‡6 Hipparchos' Ultimate Solar Orbit

& the Babylonian Tropical Year

Summary

The sole extant Babylonian tropical yearlength value is found to be based upon Hellenistic observations, one of which — a 135 BC Summer Solstice — was performed and used by Hipparchos (a few years before his death) to improve his wellknown erroneous 146 BC solar theory (PH orbit). His new Ultimate Hipparchos (UH) orbit (epoch 128 BC) was that from which he set the astrolabe for his last 3 surviving dated observations & for the nearly contemporaneous Ancient Star Catalog's zodiacal longitudes. The elements of this UH orbit are completely reconstructed and are found to be almost twice as accurate as the famous standard (PH) Hipparchan solar tables preserved in the *Almajest*.

A The Initial Cuneiform Clue

A1 The only surviving explicit[1] Babylonian estimate of the tropical year's length is found on the well known Astronomical Cuneiform Text (ACT) #210 [BM55555] Sect.3 (Neugebauer 1955 1:271-3, 3:243a; Neugebauer 1975 p.528). ACT #210's yearlength is:

$$Y_{\mathrm{B}} = 365^{\mathrm{d}}14'44''51''' \tag{1}$$

(precision: 2/5 of a timesec), a much discussed but previously unexplained datum. (See ‡5 fn 8.) If we run a continued fraction analysis on this value (& truncate before the

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[1] Rawlins 1999 reconstructs a Babylonian tropical (civil) year of $365^{\mathrm{d}}1/4 - 1/285$, evidently arrived at by ancients' division of 19 into $235M_{\mathrm{A}}$ = $6939^{\mathrm{d}}41'$. (M_{A} is from eq. 10; note that 285 is an integral multiple of 19.) At least as early as Meton (432 BC), $235M/19$ was a politically useful civil year (bringing lunar & solar priests together under a single calendar, a scheme still used to compute Easter's date). But equating this amount to an empirical *tropical* year was a fateful blunder, apparently originated (from early, shaky evidential indications: Rawlins 1985H) by Meton, Kallippos, & Aristarchos, later adopted by Hipparchos & Ptolemy. However, the fact that Aristarchos was the earliest (Rawlins 1999) to use a year near eq. 7 also imparts the vital information that *he was the first known astronomer to possess a highly accurate value of the month*, a value we may virtually recover just by multiplying 19/235 times his tropical year (giving $29^{\mathrm{d}}.530602$; see Rawlins 1985H and Rawlins 1999's decipherment of the mss data listed at Neugebauer 1975 p.601). Aristarchos (280 BC) was specifically the originator of the remarkably correct "Babylonian" month M_{A} (see §B10). On the other hand, $19Y_{\mathrm{K}}/235$ = $27759^{\mathrm{d}}/940$ = $29^{\mathrm{d}}.530851$; thus, in 330 BC, Kallippos' month was (Dinsmoor 1931 p.409) 22^{s} longer than the real month (then equal to $29^{\mathrm{d}}.530597$, according to the Earth-acceleration of §B2). In 432 BC, Meton's month was $19{\cdot}(365^{\mathrm{d}}5/19)/235$ = $6940^{\mathrm{d}}/235$ = $29^{\mathrm{d}}.531915-$, which is 114^{s} longer than reality. By contrast, Aristarchos' M_{A} (eq. 10) is correct to a fraction of 1^{s}. Since determining M required possession of [a] reliable ancient lunar eclipse records & [b] an accurate theory of the syzygial Moon's non-mean motion [however, see *DIO 6* ‡1 fn 18], remarkable improvement in both categories seems to have occurred during the 1 1/2 century interval: 432 BC to 280 BC. Regarding [a]: Kallippos was among the very first Greeks with access to the ancient lunar records of Babylon (van der Waerden 1974 p.290; note the Theon of Alexandria testimony there cited in n.3, from Rome 1931-43 p.839-840, and its conflict with Rawlins 1985H); Kallippos obviously used (and so helped immortalize) the −330/9/20 Arbela eclipse (his most recent) as a prime contemporary empirical anchor for his lunisolar theory & calendar (Rawlins 1985H), whose epoch was the latter of a millennially unique pair of close approaches of Summer Solstice & New Moon, −348/6/27 and −329/6/28. Simply by comparing monthlength accuracy (22^{s} vs. 1^{s}), we can date [b] to the 1/2 century between Kallippos & Aristarchos. This allows us to pinpoint (at least within a few decades) just when the amazing flowering of the full genius of Hellenistic empirical astronomy occurred. A measure of that genius: Aristarchos' sidereal motions of Sun (Rawlins 1985S, Rawlins 1999) & Moon (*idem* plus eq. 10 & §B10) were both accurate to about 2 parts in ten million; Rawlins in-prep A.

