffia klan has simply been bluffing in this regard", especially with respect to its capos' (unwritten) slanders against DR's accuracy. But Hist.sci's brave & just leadership still hasn't required that *2 decades* of Muffia libels be made good.] (Compare Newton's frankness to Toomer's §G2 behavior when upset by the very same problem: altered number-basis.) Thank heavens for NCS' alertness. Without the Muffia on guard, why, academe might become corrupt.

[253] Given the contrast between eq. 23 & eq. 24 (even accounting for the likelihood that they were produced at different times for different works: §N3): it is unlikely that the same person performed the lunar calculations leading to eq. 19 as well as eq. 20. (The Muffia also regards the trio A & trio B calculations as well separated events, e.g., fn 209.) More revealing: If Hipparchos understood the identity of the ultimate basis for both choices of r_M (eq. 23 & eq. 24), then he could not have innocently published two different versions of the exact same amount! (Had the bungled eq. 24 been published first, we could suppose that eq. 23 was set forth later to quietly correct the r_M given. But it is generally agreed that the reverse was the case: §N3. And trio B not trio A became the basis of Hipparchos' later work: §R1.) So the blunderful disparity, between eqs. 23 & 24, provides unexpectedly strong evidence for some historically intriguing realizations: [a] Hipparchos headed a stable of talent. (None of whose names are known to us. Did a now-anonymous Hipparchos-circle Kepler go on to produce work now glimmering in the fragments we have of Poseidonios' corpus? Was Poseidonios himself a Hipparchos protégé?) [b] The alertness & comprehension of Hipparchos' editing of his computers' work were about on the order of "Editor" Toomer's when he vetted [Muffia 1990].

[254] Nonetheless, in the 1991/11/14 first draft of this paper (laserprinted 11/25), DR felt obliged (before finding eq. 24) conservatively to call eq. 23 speculative. (DR's attitude in such matters is best gauged from his remarks at Rawlins 1985G p.255 last sentence, & here at §R10.)

for geocentrists to express the Moon's distance in Earth-radii, it was just as natural for heliocentrists to express the Moon's distance in AUs. A final point: since [a] Babylonians preferred sexagesimal expression for numbers over 59 (van der Waerden 1963 p.38, van der Waerden 1978 p.667), and [b] the base distance used for Hipparchos' lunar work was 1000[a] (a choice of round number which denotes a decimal system for large values), we know that the work behind the Hipparchan *Almajest* 4.11 material was not Babylonian (as suggested by Jonestown) but Greek.

O6 An overview reflection: there is a striking analogy between [a] the foregoing lunar findings and [b] the matter of the *Almajest* planet mean motions. For both problems, DR has found *perfect* fits to Greek data via Greek methods, as against Muffia and Moesgaard Babylonian solutions that *don't* fit the same Greek data. See tabular comparisons at §P2 for the lunar numbers, and *DIO 2.1* ‡3 §C3 (also §H3 here) for the planet mean motions. Notice also that Greek heliocentrism provides part of the solutions for *both* problems: eqs. 23 & 24 (distance of the Moon), and fn 129 (Mars mean motion; see also *DIO 2.1* ‡3 §C3).
[Note added 2003. DR had thought the Mars mean motion tables were based on an integral number of longitudinal (heliocentric) revs. The basis has indeed turned out to be longitudinal (contra Ptolemy), but not that way. See just irony at *DIO 11.2* ‡4 fn 21.]

O7 Besides the foregoing specifics, there is an implicit total-weirdness about Muffia&0 forcing a Babylonian step-function onto the *Almajest* 4.11 solar data — a contradiction that should immediately have impressed itself upon such proprietary experts, namely: the undeniable context. Question: What is Hipparchos' purpose in the work reported at *Almajest* 4.11? Answer: he was effectively trying to find the amplitude of the *trig* (Greek) syzygial equation of center for the lunar motion. Have any of the genii selling the solar-step-function Babylonian unicycle ever stopped for a moment to take in the implicit lunar-vs-solar incongruity? — i.e., is it credible that Hipparchos would use a crude Babylonian-*arithmetical* solar scheme for the mid-eclipse solar longitudes on which he bases the *highly error-sensitive* parameters of his Greek-*trig* lunar model? How did *JHA* & *Isis* end up accepting (and so readily publishing) a hypothesis that is about as credible as a painting that mingles subtle Rembrandt figures in with stilted-profile Egyptian ones?

O8 Historical note: when the *JHA* Editor-for-Life suppressed Rawlins 1999, His Lordship killed one of the key links (fn 9, fn 81) revealing Hipparchos' intimate debt to Aristarchos. Now that this debt has been independently verified here (eqs. 23-24), thus adding further credibility to the suppressed paper, I look forward to enjoying the *JHA*'s updated alibis for the original censorship (*DIO 1.1* ‡1 fn 25). They will be as honest as the rest of the *JHA*'s act.

P Basking Case

P1 From §N14 & eqs. 23-24, we have found the long-sought solutions to the four mysterious parameters set forth at the outset of our explorations here (§D1 item [b]): 3144′ & 327′2/3, 3122′1/2 & 247′1/2. The above solutions agree with the attested values, to the precision displayed, in all four cases. (Note that, even if a Muffia anti-heliocentrist-influence fanatic rejects the solutions for 3144′ & 3122′1/2, he must face the fact that the other 2 solutions stand on their own merits, being based merely upon entirely attested Greek astronomical math: §N12-§N14. Indeed, of the current paper's discoveries, this solution-pair and §N10's matching integral hits will present Muffia evasiveness its sternest challenges.)[255] Before proceeding further here, let us examine, for contrast, how closely Muffia capo Toomer's quarter-century of labors (§D1 & fn 116) has brought him to the same four numbers. His quasi-agreements are so compellingly semi-good that Toomer's typical

[255] Muffiosi will predictably resort to a chronology-argument (fn 234) for rejection (since there isn't any other ground for their foregone conclusion). But this will, typically, be mere opinion, not demonstration — though it will just as predictably masquerade as the latter.

self-satisfaction (see *DIO 2.1* ‡3 §A) is complete, and he — Muffiosi's **the**-expert on Ptolemy (fn 240) — basks in the glow of his genius (Toomer 1973 p.15): "our calculations have proven" his theses. (See also §O1.) From *Funny Thing Happened on the Way to the Forum* (1966 Plautus-based cinema musical),[256] one recalls the equally-modest Miles Gloriosus' appraisal, upon observing himself: "Even *I* am impressed." But Toomer's intricate & learned geometrical development, despite various arbitrary steps & nudgings, has never quite recovered any of Hipparchos' four numbers. His trio A investigation wasn't very convincing: $e = 338'$, & $r_M = 3134'$, vs. the attested (*Almajest* 4.11) numbers, 327′2/3 & 3144′. But the trio B search seemed to get within an ace of the *Almajest* 4.11 value r = 247′1/2, when Toomer 1973 pp.10-11 came up with r = 246′1/3. (His associated value for r_M was not so lucky: 3082′2/3 vs. 3122 1/2 attested.) But then, the entire trio B analysis of Toomer 1973 turned out to be founded upon a scribal error for the 2[nd] time-interval. (See Toomer 1984 p.215 n.75. This correction is one of numerous useful contributions Toomer 1984 has made to our knowledge of the *Almajest*. It is highly probable that without this fruit of the massive labors that went into Toomer 1984, I would never have been pushed away from the Toomer 1973 theory and fallen into eqs. 23 & 24.) I have recomputed Toomer's values, based upon the correction. The results: $r = 231'$ & $r_M = 3021'$. These are a long way from the numbers in *Almajest* 4.11: 247′1/2 & 3122′1/2. (For trio B, Toomer arbitrarily assumes an unlikely Hipparchos blunder, in order to get even that close; without this convenient step, his trio B's corrected value would be $r_M = 2916'$.) As noted above (§G2, §O1, fn 252): despite these flabbinesses & crumblings, Muffia scholars — including Jones & Toomer himself — continue to act as if this analysis has proved Hipparchos' use of the chord trig table that Toomer hypothesizes for him throughout the attempted reconstruction. Yet it is now all too plain that Toomer's entire intricate structure has been a castle in the air: a lovely, well-crafted, admirably imaginative fiction. But fiction, nonetheless.

P2 Let us tabulate the results of DR vs. those of Toomer for the four numbers of *Almajest* 4.11:

Parameter	*Almajest* 4.11	Toomer	DR
Trio A's e	327′2/3	338′	327′39″
Trio B's r	247′1/2	231′	247′30″
Trio A's r_M	3144′	3134′	3144′
Trio B's r_M	3122′1/2	3021′	3122′1/2

As students of cult behavior have already realized: the Muffia has painted itself into a *very* tall corner here, since it MUST continue to pretend that "Editor"-potentate Toomer's theory is correct. (This in spite of the fact that his solutions' agreements, with the four attested numbers, are worse than DR's by factors up to 1000 — and even ∞ in the final case.) But optimistic DR now lodges the confident prediction that Muffia sinuosity will prove equal to the seemingly insurmountable hurdle DR has here set before it.

P3 The main outcome may be: another (§D4) thrombus. Though Muffiosi will (at least privately) eternally try to chip away at the findings of this paper, they will (as also for the mean motions & Star Catalog logjams) *never* find generically different solutions that gel as neatly & are as precise in fit. (This for the simple reasons that: [a] the DR solutions accord with reality, so [b] Muffia complaints will be mere lawyering.) Thus, junk solutions (§H2 option [g]) must be invented to haze the void into a permanent fogjam. (Among fun-dividends of ungrabbable DR solutions to mysteries in this field: each forces the Muffia

[256] Lyrics excerpted here are by S.Sondheim. The "ancient" 1966 screenplay contains an unintentional anachronism, when a character worries about rubbing elbows with someone allegedly infected with a supposed Cretan plague. He asks: Is it contagious? Reply: Ever see a plague that wasn't? This line was a bad joke in 1966. Now, it's AIDS-lobby-approved gov't policy. In other words: it's still a bad joke.

to insist on the validity of its own patently inferior solution to the same mystery. Another way of putting it: I am, in effect, effortlessly compelling Muffiosi to insist upon going in the opposite direction from the truth — i.e., betraying their very profession. Not every scholar's detractors are so obligingly cooperative in thus destroying their own intrinsic credibility.) The same sort of thrombus has occurred with respect to, e.g., the *Almajest* 5.3&5 trio of Hipparchos lunar positions (§G7 & §I2), the *Almajest* mean motions (§D4 & fn 129), and Eratosthenes' precision (Rawlins 1982G). When correct solutions are blocked by political cholesterol-clog, then, inevitably, the flow of progress halts in the stricken areas.

P4 During the foregoing, I've tried to appreciate Toomer's erudition. And I'm tempted to sympathize with him, even though he's about as merciful[257] as his Funny-Forum mentor. But sympathy for Malignant T. Gloriosus would be wasted here, for an obvious reason: he will never know disappointment in this matter — because [a] his colleagues & hangers-on will emit no (audible) snickers, & [b] he will himself never accept the obvious preferability of the DR quadruple-success perfect-fit solution. Even if *direct ancient attestation* of the truth surfaced, he would spurn enlightenment. (*DIO* is not speculating. Indeed, two instances of Toomer's stubborn rejection of the plainest possible ancient testimony, have already been presented above: §I1 & §M7.)[258] You see, there *is* a positive side to possessing a robust hallucinatory capacity. *Forum*'s Miles Gloriosus: "*I* am my ideal."

Q Improved Estimates of Aristarchos' Distances to Sun & Moon

Q1 Because the eventual suppression of (public) ancient heliocentrism (*DIO 1.1* ‡7 §G2) was ultimately so successful, numerous scholars are unaware of the prominence Aristarchos enjoyed in antiquity. The incomparable Archimedes (in "Sandreckoner") speaks of him as if he is the most respected of astronomers. He and-or his followers are also cited by Hipparchos, Vitruvius, Plutarch, & Ptolemy. (Ptolemy's solar distance is obviously based on Aristarchos: §O2. For Pappos' late comments, see fn 220.)

Q2 Yet, despite the fact that modern freshman astronomy textbooks customarily illustrate Aristarchos' method of finding the relative distances of the Sun & Moon, convincing absolute distances have never even been approximated. (See, e.g., fn 262: 20^{e}!) Until now.

Q3 DR's emendation (fn 261) of pseudo-Aristarchos' infamous 6$^{\mathrm{th}}$ hypothesis, in accord with Archimedes' explicit testimony, permits a vast improvement in our estimate of Aristarchos' actual r_{M}. The key equation (based on the standard ancient eclipse-diagram, with the traditional assumption that Sun & Moon have the same[259] angular semidiameter) is well known.[260] For lunar distance r_{M}, solar distance r_{S} (both in Earth-radii), solar and lunar semidiameter θ, Earth-shadow semidiameter υ times larger than θ:

$$1/r_{\mathrm{M}} + 1/r_{\mathrm{S}} = (1+\upsilon)\sin\theta \qquad (25)$$

If the half-Moon occurs γ short of quadrature, then:

$$r_{\mathrm{M}}/r_{\mathrm{S}} = \sin\gamma \qquad (26)$$

[257] His calloused nonempathy with & sneering tactics toward seemingly powerless dissenters, reveal a less than attractive character. Both as regards ethics & simple sportsmanship. A cult, whose ancient hero stole others' labors at will (Rawlins 1987), naturally cannot be greatly disturbed at seeing a modern scholar's original work suppressed or misattributed for a trifle like half his career. However, that cult can hardly expect the robbed party to be entirely respectful under the circumstances.

[258] DR's approach is: give higher weight to attestation when it leads in unzany directions; lower weight, when it leads to incredible results. Muffiosi have a precious talent for inverting these rules. For several further examples of our respective viewpoints in action, see: fn 66, fn 162, fn 192, & *DIO 1.1* ‡7 fn 6.

[259] Ancients disagreed regarding whether to adopt this key condition for mean or greatest lunar distance from the Earth. See fn 273.

[260] E.g., *Almajest* 5.15, Swerdlow 1969 p.294, R.Newton 1973-4 p.382, Toomer 1974D p.130, Neugebauer 1975 pp.636-637, Van Helden 1985 pp.7&18.

Combining equations eq. 25 & eq. 26, we have (again in Earth-radii):

$$r_{\mathrm{M}} = \frac{1+\sin\gamma}{(1+\upsilon)\sin\theta} \qquad (27)$$

Q4 Aristarchos' estimate of γ, implicitly providing (via eq. 26) the ratio of the lunar distance to the solar, is reported as (Heath 1913 p.353):

$$\gamma_{\mathrm{A}} = 3^\circ. \qquad (28)$$

(See below: eq. 45.) And pseudo-Aristarchos' (poor) value of the Earth-shadow/Moon ratio is (*idem*):

$$\upsilon_{\mathrm{A}} = 2 \qquad (29)$$

Finally, the corrected[261] Aristarchos value of the lunar semidiameter:

$$\theta_{\mathrm{A}} = 1^\circ/4 \qquad (30)$$

Q5 Substituting the data of §Q4 into eq. 27, we have:

$$r_{\mathrm{M}} = \frac{1+\sin\gamma_{\mathrm{A}}}{(1+\upsilon_{\mathrm{A}})\sin\theta_{\mathrm{A}}} \doteq 80^{\mathrm{e}} \qquad (31)$$

where, again, 1^{e} = 1 Earth-radius. This entails (via eqs. 26 & 28)

$$r_{\mathrm{S}} = 1536^{\mathrm{e}} \qquad (32)$$

Since r_{M} is actually $60^{\mathrm{e}}.27$, eq. 31's result is about 1/3 high.[262] But it approximates[263] the earliest reconstructable empirically-based estimate of a celestial distance. (Ptolemy's later 59^{e} value is far, far better: *Almajest* 5.15.) Note: all of the data producing eqs. 31 & 32 are attested.

R Impure Speculations, Pseudo-Aristarchos' Fatal Contradiction, & The Muffia's Haute Cowture

R1 This next section will not be pure speculation. But it'll be near enough to justify the sectional title's lead here. To anyone familiar with our few scraps of information about Hipparchos' lunisolar work, the foregoing (eq. 31) should trigger recollection that Hipparchos' initial lunar distances in his own (now lost) work "On Sizes & Distances" were in the same range. Pappos says these distances were, for part 1 of the work: 77^{e} (mean distance) and 83^{e} (greatest distance). For part 2: $67^{\mathrm{e}}1/3$ (mean distance) and $72^{\mathrm{e}}2/3$ (greatest distance). It has long been known (Toomer 1967) that each ratio (greatest/mean) corresponds almost exactly to the r of trio B (§N10).

[261] See *DIO 1.1* ‡7 fn 6.