remainder-denominator becomes outsize) we get the close approximation:

$$Y_{\mathrm{B}} = 365^{\mathrm{d}} + \cfrac{1}{4 + \cfrac{1}{15 - \cfrac{1}{2 + \cfrac{1}{2}}}} = 365^{\mathrm{d}}73/297 \tag{2}$$

A2 We may also express this result with respect to the familiar Kallippic (Julian) year, which is equal to

$$Y_{\mathrm{K}} = 365^{\mathrm{d}}1/4 \tag{3}$$

Combining eqs. 2 and 3:

$$Y_{\mathrm{B}} = Y_{\mathrm{K}} - 5^{\mathrm{d}}/1188 = Y_{\mathrm{K}} - (5^{\mathrm{d}}/4)/297 \tag{4}$$

(Eqs. 2 or 4 will easily produce the attested Y_{B} of ACT #210 to full sexagesimal precision, since eq. 4 differs from eq. 1 by less than $0^{\mathrm{s}}.04$.) An alternate way of rendering eq. 4: 297 Babylonian tropical years are cumulatively $5^{\mathrm{d}}/4$ shorter than 297 Kallippic years:

$$297 \cdot Y_{\mathrm{B}} = 297 \cdot Y_{\mathrm{K}} - 5^{\mathrm{d}}/4 \tag{5}$$

Empirical ancient solstices & equinoxes were customarily rounded to the nearest quarter day. Such data could be the basis of Y_{B}.

A3 Ancient astronomers Meton, Kallippos, Aristarchos, Archimedes, & Hipparchos evidently used Summer Solstice (SS) observations for determining the tropical year's length because equinoxes are subject to vexatious systematic errors[2] (VE & AE: same magnitude, but opposite sign; Britton 1967 p.29) from misplacement of instrumental equator. (See below, §C1.) The hypothetical solstices producing Y_{B} would have been recorded 297^{y} apart, with the 2nd datum occurring (as shown by eqs. 4 & 5) $5^{\mathrm{d}}/4$ ahead of the time predicted by just adding $297{\cdot}Y_{\mathrm{K}}$ onto the 1st datum.

A4 Understand: besides 297^{y}, no other span of time (relatable to a not too long interval between observations)[3] can yield eq. 1 via standard ancient $1^{\mathrm{d}}/4$ precision solstice data.

A5 So, now one goes fishing: are there extant ancient solstice observations that are 297^{y} apart? Well, since there are only 3 real examples of such data whose observers and years are directly attested, the *a priori* odds certainly are not encouraging. These three records are mentioned in *Almajest* 3.1: the solstices of Meton (−431/6/27 1/4 = dawn or 6 AM), Aristarchos of Samos (−279/6/26), and Hipparchos (−134/6/26). (Ptolemy does not provide either Aristarchos' or Hipparchos' solstice hour — nor even day, though the dates are fortunately not in dispute. I thank the late Willy Hartner for bringing Ptolemy's silence to my attention in a letter of 1980/8/15.)