[262] Note, however, the enormous improvement of this estimate (80^{e}) vs. the terrible value (20^{e}) resulting if no correction is made to Hypothesis #6: Neugebauer 1975 p.637 eq.19. (Said improvement's approximation to Aristarchos' probable actual r_{M} was earlier pointed out by R.Newton 1977 p.392, whose analysis is otherwise not much like DR's.) Needless to add, if Aristarchos lacked trig, the calculation of eq. 31 would have been carried out less precisely, and the result would perhaps have been expressed as belonging between 2 geometrically-derived limits. However, the evidence against his having trig (e.g., Neugebauer 1975 p.638) is not much different from what could be pseudo-induced from much of I.Newton's published work, to prove the nonavailability of calculus for him. See fn 53 & fn 249. (P.Huber has made a similar comment to defend Ptolemy: fn 224.)

[263] [Note added 1992: An incompletely attested but more reasonable case can be made that Aristarchos' $r_{\mathrm{M}} = 60^{\mathrm{e}}$. See "Ancient Vision", to appear in a future *DIO*.]
[Note added 2008: "Ancient Vision" eventually appeared at *DIO 14* ‡2 (2008 March). See its eq.11.]

R2 All these data (§R1) are given by Pappos. (See Rome 1931-43 p.68, Swerdlow 1969 p.289, & Toomer 1974D p.127.) The same material is discussed (without data) at *Almajest* 5.11. Hipparchos evidently indicated that he derived the lunar parameters for his part 1 by analysing an undated eclipse that was total in the Hellespont but only 4/5 total (Kleomedes 95) in Alexandria. Toomer 1974D attempts to show that various able scientists' previous identifications of the eclipse are just the usual "worthless" effusions of inferior nonMuffia figures: G.Celoria & P.Neugebauer (no relation to ON) opted for the "Agathocles" eclipse of −309/8/15, while J.Fotheringham, S.Newcomb, & C.Schoch went for −128/11/20. Most of us have thought (even though none was a Hist.sci professional) that these men were among the finest astronomers & other experts to examine the question; but Toomer 1974D shows that they were morons compared to himself. (His suspicions of this went back at least to Toomer 1967.) Toomer gets even bitchier toward an appraisal by Robert Newton, who had just recently outraged the Muffia by showing that Ptolemy's data were largely faked. Thus, Toomer 1974D sneers at Newton's grammar[264] — in a swipe whose triviality self-nominates it for recognition as Prissiest-Criticism-of-the-Century. (We can almost[265] match it. See p.iv of [Muffia 1990], "Edited" by Toomer & published by Springer-Verlag, where the TEX-based Leslie Lamport system, which both [Muffia 1990] & DR use, has its trademark

LaTeX

rendered by [Muffia 1990] as

"LaTeX"

— like sumsorta newfangled Frenchie high-fashion, high-midnight cow-poke.) Toomer's same grammar-fussy footnote also scorns a helpful RN observation (fn 264). Compare all this to Toomer's simultaneous Autumn-Solstice calendaric brilliance (§B4).

R3 Thank heavens for Toomer's elimination of all those astronomers' unacceptable scholarship. But now (Wm.Tell Overture, please): Muffia to the rescue! Toomer 1974D masterfully analyses 6 eclipses (in order to show how a nonworthless scholar will choose the right eclipse): −309/8/15, −281/8/6, −216/2/11, −189/3/14, −173/10/20, −128/11/20. Hmmm. I find that the eclipses of −281, −216, −173 were not even close to total at the Hellespont (which I place at 40°N, 26°.2 W); so one may drop them from the sample at once. (Indeed, the −216 eclipse was probably nearer totality in Alexandria!) In order to deduce which eclipse best fits Hipparchos' likely deductions, Toomer 1974D: [a] computes with geocentric (not topocentric) latitudes & declinations (p.134), [b] tabulates no altitudes or magnitudes, and [c] uses a meridian diagram to choose between eclipses! (pp.131-135) — this despite the minor inconvenience that, for all 3 of the serious candidates (−309, −189, −128), mideclipse at Alexandria occurred nearer (in azimuth) to the prime vertical

[264] Evidently following the lead of (uncited) Gingerich 1972, Toomer 1974D n.13 treats an intelligent suggestion by R.Newton 1970 (pp.106-107) as idiotic. (It's revealing that both reviewers quote the same solitary passage from R.Newton 1970, when looking for a means to denigrate the book. Neither reviewer even explains his objection. Perhaps Toomer figured that OG must have had some reason for scoffing at the passage — and so just signed on, to share the credit for abusing The Hated One.) But Toomer improves on OG by attacking even the RN statement's English usage: "Hipparchus stopped work [sic] a year and a half after the eclipse of −128." Now, now, our little fangs are showing. If Toomer is out to sterilize alleged typos, let's *sic* His Immaculacy onto the Augean task of cleaning out the misprints all over [Muffia 1990], for which he was himself "Editor": see fn 149 & §I1. If one did not know better, one would assume that Toomer didn't read the [Muffia 1990] final text before it went to press. However: [a] the book's camera-ready copy was a TEX file; [b] the TEX Users Group is Toomer's Providence, RI, neighbor; [c] TEX is out of the Amer Math Soc; and [d] Toomer is an eminent Ivy League mathematician. He must therefore be completely at ease with the TEX system. (And, from Toomer 1984 p.viii & App.C, one is deeply — nay profoundly — impressed with Toomer's computer facility: why, of the 20 computer-generated calculations in Appendix C of Toomer's world-famous edition of the *Almajest*, fully 55% of the results are absolutely correct! See *DIO 2.1* ‡3 fn 18.) Thus, we know he had [Muffia 1990] right up there on his monitor. To borrow a fn 63 word from Toomer: "inexplicable".

[265] See also *DIO 2* ‡1 §A7.

than to the meridian. For the most popular contenders rejected in this fashion by Toomer: the −309 mideclipse was virtually due east (azimuth 100°±4°), and the −128 mideclipse was only about 8°±4° above the horizon. (The −216 Alexandria mideclipse was probably below the horizon.) Only the Muffia could resort to meridian math under these conditions[266] — all the while quarreling with astronomers' linguistic usage . . . .

R4 According to my calculations,[267] the 3 eclipses that merit examination (i.e., those that were virtually total near the Hellespont) had linear magnitudes[268] at Alexandria of: 76% ± 1% (−309/8/15), 90% ± 4% (−189/3/14), 77% ± 3% (−128/11/20). Thus, for his purposes, Toomer's selection, −189, is probably the worst of the 3 candidates. (His choice is still routinely promoted by Muffiosi; see, e.g., Neugebauer 1975 p.316 n.9, Toomer 1984 p.244 n.38, & Van Helden 1985 p.11.) Facts:
[1] The −189 eclipse's actual magnitude was probably the farthest (of the 3 candidates) from the observed value (80%).
[2] The direction of the O−C error would (if the erroneous magnitude 80% were used in an otherwise accurate Hipparchos calculation) lead to a too-small Hipparchos value for $r_{\rm M}$, not too-large — as is the case here: $77^{\rm e}$ (Hipparchos) vs. 56-$57^{\rm e}$ (real lunar distance during these eclipses).
My favorite Toomer pronouncement[269] has always been the Toomer 1975 p.196 appraisal

[266] It's delightful to see Springer's current series of amazingly expensive books, "Studies in the History of Mathematics and Physical Sciences", in such discerning hands as those of "Editor" Toomer — who is in truth editing this series with as much care as Editor-for-Life Lord Hoskin applies to the *JHA*. And what a lift to realize that, unless one assents to such genii's tenaciously-held creeds, one will be shut out of ancient astronomy conferences, grantfunds, publication, etc. A recent assessment (in another area) may throw light on the actual political & managerial logic behind this situation (*Isis 82.3*:601; 1991/9): the creation of the Space Telescope "demonstrates the requirement for near unity in the relevant scientific community for such great projects to succeed". (Comment upon the implications for dissenters would be tautological.) Extrapolating to the current quest for funds to support the grand Muffia project of recovering (Seleukid-era) Babylonian tinkertoy astronomy: Muffiosi hotly resent having an external scholar exposing late Babylonian astrology's crudity, degeneracy (devolving from skilled Greek astronomy), non-empiricism, & exclusively indoor-horoscope-application (§E3). (Question in passing: why do Muffiosi find it so tolerable, as in Jones 1983, to trace Greek influence in Indian astrology — but not in Babylonian?) I.e., open controversy is seen as simply: bad-for-business. (Ironically, *DIO* supports gov't funding for the Muffia needy. After all, a sacred mission of the *J.HA* is to show just how desperate this cult's plight is.) The underlying reality of the Muffia's strange fixation on hyperpuffing the rôle of Babylonian mathematical astronomy is simply this: while very few Greek astronomical papyri survive, there are lots of extant Babylonian astronomical cuneiform texts which ought to be joined & translated. (Isn't it a provocative coincidence that the alleged ancient Babylonian progenitors of Greek math astronomy happened to write on a material more durable than papyri? Had Greek astronomical texts survived in numbers comparable to Babylonian, would modern scholars be accepting so readily that Babylon was father to great Greek astronomy?) Thus, it is wonderful that Muffiosi wish so to spend their time. And, one understands that grantsmanship must justify this task. (Muffiosi abhor fraud charges against Ptolemy for a related reason: it's tough raising grants for research in a field whose central document acquires notoriety as a fake.) But let's cool the exaggerations (e.g., fn 242) and commence exhibiting some deference towards truth-in-advertising (*DIO 2* ‡4 fn 14).