A6 We know that something quite remarkable has been revealed when we find that: *the Meton and Hipparchos observations are in fact* 297^{y} *apart*. The likelihood of this being a chance agreement with the 297^{y} interval of eq. 5 is ordmag 1%. (It was on 1982/1/28, while typing a letter to R.Newton, that I hit upon eqs. 4 & 5 and the astonishing connection between ACT #210 and the Meton & Hipparchos data. The discovery was reported briefly in, e.g., Rawlins 1984A p.989 n.43, and Rawlins 1985G p.256 n.3.)

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[2] Unlike these astronomers, Ptolemy was utterly unfamiliar with actual outdoor observing (see, e.g., fn 24) and so preferred equinoxes (*Almajest* 3.1). See also ‡5 fn 20.

[3] Only sub-500^{y} alternatives (to eq. 4 remainder) are: $(7^{\mathrm{d}}/4)/416^{\mathrm{y}}, 2^{\mathrm{d}}/475^{\mathrm{y}}$. (Each yields an adequate approximation to eq. 1, though not so close as eq. 4.) But either requires availability before c.68 BC (see §B9) of empirical solstices over 4 centuries old, i.e., from c.500 BC. (As for Babylonian solstices, see Neugebauer 1975 p.363.)

A7 The tracing of a Babylonian cuneiform parameter back to wholly Hellenistic sources is a watershed, marking the commencement of our awareness of how heavily Seleukid-era Babylonian astronomers (more likely astrologers) depended upon the science of the superior civilization that had under Alexander conquered Babylon. (Subsequently discovered details of extensive Babylonian use of Greek lunar and planetary orbital work will appear in Rawlins in-prep A.)

B Hipparchos' Accurate Solstice & the Date of ACT #210

B1 Scholars have long conjectured regarding the hour of the Hipparchos –134/6/26 solstice, commonly presuming[4] it to be noon because that is consistent with the Hipparchos-Ptolemy (PH) *Almajest* solar tables (see §B3). Now at last the hour may be firmly reconstructed just by adding $297{\cdot}Y_{\mathrm{B}}$ to the Meton time (−431/6/27 6 AM; *Almajest* 3.1):

$$-431/6/27\ 1/4 + 297 \cdot Y_{\mathrm{B}} = -134/6/26\ 1/4 = 6\ \mathrm{AM} \tag{6}$$

(Rawlins 1985H. This equation merely rearranges the original process whereby Y_{B} was found by its ancient inventor: dividing 297 into the time-interval between these 2 solstice-data.)

B2 The actual –134/6/26 solstice was about 7 AM Rhodos local mean time (if one adopts Earth mean fractional secular spin acceleration $-19\mathrm{x}10^{-9}$/century;[5] Tuckerman 1962&64 makes it 6 AM); therefore, the observation was accurate within rounding error ($\pm 3^{\mathrm{h}}$), as such data will usually be (fn 13; Rawlins 1985H).

B3 Hipparchos' observed Summer Solstice (SS) hour 6 AM (eq. 6) does not agree with the *Almajest* 3.2&6 Hipparchos (PH) solar tables (which give 11 AM); this presumably explains why Ptolemy in *Almajest* 3.1 neither states the hour nor compares his own tabular 140 AD solstice "observation" to this discrepant Hipparchan datum, in order formally to establish the tables' yearlength, which was his procedure earlier (twice in the very same chapter: *Almajest* 3.1) regarding Hipparchos' equinoxes. He instead compares his 140 AD datum to Meton's agreeable old solstice. This inconsistency is especially odd because

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[4] E.g., Britton 1967 pp.23, 56; R.Newton 1977 p.83 n.3; van der Waerden 1986. However, in a generous 1986/9/20 letter to DR, van der Waerden, whose desire to adjust his opinions to new evidence is legendary, has withdrawn his paper's conjectured Aristarchos & Hipparchos solstice-hours. (BvdW's letter also proposed to send a retracting note to *Isis* on the basis of the UH orbit. This noble offer I regretfully declined, having experienced a succession of weird encounters with *Isis*. I instead made plans to publish the UH orbit discovery in *DIO*. Of course, *Isis* is always free to republish *DIO*'s findings. We're not holding our breath.) In a 1988/12/20 letter to DR, van der Waerden objects to the foregoing word "legendary", protesting that no such legend exists. If he is right, I hope to change that situation. He also objects to the word "generous". Clearly, his logic is: one should follow the truth wherever evidence leads, and that is not a matter of personal generosity: van der Waerden will praise a detractor or criticize a friend without favor, a virtue which he has inspired in others and which I have pledged will long survive him in *DIO*. So, I accept & support the interpretive correction, but wish to add that I call it not merely proper but *additionally* generous when one acknowledges the rightness of a scholar who is correcting a published work of oneself. And, if there is any word that succinctly describes the Neugebauer clique's attitude toward R.Newton, Diller, Billard, and sometimes even van der Waerden, it is: ungenerous. Incidentally, the frequently entertaining math of the Neugebauer gang is sampled at Rawlins 1987 n.30. (In the *American Journal of Physics*: undeniably accurate but highly embarrassing material which pathetic *Isis* had previously refused to publish.) See also fns 9 & 35 there, and here at fn 6, fn 21, & fn 33; also ‡1 §C5 & ‡5 fn 7.

[5] I use this figure here throughout. It is accurate to better than 10%, and is based upon [a] modern lunar places & gravitational theory, [b] the tidally-induced lunar acceleration of Dickey & Williams 1982, and [c] taking the successful *Almajest* 4.4 lunar mean elongation tables as correct for anytime between epochs Phil 1 (324 BC) & Ant 1 (137 AD). The fit is so smooth that any chosen epoch in this semi-millennial range produces the same result. (If the pre-Ptolemy solar equinox data of *Almajest* 3.1 are trusted to $1'$, then $-19\mathrm{x}10^{-9}$/cy might be a few percent on the small side; but an alteration of even 10% would require the existence of an unsurvivably flagrant asymmetry in errors of ancient eclipse-time predictions from the tables, i.e., comparable to their 16^{m} rms scatter: §E1.)

Almajest 3.1 correctly describes Hipparchos' solstice as "accurate", while twice calling Meton's solstice "crude". (Thus, I doubt that the doubly greater antiquity of Meton's solstice would justify using it in preference to Hipparchos'.)

B4 Placing Ptolemy's reticence into context: in *Almajest* 3.1&7, he provides 28 solar data (24 equinoxes and 4 solstices, helpfully tabulated in full by Britton 1967 p.23). Of all these data, the *only* ones for which he omits the time of day are the above-mentioned solstices of Aristarchos (−279/6/26) & Hipparchos (–134/6/26), where instead he merely quotes Hipparchos' statement that the *interval between* these 2 solstices was $1^{\rm d}/2$ shorter than $145{\cdot}Y_{\rm K}$, in close accord with the standard Hipparchos-Ptolemy tropical year used throughout the *Almajest*:

$$Y_{\rm J} = 365^{\rm d}14'48'' = Y_{\rm K} - 1^{\rm d}/300 = 54787^{\rm d}/150 \tag{7}$$

B5 In retrospect, we really didn't need the foregoing ACT #210 discovery to tell us: if Ptolemy evaded giving these two solstices' times, it was because *they did not agree* with the Hipparchos (PH) solar tables his discussion was trying to establish (*Almajest* 3.1-7). Obviously, the Aristarchos and Hipparchos solstices were jointly offset by roughly $1^{\rm d}/4$ from the PH solar tables of *Almajest* 3.2&6; and Hipparchos most likely differed in the direction of accuracy, given the surety with which the solstice can be determined, within about $2^{\rm h}$ (Rawlins 1985H, contra R.Newton 1982 p.42) of the truth.[6]