[267] For adopted Earth-spin acceleration, see *DIO 1.1* ‡5 fn 11.

[268] The uncertainties cited correspond to allowing 1/4 hour (3° 3/4) of uncertainty in the Earth's rotational position. The Alexandria magnitudes of the other 3 eclipses: 55%±2% (−281), 90%±3% (−216), 72%±8% (−173). Their Hellespont magnitudes: 83%±1% (−281), 83%±3% (−216), 86%±7% (−173).

[269] Still undead at fn 166. My 2nd-favorite Toomerism is his blanket-libellous characterization *in a standard Hist.sci reference work* (the *DSB*: Toomer 1975 p.201) of Ptolemy-skeptics as "incompetents". (For a wider spectrum of Muffia compliments, see fn 31.) No Hist.sci leader has publicly recorded the slightest disapproval. (Indeed, as we saw at fn 172: when objections were laid before Hist.sci superarchon H.Woolf, Toomer got carte blanche to continue behaving as high-handedly as he pleased.) When pointing out such behavior to the *DSB* in 1978, I proposed a private get-together, which some archons may eventually (if not already) wish had come to pass. DR to Princeton's C.Gillispie (Editor *DSB*) 1978/5/18: "I could be in Princeton at a pre-specified time in early June to meet GJT [Toomer] and co. before a *DSB* panel if you want to find out which side is making sense in this controversy. [Robert] Newton has recently informed me by letter that he would also be willing to meet such a body." Gillispie (former Pres. History of Science Society) 5/24: "it would not seem to me appropriate to ask Professor Toomer to come to Princeton." Understandable. The offenses were merely censorship & slander; so what's the concern? And when R.Kargon (sometime *Isis* personage), and privately no more a Muffia-admirer than Gillispie, was shown N.C.Swerdlow's sweet 1983/6/2 letter (*DIO 1.1* ‡3 §D2-D3) to physicist R.Newton, calling him a "con man", Kargon just shrugged: "well, maybe he is." (The letter was also sent 1983/8/12 to *Isis*, whose reaction was to ban DR's criticism of Swerdlow's solstice folly: §I13 item [a].) When it comes to pristine housecleaning, Hist.sci could instruct Congress.

(characterized by a restraint well known to MadAve devotees) of faker C.Ptolemy's "brilliance . . . In his mastery of the choice and analysis of observations in conjunction with theory he has no peer until Kepler." Shall we now amend to: "until Toomer"?

R5 Given our previous findings here (§N), we are not bound to accept that the basis for Hipparchos' figures are those stated.[270] (Though, I have not the courage or "brilliance"[271] to speculate so imaginatively on Hipparchos' unstated methods as Jones 1991H has done.) We have already noted (§R1) the proximity of the 77^{e} value to our newly reconstructed Aristarchan value (eq. 31). This suggests that a calculation similar to that of §Q (leading to eq. 31) could be the true source of the 77^{e} value (of Hipparchos' "Sizes" part 1) — especially given the prominence of Aristarchos' work (§Q1). Now, the slightest testing will show that the later (Hipparchos or Ptolemy) values for shadow-ratio υ will (if used in eq. 25 with any anciently-attested θ) require ludicrous values for γ (well over 10°!) to produce r_{M} = 80^{e} — thus, we must resort to the Aristarchan value (eq. 29): $\upsilon = 2$. This suggests we also try the Aristarchan $\theta = 1^\circ/4$ (eq. 30), and presume that only the solar parallax was altered in the Aristarchos scheme. (Variation of parallax is all that is spoken of at *Almajest* 5.11.) Pappos & Ptolemy (sources: §R2) give some hints as to what new parallax was used. My sense of their descriptions is that it was imperceptible but not null. (*Almajest* 5.11: "no perceptible parallax".) Now, we recall that Poseidonios proposed a solar distance (eq. 21) which entails a solar parallax of between $0'.3$ and $0'.4$, which is indeed the limit of naked eye vision, as I know from personal testing. Setting negligible diurnal parallax = 10^{-4} radians makes the Sun's lower-limit distance = $10{,}000^{\mathrm{e}}$ (eq. 21);[272] and thus (from eq. 30) the Sun's radius is:

$$R_{\mathrm{S}} = 10{,}000^{\mathrm{e}} \sin(1^\circ/4) = 44^{\mathrm{e}} \tag{33}$$

(This is not a bad crude estimate, since the correct R_{S} is 109^{e}. See fn 248.) Since Poseidonios wrote in Rhodos (not long after Hipparchos' work on Rhodos), his solar distance, $10{,}000^{\mathrm{e}}$, is as likely as any to have been used. Substituting it and eqs. 29 & 30 into eq. 25, we have (using 100%-attested ingredients):

$$r_{\mathrm{M}} = 1/[(1+\upsilon)\sin\theta - 1/r_{\mathrm{S}}] = 1/[3\sin(1^\circ/4) - 1/10{,}000] = 76^{\mathrm{e}}59' \doteq 77^{\mathrm{e}} \tag{34}$$

— which is just the value Pappos cited as the mean[273] lunar distance in Hipparchos' preliminary work (§R1).

R6 We now turn to the final value (§R1) which Hipparchos deduces for the lunar distance (part 2 of "Sizes"):

$$r_{\mathrm{M}} = 67^{\mathrm{e}}1/3 \tag{35}$$

— which is (unlike 77^{e}) pretty consistent with the Hipparchos shadow-ratio for mean distance (*idem* & fn 273) attested in *Almajest* 4.9:

$$\upsilon_{\mathrm{H}} = 2\,1/2 = 5/2 \tag{36}$$

[270] Pappos indicates that Hipparchos' lunar distance estimates were based on the Hellespont-Alexandria solar eclipse discussed here. Unlike Pappos' account, that of *Almajest* 5.11 does not definitely say that this calculation was based upon an eclipse. Incidentally, if the eclipse referred to by Pappos was that of −309, then it may be that Hipparchos was using an analysis of it by another astronomer, such as Timocharis or Aristarchos.

[271] Toomer 1988 n.25. And see fn 61 here.

[272] [Note added 1992: Aristarchos evidently chose his lower-limit 10,000 AU distance to the sphere of the fixed stars by just the same uniform 1/10000 rad vision-limit underlying all ancient heliocentrists' cosmic-scale estimates: [a] the foregoing (10,000 AU), [b] the half-Moon experiment (eq. 45 & perhaps eq. 46), and [c] eq. 21. (See "Ancient Vision", in [*DIO 14* ‡2 (2008)].) It is a matter of record that Aristarchos wrote on vision & light: Heath 1913 p.300.) If so, then Poseidonios presumably agreed. The 10,000 AU estimate is far better than the geocentrists', but it is still much too small: the distance to the nearest star outside the Solar System is over a quarter million AU.]

[273] *Almajest* 5.14 says that Ptolemy's "predecessors" used standard eq. 25 (i.e., set lunar & solar semidiameters equal) for mean distance, while Ptolemy (implicitly declaring annular eclipses impossible) uses it for greatest distance. See fn 259. Heath 1913 p.383 n.2 might suggest that Aristarchos could have agreed with Ptolemy, whose γ is so nearly Aristarchos'. If so, then a speculative route to explain the 1st Hipparchos greatest distance of §R1 (83^{e}) is: use $\gamma = 5^\circ$ in eq. 26 with eqs. 29 & 30, which yields $r_{\mathrm{M}} = 83^{\mathrm{e}}.053$. I mention this possibility largely just to cover all bases — to somewhat discourage Muffia reflex-options [f]&[g] at §H2.