B6 And that is exactly what we have found in §B1, since (to the nearest $1^{\rm d}/4$) Hipparchos' observed –134 SS time (deduced in eq. 6) was rightly earlier by $1^{\rm d}/4$ than the time given by the *Almajest* solar tables (PH).[7]

B7 Hipparchos' information (*Almajest* 3.1), that there was (between Aristarchos' solstice & his own) an interval of 145 of his yearlengths $Y_{\rm J}$, now additionally permits our reconstruction[8] of Aristarchos' solstice-time (using the result & the method of eq. 6, again ignoring the small geographical longitude difference between the observations, as does *Almajest* 3.1); rounding to the nearest $1^{\rm d}/4$:

$$-134/6/26\ 1/4 - 145 \cdot Y_{\rm J} = -279/6/26\ 1/2 = \text{noon} \tag{8}$$

(The elementary source of the illusory huge errors in this solstice & Meton's is revealed in Rawlins 1985H.)

B8 Though too long (vs. reality) by almost 5 timeminutes ($5^{\rm m}$), $\mathrm{Y_B}$ (eqs. 1 & 4) is nonetheless the best of a rather poor lot of surviving ancient estimates of the tropical year's length (Rawlins 1999). It was likely a Hipparchos value in some sense, though whether it was his own attempted late improvement (subsequently neglected by Ptolemy and Censorinus; Neugebauer 1975 p.624) upon the traditional and tabular value (eq. 7: $365^{\rm d}1/4\ -1^{\rm d}/300$) or was due to a later disciple, one cannot now be sure. I prefer the latter theory, partly because eq. 8 shows that a late Hipparchos work (after –134) justified his yearlength value by comparing his own –134 solstice not to Meton's (which would have given $Y_{\rm B}$, eq. 4) but rather to Aristarchos' (yielding $Y_{\rm J}$, eq. 7); and an even later self-summary (cited *Almajest* 3.1) of all his works still stands by $Y_{\rm J}$.

B9 A nice byproduct of the foregoing findings is a major temporal restriction upon the hitherto undated ACT #210 (Systems A & B): it was written after 135 BC. And since no System B lunar text is dated to later than 68 BC (Neugebauer 1955 pp.xvi & 182), we have the probable range:

$$\text{date of ACT \#210} = 100\ \text{BC} \pm 35^{\rm y} \tag{9}$$

This tablet is one of the very few explicitly exhibiting the famous and highly accurate "Babylonian" monthlength (System B):

$$M_{\rm A} = 29^{\rm d}31'50''08'''20'''' = 29^{\rm d}.530594 \tag{10}$$

which Ptolemy attributed to Hipparchos (*Almajest* 4.2).

B10 It has long been assumed (starting with the epochal work of F.Kugler S.J., who first elicited $M_{\rm A}$ from cuneiform material: Kugler 1900 pp.24, 53, & 111) that Ptolemy was wrong and that Hipparchos instead just appropriated $M_{\rm A}$ from Babylon. . . . ACT #210 is now revealed here as post-Hipparchos [DR 2008: I thank A.Jones for a correction here]; I have already published evidence that $M_{\rm A}$ originated with neither him nor Babylon but instead is due to Aristarchos. (Rawlins 1984A p.987 n.25, Rawlins 1985G p.267 n.3, Rawlins 1985S & Neugebauer 1975 p.603; full details to appear in [*DIO 11.1* ‡1].)