Almajest 4.9 also says that Hipparchos used a curious mean-distance lunar semidiameter:

$$\theta_{\mathrm{H}} = 360^\circ/1300 = 18^\circ/65 \tag{37}$$

Previous investigators (most recently Swerdlow 1969) have proposed ingenious theories relating eq. 35 to other assumed numbers (e.g., eqs. 36-38). Swerdlow 1969 makes a strong case that Hipparchos adopted (using $3438'$ = 1 radian) a solar parallax of $7'$, thus Hipparchos' solar distance was:

$$r_{\mathrm{S}} = 3438'/7' \doteq 491^{\mathrm{e}} \approx 490^{\mathrm{e}} \tag{38}$$

But no prior scholar has yet been able to: [a] explain the origin of the weird expression in eq. 37 or [b] recover exactly $67^{\mathrm{e}}1/3$ from normal[274] computation. I will next hypothesize an explicitly speculative route which can explain how these parameters came into being.

R7 Whereas Aristarchos' value for the lunar semidiameter θ (eq. 30) is about two times more accurate than eq. 37 (Hipparchos), his alleged shadow-ratio estimate (υ_{A}: eq. 29) is crude — far less accurate than Hipparchos' υ_{H} (eq. 36). So, though *Almajest* 4.9 mentions eqs. 36 & 37 together,[275] the empirical[276] impetus to revise υ would be much greater than to mess with θ. Suppose, then, that Hipparchos at first adopted just eq. 36 (his best move, accuracy-wise, as just noted). A Hipparchan choice of γ which was more accurate than that of either Aristarchos or Archimedes ("Sandreckoner" p.223: γ = c.2°) was first highlighted by F.Hultsch (Toomer 1974D p.140): Theon of Smyrna said Hipparchos had the Sun's volume = 1880 Earths, while the Moon's volume = 1/27 Earths. Refining the argument a bit, I will first compute (from eq. 26):

$$\csc\gamma = r_{\mathrm{S}}/r_{\mathrm{M}} = \sqrt[3]{1880\cdot 27} \doteq 37 \doteq \csc 1^\circ 33' \tag{39}$$

Next, combining eqs. 26, 27, 30, 36, 39, we have:

$$r_{\mathrm{S}} = \frac{\csc\gamma + 1}{(1+\upsilon)\sin\theta} = \frac{\csc 1^\circ 33' + 1}{(1+5/2)\sin 0^\circ 15'} \doteq 2486^{\mathrm{e}} \approx 2490^{\mathrm{e}} \tag{40}$$

(Note: without eq. 39's rounding of γ to whole arcmin, eq. 40 yields $r_{\mathrm{S}} = 2489^{\mathrm{e}}59'$.) This is the very r_{S} Hultsch proposed was originally stated by Pappos, a value later rejected[277]

[274] The ploy of Swerdlow 1969 p.298 bottom (use of 490^{e} in numerator & denominator instead of cancelling it: §R7) is artful but somewhat abnormal. However, it is not at all impossible, since the steps displayed could have occurred in several successive stages of math development. And see fn 277 (which Swerdlow *loc cit* anticipates in a general way).

[275] This important connection is the strongest point in favor of Swerdlow 1969 — overcoming the negative items noted at *DIO 1.1* ‡5 fn 7.

[276] Hipparchos' *Commentary* (Manitius ed, pp.90-91, quoted by Jones 1991M p.449) states that already-existing eclipse predictions were only off by 2 digits at worst, which is better than one would get by using eqs. 29 & 30.

[277] Swerdlow 1969 n.4 is commendably clear that there is a discrepancy in mss between "490" and "[space] 90"), adding: "This disagreement doubtless strengthened the suspicion of textual corruption." (Unfortunately, this Swerdlow note consistently uses the Greek symbol for 6 where he intends that for 90.) I must emphasize that there is no direct attestation of 2490^{e}. That is just Hultsch's proposed emendation (long-accepted, before Swerdlow 1969), simply on the basis of multiplying eq. 39 times eq. 35, which yields 2490^{e}. (Note: the resurrection of the same figure in eq. 40 is from a different direction.) It should be added that there is here an internal contradiction (to which Swerdlow 1969 is exceptionally immune), namely: one may easily compute the angular size of the Moon (similarly for Sun) from its size ($1^{\mathrm{e}}/3$, §R7) and distance (eq. 35), yielding arcsin(1/202) = $17'01'' \approx 17'$. This is a neat rounded number, but it is not the value used in eq. 40, by which 2490^{e} was recovered here. (However, it does suggest an alternate path to $r_{\mathrm{M}} = 67^{\mathrm{e}}1/3$, namely: round the Moon's radius R_{M} to simply $1^{\mathrm{e}}/3$, and round its semidiameter in eq. 37 to the nearest arcmin so that $\theta_{\mathrm{H}} = 17'$, which is arccsc 202; thus, $r_{\mathrm{M}} = R_{\mathrm{M}}/\sin\theta = 202^{\mathrm{e}}/3 = 67^{\mathrm{e}}1/3$ precisely. The common factor $1^{\mathrm{e}}/3$ in both R_{M} & r_{M} makes this an attractive theory. It accords nicely with — and I hope boosts the credibility of — Swerdlow 1969.) Thus, the hypothetical development here (§R7, leading to eq. 40) implicitly presumes that Theon of Smyrna's material indicated only approximately $r_{\mathrm{M}} = 1^{\mathrm{e}}/3$, and that, after he multiplied this rough figure by 37 (eq. 39), yielding about $12^{\mathrm{e}}1/3$, he cubed it and got c.1880 Earth-volumes, the figure Theon cites (§R7). Thus, to sidestep contradiction, all I am drawing from Theon's data is the ratio 37 (eq. 39). It should be noted that the same small internal contradiction (discussed in this fn) also resides in the Theon & Kleomedes calculations of Toomer 1974D p.140, but Toomer does not notice this.

by Swerdlow 1969. From eqs. 40 & 26, one finds:

$$r_{\rm M} = r_{\rm S} \cdot \sin\gamma = 2490^{\rm e} \sin 1^\circ 33' = 67^{\rm e}21' \doteq 67^{\rm e}1/3 \qquad (41)$$

By contrast, the development of Swerdlow 1969 p.298 (without some forced procedures: fn 274) produces not $67^{\rm e}1/3$ but $67^{\rm e}1/6$ or $67^{\rm e}1/5$ (as acknowledged by Van Helden 1985 p.13). This discrepancy led to the nimble Capt. Captious manipulations which are displayed & admired at *DIO 1.1* ‡5 fn 7.

R8 An alternate route to $67^{\rm e}1/3$: starting with the attested Hipparchos solar volume 1050 Earths (Kleomedes 83), we take the cube root and have

$$R_{\rm S} = 10^{\rm e}1/6 \qquad (42)$$

Thus, applying eq. 30,

$$r_{\rm S} = R_{\rm S}/\sin\theta_{\rm A} = 2330^{\rm e} \qquad (43)$$

Now, bringing in eq. 25 & eq. 36, we find

$$r_{\rm M} = 67^{\rm e}22' \qquad (44)$$

which ancients would most likely round to $67^{\rm e}1/3$, the value (eq. 35) attested by Pappos. (Of course, it is possible that eq. 42 was not the source of eq. 35. Perhaps eq. 35 led to eq. 42.)

R9 We now pause to ask: whence came the crucial (eq. 39) factor, 37? (It presumably appeared before the development set forth here in §R7. Keep in mind that we really don't have to justify the finding that $\csc\gamma = 37$, since it is effectively attested by Theon.) A guess at its origin: Aristarchos' Experiment depends critically upon the observer (of the half-Moon's occurrence) detecting lunar terminator-curvature.[278] Now, the mean distance between the human eye's foveal cones is (Rawlins 1982G p.263) no less than $0'.4$, and this is very near the actual limit of naked-eye discriminatory ability, which is about $1'/3$ or 1/10,000 rad (§R5). If the middle of the terminator deviates by twice as much (that is, $0'.8$) from the straight line (really: great circle) connecting the Moon's horns, then no part of the terminator will be more than $0'.4$ from a straight line parallel to the hornline & bisecting the tiny midperpendicular connecting the hornline & the terminator's middle. Understand: this minuscule angular distance is the crucial basis of Aristarchos' Experiment; i.e., the $0'.4$ is all that visually distinguishes Aristarchos' $r_{\rm S} \doteq 19 r_{\rm M}$ from $r_{\rm S} = \infty$: §R10. For, if $r_{\rm S} = \infty$, the lunar elongation complement γ corresponding[279] to the slight terminator-curvature just described is (for Aristarchos' value of the lunar semidiameter, eq. 30):

$$\gamma_{\rm A} = \arcsin\frac{0'.8}{\theta_{\rm A}} \doteq \arcsin\frac{1}{19} \doteq 3^\circ \qquad (45)$$

— which agrees with eq. 28. This suggests a possible source for the 3° figure, founded on the speculation of *DIO 1.1* (‡7 fn 6 — discussed here at fn 220) which questions whether the extant "Aristarchos" ms was directly written by him.

[278] Presuming the original work of Aristarchos contained a discussion of this key point: the confused, self-contradicting, & rather pointless pseudo-Aristarchos justification of Proposition 4 may be a "garbled" (to quote Neugebauer 1975 p.639) remnant thereof. The fact that this Aristarchos-influenced ms speaks here of the limit of human vision's fineness suggests that eq. 45 is surprisingly impure speculation.

[279] Likewise, if $r_{\rm S}/r_{\rm M} \doteq 19$ (the traditional Aristarchos-Ptolemy ratio), then the quadrature Moon will be slightly gibbous, with a terminator-curvature bulge of $0'.8$ (beyond the straight line connecting the lunar horns).

R10 Non-disconfirmatory (fn 254) in this connection: there is a lethal internal contradiction within the pseudo-Aristarchos ms — a contradiction which has eluded at least 1 2/3 millennia of commentators, from Pappos to the Muffia. The Proposition 4 discussion conjures up 1/3960 of a right angle (Heath 1913 pp.369-370); this amount, which equals $1'.4$, is then called "imperceptible" to the human eye (Heath 1913 pp.370-371, Neugebauer 1975 p.640). However, comparison to eq. 45 shows that this "Aristarchos" judgement implicitly destroys Aristarchos' own immortal 3° result for γ. The Aristarchos Experiment depends critically upon detecting whether a half-Moon occurs near [a] null γ, which corresponds to $r_{\rm S} = \infty$, or [b] his famous $\gamma = 3^\circ$, which corresponds via eq. 26 to $r_{\rm S}/r_{\rm M} \doteq 19$. Such fine discrimination requires vision good to $0'.4$ (§R9) — i.e., much smaller than the $1'.4$ angle which pseudo-Aristarchos claims is visually imperceptible!

R11 We now speculate that, at a later date, an Aristarchos follower revised eq. 45 by deciding that the angular length of the tiny midperpendicular of §R9 would be the amount to be visually detected, using the original $0'.4$ visual-limit criterion. Eq. 45 thus becomes:

$$\sin\gamma_{\rm T} = \frac{0'.4}{\theta_{\rm A}} = 2/75 \qquad (46)$$

— and, from that result, a heliocentrist member of the Aristarchos school could derive the Sun/Moon distance ratio in terms of the milli-AU (discovered above at §O2) substituting eqs. 22 & 46 into eq. 26, which gives:

$$r_{\rm M} = r_{\rm S} \cdot \sin\gamma_{\rm T} = 1000^{\rm a} \cdot 2/75 \doteq 27^{\rm a} \doteq r_{\rm S}/37 \qquad (47)$$

The Moon's distance $r_{\rm M}$ is thus deduced as $27^{\rm a}$ — or 1/37 of $r_{\rm S}$. That completes the tentative tracing of the ratio adopted previously at eq. 39, on our way to finding the attested value $r_{\rm M} = 67^{\rm e}1/3$ (eq. 41) — which we now return to, for the final member of our (speculative) reconstructions here.

R12 We have already seen (e.g., §N7) that Hipparchos was capable of attempting (mathematically unjustifiable) piecemeal improvement of his orbital elements. This suggests the next step in the evolution of his lunisolar theory: adopting a solar parallax of $7'$ (Swerdlow's discovery),[280] he changed his $r_{\rm S}$ to the value of eq. 38, but did not wish (perhaps because[281] his tables had been reared upon them) to change either his $r_{\rm M}$ (eq. 41 or eq. 44) or υ (eq. 36). Thus, the only means of assuring a fit to eq. 25 would be to: alter[282] semidiameter θ. I.e., the unknown being sought in the basic equation (Swerdlow 1969 p.298 bottom, or eq. 25 here) may have been not $r_{\rm M}$ (already known from eq. 41 or eq. 44) but $\theta_{\rm H}$. This is the (hypothetical) sequence that has not been previously suspected. Merging eqs. 25, 36, & 38:

$$\sin\theta_{\rm H} = (1/67^{\rm e}20' + 1/490^{\rm e})/(1 + 2\,1/2) \doteq 1/207^{\rm e} \qquad (48)$$

We now evaluate π in ancient coinage (with $1^{\rm p} \equiv 1/60$, as in §G10), using the following simple expression (which is accurate to better than 1 part in 40,000):

$$2\pi = 377^{\rm p} \qquad \text{or} \qquad \pi = 13 \cdot 29/120 \qquad (49)$$

[280] Despite my dismay (*DIO 1.1* ‡5 fn 7) at some of Swerdlow's procedures (when he tried to overforce desired agreement), I have long been sympathetic to Swerdlow 1969 (even though its implicit γ is remarkably outsized — a problem I attempt to overcome here at §R14). In a phone chat of 1981/4/5 (*DIO 1.1* ‡3 fn 7), I told Swerdlow that both the sign & size of the mean errors in Hipparchos' equinoxes (errors which degraded his PH solar theory's equation of center, making it worse than Kallippos') were pretty consistent with Hipparchos' having reduced his observations using a $7'$ solar parallax correction. Swerdlow said he already knew about that. [See DR to *Centaurus* 1977/3/9 p.B11.]

[281] If different computers at his school were involved at different stages of his alterations of his lunar model, then coherence will be hard to come by. Which is one more reason why I regard most of the theories discussed in this section as speculative — except for Swerdlow 1969, which deserves better ranking than that, by the coherence criterion. See fn 275.

[282] I see that Hartner accused Ptolemy of very similar inverse procedure at *Almajest* 5.14: Van Helden 1985 p.19.

The sine of a slim angle is virtually equal to that angle in radians, so we use eq. 49 to rewrite eq. 48. Now, 207·29/60 = 6003/60 = 100.05 ≈ 100; so we have:

$$\theta_{\rm H} = 180^\circ/(207\pi) = (360^\circ/13)\cdot 60/(207\cdot 29) \doteq 360^\circ/1300 \qquad (50)$$

which accounts perfectly for Hipparchos' otherwise inexplicable (& relatively inaccurate: §R7) value, $\theta = 360^\circ/1300$ (eq. 37). (Note that the odd factor 13 in eq. 37 is now explained as merely a byproduct[283] of the proper use of a sexagesimal expression for π: eq. 49.)

R13 Assuming that Hipparchos did indeed adopt Swerdlow's proposed $r_{\rm S} = 490^{\rm e}$, we are left with the discordant Hipparchan value, $R_{\rm S} = 10^{\rm e}1/6$ (Kleomedes: our eq. 42). Toomer 1974D has shown (though see fn 277) that if we also use $R_{\rm M} = 1^{\rm e}/3$, and thus (from eq. 26) get $\sin\gamma = 2/61$, and then use these data along with eqs. 36 & eq. 37 in eq. 27, we have $r_{\rm M} = 61^{\rm e}$, which is close to the truth ($60^{\rm e}.27$). (For the Theon-based $R_{\rm S} = 12^{\rm e}1/3$ of fn 277, Toomer similarly finds about the same $r_{\rm M}$.) It would be nice to suppose that this was Hipparchos' final value, but [a] we don't know so, and [b] it seems unlikely that he would jump back and forth from low solar parallax to high and then back to low.