B11 My impression has been that, from Kugler through Neugebauer, orthodox scholars have at least tacitly been assuming (e.g., Aaboe 1955; Britton 1967 p.iii; Neugebauer 1975 pp.4, 309, 351-5, 622) that parameters common to Babylonia & Greece show that Babylonian theoretical astronomy was a source for Greek, not vice-versa[9] — even though

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[6] Simply accomplished by the exceedingly elementary method of equal altitudes, which appears to be known to everyone in the universe except Ptolemy (fn 2) & U.Chicago's Noel Swerdlow. For the latter's epochally entertaining preschool anti-solstice argument, see p.527 of Swerdlow 1979 (lowlights: ‡5 fn 20), a review whose demeanor toward Ptolemy-skeptics is apt to the same educational level. This precious gem was published in the journal of the Phi Beta Kappa honor society, *American Scholar*. Of course, it goes without saying that Swerdlow questions the integrity of the author under review (as also in Swerdlow 1973): on p.528, he charges R.Newton 1977 with hiding his use of the French (Halma) 1813-6 translation of the *Almajest*, though Halma's edition is in fact cited at p.146 of R.Newton 1977, as well as at p.121 of R.Newton 1973-4 (the very paper where the proposal Swerdlow 1979 is assaulting was 1st published, at p.112). Similarly, Swerdlow 1973 p.243 (in *Isis*) accuses van der Waerden 1970 of noncitation of works disagreeing with him, a charge contradicted 2 paragraphs previously, and in any event a neat trick for a work with a 42-item bibliography, since, at the time, no one agreed with (or had even thought of) van der Waerden's central new proposal, which has since been proven correct (fn 36). Note: [a] van der Waerden 1970 cites 4 works from the Neugebauer clique that loathes the theory under discussion; [b] not a single inner member of this clique has ever cited any work by DR. (Watch Neugebauer's clonies handle the lovely UH discovery by: [a] ignoring it, [b] attacking it, or [c] trying to grab prime credit for it.) Swerdlow 1979 appears in the journal of ΦBK, whose editorial board included Ptolemy's most public defender, power-operator O Gingerich (on whose scholarly ability Swerdlow has somehow never gotten around to publishing his strong private opinion). Throughout, Swerdlow 1979 falsely treats R.Newton as if he does not have a PhD, by deliberate & consistent reference to "Mr.Newton". (Details at ‡3 fn 3. Question: why bother being accurate, in a field where one can ascend anyway by catering to power and taking care to attack only the pet hates of the influential?) Since Swerdlow's behavior suffers no public criticism by Hist.sci's other archons (to the contrary: ‡1 fn 15), one assumes that his output is regarded by them as exemplifying the scholarship & credit this field's leaders generate when they are placed at the best-known universities.

[7] More accurately: $5^{\rm h}$ earlier; from §B1 & §B3: observed-minus-PH $= 6$ AM $-$ 11 AM $= -5^{\rm h}$. Hipparchos' PH tables agree with his observation (virtually exactly) for the –145/9/27 1/4 Autumn Equinox. This is also the 1st year for which Hipparchos leaves us 2 cardinal-point solar data. (And he adds another –145 VE observation from Alexandria; all 3 data are in *Almajest* 3.1. There was probably also a –145 SS-time: §C1.) Thus, it is reasonable to suppose that Hipparchos' contemporary epoch for his PH tables was –145. If so, this exact epoch was (just $54^{\rm h}$ after his AE observation) at: Pot 1 = Physkon 1 Thoth 1 = –145/9/29 noon. (The astronomical 1st regnal year of Ptolemy VII Physkon; φυσκων is Greek for pot-belly.) Proposed in Rawlins 1985K (though not necessary to that abstract's rounded-ϵ theory). (Note the oddity that the AE occurs at Thoth 1 noon in –136 for PH orbit, –135 for UH orbit & reality. Hipparchos' formal PH lunisolar epoch: Philip 1 Thoth 1 = −323/11/12 noon, likely borrowed from Kallippos and-or Aristarchos; Rawlins 1985K.) Since the PH (& UH) tables are based on yearlength $Y_{\rm J} = Y_{\rm K} - 1^{\rm d}/300$ (eq. 7), these tables must depart from Kallippically spaced $1^{\rm d}/4$ precision data by $1^{\rm d}/300 = 4^{\rm m}48^{\rm s}$ per year after –145 (when the PH error for SS was over $+2^{\rm h}$). By –134, this departure had accumulated to more than $1^{\rm h}$; by –127, to $2^{\rm h}$, bringing the PH error in SS to over $+4^{\rm h}$, a discrepancy which was later revised by the new UH theory (see fn 12).