R14 A conjecture: Hipparchos finally settled on his 7′ solar parallax — *because* it made the Sun about as small as scholars could possibly accept. Obvious ocular evidence was against it (as noted at *DIO 1.1* ‡5 fn 7 item [a]), since it entailed (from eqs. 26, 35, & 38) half-Moons at elongation c.82° ($\gamma = 8^\circ$ Sunward of quadrature) — while in fact (as I know from repeated outdoor experiment) the Moon is definitely crescent-looking at this elongation (or even for $\gamma = 5^\circ$). However, perhaps desire[284] (assisted by aging eyesight) pushed ever-upward Hipparchos' estimate of γ: as a geocentrist astrologer (even if he perhaps hired heliocentrist computers: §O5), Hipparchos would prefer a smaller Sun because Aristarchos' estimate made it over 100 times the Earth's volume. But, at a distance of $490^{\rm e}$, it's an ordmag less. That made it alot easier to believe it went around the Earth instead of vice-versa. See "Figleaf Salad" (*DIO 1.1* ‡7 §C3). (Give Ptolemy credit for going with a far better figure.) Still, two important positives ameliorate one's disappointment at such (hypothetical) prejudice: [a] The evident preference (§I2) by Hipparchos (at his career's end) for lunar longitude observations made when (as stated by Ptolemy) the longitudinal parallax was null (2 out of the 3 Hipparchos observations at *Almajest* 5.3&5) suggests he died still honestly unsure[285] of what the Moon's distance was. (This entails far greater uncertainty of the solar distance, unless he thought of his 7′ parallax figure as not a limit but

[283] The best compact expression for π is 355/113. Adding components of the familiar fraction 22/7 to this produces eq. 49, which is the most accurate brief sexagesimal expression for π. I see that the first 4 entries in Ptolemy's trig table (*Almajest* 1.11) are precisely based upon π = 377/120. (But the first 3 entries are simply correct, so this is not a unique explanation.) Thus, the appearance of the factor 13 in Hipparchos' lunar diameter expression (eq. 37) appears to be a precious thread, leading us to the realization that accurate π and accurate trig tables existed already in the 2nd century BC. (However, note that if 22/7 is multiplied by 120, the rounded result is 377. Did ancient sexagesimal rounding accidentally produce the best ancient π?)

[284] We have here noted, perhaps for the first time in explicit terms, something ignored by all textbooks: Aristarchos' Experiment (the half-Moon argument) is pro-heliocentrist. It placed a damper on how near (and thus small) geocentrist prejudice could make the Sun — without requiring ludicrous visible effects. It is remarkable that Ptolemy's crank mechanism (by bringing the Moon hugely closer to Earth near half-Moons) considerably eases the above-noted problem with visibly-silly γ. Which suggests the speculation that geocentrism could have been part of the pre-Ptolemy impetus that produced Ptolemy's well-named crank theory of the Moon. If so, the gain was hardly worth the trouble: while (unknown to Ptolemy) implicitly deflating the solar volume, this theory simultaneously required the Moon's angular diameter to vary by nearly a factor of two! — which anyone who looked at the real outdoor sky (a class obviously not including Ptolemy) knew did not happen. In this connection, with respect to the Ptolemy Controversy: for a quick & simple measure of which side knows what it's talking about, see the hilariously innocent remarks of Muffia-godhead Neugebauer 1957 p.196 (even while, only 10 pp later, he savages Duhem for promoting "flagrant nonsense" about Ptolemy's lunar theory!) vs. the truth, at R.Newton 1977 pp.182f — which shows that [a] Ptolemy not only believed his theory's absurd quadrature-distance (a fact haughtily denied by Neugebauer *loc cit*), but [b] Ptolemy even faked a *grossly* (fn 288) erroneous "observation" in perfect agreement with it. . . .

[285] If Hipparchos' only extant quadrature observation (*Almajest* 5.3) was for checking Aristarchos' Experiment, he got $\gamma = 3^\circ 3/4$ (not 8°). Against this suggestion is Hipparchos' arrangement for null longitudinal parallax: that is irrelevant for measuring γ, while it is useful for checking the lunar longitudinal position, which is what *Almajest* 5.3 suggests was its purpose.

a measurement.) [b] I am glad to say that, by Muffia criteria (§I1), Hipparchos the scientist was such a poor specimen that he did not force records to support his viewpoint, as did his later superiors Ptolemy and (§I1) Toomer.

S Hipparchos in Scientific History

S1 This seems the best place to offer my own (still-evolving) view of Hipparchos, for what it may be worth. As an astrologer in youth, he made a bundle off the gullible "fatheads" of Bithynia — to quote an ancient writer's delicate appraisal of the rich Bithynian victims of an infamous 2nd century AD occultist (other than Ptolemy). (See Lucian's delightful & instructive "Alexander the Oracle-Monger", which tells us that "fathead" was a professional term, used by magicians to denote easy marks.) On the illgotten proceeds, he then transplanted (or retired) to Rhodos, perhaps during or just before 147 BC (§K3) — not only for the good life there but also in order to carry out (in Rhodos' famous climate) his long-nurtured dream of improving man's astronomical knowledge. Toward this goal, he bought instruments (including a transit-circle & an armillary astrolabe) and hired skilled mathematicians, computers, and observers. (No wonder Tycho hung Hipparchos' portrait on his wall: see *DIO 2* ‡4 §A2.) Possibly he found royal sponsorship at Rhodos.[286] As we saw in eqs. 23 and 24, some of his computational work shows debts to the dominant competent astronomers of his time, who were heliocentrists, of course. These debts (probably to Aristarchos, ultimately) include: synodic & anomalistic monthlengths (eqs. 6 & 7), epoch-values for lunar synodic longitude (eq. 8) & anomaly (eq. 9). As van der Waerden has already pointed out,[287] Hipparchos was himself not as able a mathematician[288] as Ptolemy (or the latter's sources). (Lacking van der Waerden's sense of math, the Muffia has attempted to cast Hipparchos as the inventor of not only the eclipse triad method but the method of spherical projection: §N8 & Neugebauer 1975 p.869.) Indeed, Hipparchos' math betrays deficits in several important departments: [1] talent, [2] ethics, [3] simple coherence. (See fn 235 & fn 253.) Nor was Hipparchos the most skilled of observers: each of his 3 solar orbits' (EH, PH, & UH) equation-of-center was inferior[289] to that of Kallippos, 4th century BC (*DIO 1.1* ‡5 fn 13) — and the cause was empirical, not theoretical. (Hipparchos initially adopted Kallippos' yearlength, as discovered here at §K8 & §K9: the EH orbit. Later, Hipparchos shifted to his own famous PH yearlength, virtually equal to Aristarchos': see §N12, §G10, and Rawlins 1991H fn 1.) And this move may have been directly[290] due to a factor which suggests in itself that Hipparchos was not among the best of the ancient astronomers: he

[286] [Note added 1993: *DIO 4.1* will locate Hipparchos' main observatory at Lindos and will show that his star catalog's southern portion was observed on a transit instrument at Cape Prassonesi.]

[287] E.g., letter to R.Newton, 1986/9/16 pp.3-4 (copy to DR): "His whole style . . . not the style of mathematicians . . . . In the works of Pappos and Proklos many otherwise unknown mathematicians are mentioned, but Hipparchos is not among them." See fn 235.

[288] Hipparchos' most notorious error was the huge discrepancy (over 1°!) — cited at *Almajest* 3.1 — in his 2 lunar eclipse-based determinations of the Autumn Equinox's angular distance from Spica. DR has discovered that this mistake was simply caused by Hipparchos' use of the wrong sign for his 2 lunar parallax corrections of his 2 (accurate) observations. And, more recently, the very same parallax-sign blunder was made by a prominently-published *JHA* investigator, J.Evans, when reducing his own 1981/7/16 evening Seattle cross-staff lunar eclipse observation. The end-result was off by 2°/3, almost triple the lunar semidiameter! The 1981 achievement's ineptitude was so gloriously Ptolemaic (see R.Newton 1977 pp.182-191 on *Almajest* 5.12-13: error also 2°/3) that Evans believed his error to be observational rather than calculational — displaying just the sort of overconfidence which Ptolemy (*Almajest* 3.1) & Graßhoff 1990 (p.206) correctly criticize Hipparchos for. Thus, the result, when published by Hist.sci archons, was adorned with classic *JHA*-style moralizing about certain none-too-able scholars (read: RN&DR) who naïvely overestimate ancient astronomers' ocular & instrumental accuracy: p.275 n.50 of Evans 1987. This article's brilliance & incessant (if frustratingly insubstantial) cavilling at DR were so attractive to Univ Cambridge's Lord Hoskin & Harvard-Smithsonian's O.Gingerich that it was accorded the unprecedented honor of running, in 2 huge pieces, as the Pb paper of 2 successive issues of the extremely handsome *Journal for the History of Astronomy* (1987/8 & 1987/11).

[289] For eclipse-time prediction, Hipparchos' UH orbit was (probably by accident) superior to Kallippos' more accurate solar orbit. See Rawlins 1991H §E1.

[290] See §R14 & fn 280.

was a geocentrist (& astrologer). It is DR's contention (a point so self-evident that it would hardly even be controversial, except in a Hist.sci community that has become obsessed with "whiggism" & "paradigms") that: there is a correlation between [i] the sort of intelligence required to discern that heliocentrism is preferable to geocentrism, and [ii] that which is required for contributing new insights to a science. (Likewise, I suggest that, in modern academe, one might detect a correlation between [a] competent, talented scholarship and [b] insistence on honesty, incorruptibility, & rejection of unethical compromise with a gang — or ring[291] — of Nibelungs.)

S2 Hipparchos' historical rôles as propagandist, preserver, & calendarist will of course always be remembered (with varying degrees of gratitude). But it is as a dedicated observer that he will shine forever bright in the history of astronomy. The quality of Hipparchos' massive (astrolabe-based) Ancient Star Catalog (1025 objects) — which Pliny 2.95 rightly calls a bold inheritance left to all mankind — was not improved upon until 17 centuries later (Tycho 1598: see *DIO 2.1* ‡4). Hipparchos' most admirable trait, which Muffiosi regard as showing his mental inferiority to Ptolemy (see [Muffia 1990] pp.204f), will ensure his rank among history's premier early scientists: he refused to fake observations to accord with theory: §N15 & §R14.

S3 We are by now well beyond the firm inductions that culminated in §N-§O, and are swimming in fun but obviously speculative areas. So, rather regretfully, I leave off analysis at this point. But, it's been grand. And my thanks[292] again to the Muffia for handing me the *Almajest* 4.11 problem, which I'd foolishly skimmed past for so long.

References

Almajest. Compiled Ptolemy c.160 AD. Eds: Manitius 1912-3; Toomer 1984.
Archimedes. *Works* c.260 BC. Ed: T.Heath, Cambridge U. 1897&1912.
J.L.Berggren & B.Goldstein 1987, Eds. *From Ancient Omens to Stat Mech*, Copenhagen.
A.Bowen & Goldstein 1988. At Leichty, Ellis, Gerardi 1988 p.39.
John Britton 1967. *On the Quality of Solar & Lunar Param in Ptol's Alm*, diss, Yale U.
DSB = *Dictionary of Scientific Biography*, Ed: C.Gillispie, NYC.
J.Evans 1987. JHA *18*:155 & 233.
Geographical Directory. Compiled Ptolemy c.160 AD. Ed: C.Nobbe, Leipzig 1843-5.
O.Gingerich 1972. Centaurus *17.2*:173.
O.Gingerich 1976. Science *193*:476.
O.Gingerich 1980. QJRAS *21*:253.
O.Gingerich 1981. QJRAS *22*:40.
O.Gingerich 1988. JHA *19*:142. Review of Berggren & Goldstein 1987.
O.Gingerich 1990. JHA *21*:364. Review of R.Newton 1982.
B.Goldstein 1978. Science *199*:872. Review of R.Newton 1977.
B.Goldstein & A.Bowen 1983. Isis *73*:330.
B.Goldstein & A.Bowen 1989. Centaurus *32*:272.
Gerd Graßhoff 1990. *History of Ptolemy's Star Catalogue*, NYC. [Muffia 1990]
Thos.Heath 1913. *Aristarchus of Samos*, Oxford U.
J.Heiberg 1907, Ed. *Claudii Ptolemaei Opera Astronomica Minora*, Leipzig.
Alexander Jones 1983. ArchiveHistExactSci *29.1*:1.
Alexander Jones 1990. Trans Amer Philos Soc *80.7*:1.
Alexander Jones 1991H. JHA *22.2*:101.
Alexander Jones 1991M. Isis *82.3*:441.

[291] The title of Wagner's immortal cycle obviously has at least 2 meanings. Have we here stumbled upon a 3rd?

[292] My gratitude is but the merest trifle, beside that which Hist.sci archons will (very privately) feel towards Lord Hoskin for directly causing DR to become the discoverer of the solutions to the Hipparchos math (§N & §O) of *Almajest* 4.11 and (fn 288) *Almajest* 3.1. I will only comment that: if one assents to the use of missmen as hitmen, the responsibility for disaster is one's own, and no one else's.

E.Leichty, M.Ellis, P.Gerardi 1988, Eds. *Scientific Humanist: Stud Mem A.Sachs*, Philad.
Y.Maeyama 1984. Centaurus *27*:280.
Karl Manitius 1912-3, Ed. *Handbuch der Astronomie* [*Almajest*], Leipzig.
K.Moesgaard 1983. JHA *14*:47.
K.Moesgaard. At Berggren & Goldstein 1987 p.43.
O.Neugebauer 1957. *Exact Sciences in Antiquity*, 2^{nd} ed, Brown U.
O.Neugebauer 1975. *History of Ancient Mathematical Astronomy* (*HAMA*), NYC.
R.Newton 1970. *Ancient Astronomical Observations*, Johns Hopkins U.
R.Newton 1973-4. QJRAS *14*:367, *15*:7, 107.
R.Newton 1976. *Ancient Planetary Obs . . . Validity . . . EphemTime*, Johns Hopkins U.
R.Newton 1977. *Crime of Claudius Ptolemy*, Johns Hopkins U.
R.Newton 1979F. QJRAS *20*:383.
R.Newton 1982. *Origins of Ptolemy's Astronomical Parameters*, U.Maryland.
R.Newton 1985. *Origins of Ptolemy's Astronomical Tables*, U.Maryland.
O.Pedersen 1974. *Survey of the Almajest*, Odense U.
D.Rawlins & Max Hammerton. MonNotRAS *162*:261.
D.Rawlins 1982C. Publications of the Astronomical Society of the Pacific *94*:359.
D.Rawlins 1982G. Isis *73*:259.
D.Rawlins 1984A. Queen's Quarterly *91*:969.
D.Rawlins 1985G. Vistas in Astronomy *28*:255.
D.Rawlins 1985H. BullAmerAstronSoc *17*:583.
D.Rawlins 1985K. BullAmerAstronSoc *17*:852.
D.Rawlins 1985S. BullAmerAstronSoc *17*:901.
D.Rawlins 1987. American Journal of Physics *55*:235. [Note *DIO 11.2* §G & fnn 26-27.]
D.Rawlins 1991H. DIO *1.1* ‡6.
D.Rawlins 1999. DIO *9.1* ‡3. (Accepted JHA 1981, but suppressed by livid M.Hoskin.)
A.Rome 1931-43, Ed. *Comm Pappus & Theon d'Alex*, Studi e Testi *54*, *72*, *106*.
Geo Sarton 1959. *History of Science [2]*, Harvard.
Noel Swerdlow 1968. *Ptol's Theory of the Distances&Sizes of the Planets*, diss, Yale U.
Noel Swerdlow 1969. Centaurus *14*:287.
Noel Swerdlow 1973. Isis *64*:239. Review of van der Waerden 1970.
Noel Swerdlow 1979. American Scholar (ΦBK) *48*:523. Review of R.Newton 1977.
N.Hamilton-Swerdlow 1981. JHA *12*:59. Review of R.Newton 1976.
Noel Swerdlow 1989. JHA *20*:29.
Noel Swerdlow 1993. JHA *24*:289. O.Neugebauer obit.
Victor Thoren 1990. *Lord of Uraniborg*, Cambridge U.
Gerald Toomer 1967. Centaurus *12*:145.
Gerald Toomer 1973. Centaurus *18.1*:6.
Gerald Toomer 1974D. ArchiveHistExactSci *14*:126.
Gerald Toomer 1975. Ptolemy entry, DSB *11*:186.
Gerald Toomer 1976. Theon of Alexandria entry, DSB *13*:321.
Gerald Toomer 1977. ArchivesIntHistSci *27*:137. Review of Pedersen 1974.
Gerald Toomer 1978H. Hipparchos entry, DSB *15*:207.
Gerald Toomer 1980. Centaurus *24*:97.
Gerald Toomer 1984, Ed. *Ptolemy's Almagest*, NYC.
Gerald Toomer 1988. At Leichty, Ellis, Gerardi 1988 p.353.
A.Van Helden 1985. *Measuring the Universe*, U. Chicago.
B.van der Waerden 1963. *Science Awakening I* (Tr. Arnold Dresden), NYC.
B.van der Waerden 1970. *heliozentrische System . . . griech, pers & ind Astron*, Zürich.
B.van der Waerden 1974. *Science Awakening II* (contrib. Peter Huber), NYC.
B.van der Waerden 1978. DSB *15*:667.
B.van der Waerden 1984-5. ArchiveHistExactSci *29*:101, *32*:95, *34*:231.
J.Włodarczyk 1987. JHA *18*:173.