[8] Rawlins 1985H. (I here withdraw that paper's explicitly speculative Hipparchos $30400^{\rm y}$ precession cycle.) Note probable use of a nearby eclipse-anchor (−279/6/30, Rawlins 1985S; as also in the case of Kallippos: fn 1). This and Aristarchos' −279 solstice observation (only a few days earlier) were presumably the empirical foundation-stones of the astronomical calendar named for Dionysios the Renegade (the philosopher whose name is one of the inspirations for the title of this journal: ‡1 fn 23).

[9] Neugebauer once flirted with the idea that Meton's cycle was original (Neugebauer 1957 p.140; Samuel 1972 p.21) but later rejected this (Neugebauer 1975 p.622).

(until now) the evidence adduced actually favored neither alternative.[10] (However, see the two ancient tables of astronomers' yearlengths at Neugebauer 1975 p.601: both's hitherto-unremarked chronologies support Greek priority.)

B12 Our previous uncertainty regarding who got common (high-level astronomy) parameters from whom is eliminated by ACT #210, since it is a Babylonian text providing a parameter which is dependent upon and thus subsequent to a specific, dated twosome of famous, purely Hellenistic instrumental *observations* (Meton & Hipparchos). This is vastly more informative than a sharing of common parameters of unknown empirical origin, which might have been transmitted in either direction or be from an earlier mutual source.

B13 The Kugler-Neugebauer Babylonia-to-Greece presumption may ultimately have been due to little more than the very natural and human hopes of those making discoveries (among Babylonian cuneiform material) that their ingenious, hard-wrought finds represent original not merely secondary science. Another possible unconscious contributing factor: the greater antiquity of Babylonian civilization; but late Babylon had no sophistication in observational instruments or astronomical mathematics — which presumably explains why virtually all (if not precisely all) worthwhile orbital data on cuneiform texts date from after the Greek conquest of Babylon. (See Neugebauer 1955 1:xvi, 2:xii.)

C Hipparchos' Improved Solar Observations & Ultimate Orbit

C1 It is well known that at his career's peak, Hipparchos' instrumental equator (IE) was a few arcminutes low (see fn 13, and Rawlins 1982C p.370 & sources there cited), causing his Vernal Equinox observations to be early, his Autumn Equinoxes late. He also evidently observed a Summer Solstice (record not directly extant) in –145. (A solstice time measurement is unaffected by IE error; §A3. For an elucidating discussion of the distinction, consult R.Newton 1977 pp.81-82, 90.) Shortly thereafter, using this solstice and the 2 recorded equinoxes (*Almajest* 3.1) of the same year (3 empirical data), he founded his solar tables by the method explained in *Almajest* 3.4-7. The solar orbit thus established I am calling: the PH (Prime Hipparchos) orbit. The PH theory was identical[11] to the orbit preserved in the tables of *Almajest* 3.2&6, and treated by Ptolemy as the only Hipparchos solar orbit — mistakenly, as we are about to see.

C2 Hipparchos' last extant Autumn Equinox observation (–142/9/26 3/4) crucially snapped his equinoxes' pattern of systematic error (a point emphasized in R.Newton 1970 p.15): it was correctly observed as having occurred a 1^{d}/4 notch earlier than indicated by the PH tables, themselves 7^{h} late at this moment. (The PH tables predicted Autumn Equinox at –142/9/27 0^{h} = midnight; for Earth-acceleration of §B2, the actual Autumn Equinox was at –142/9/26 17^{h} = 5 PM, within about an hour of the recorded Hipparchos observation.)

C3 Putting this notable –142/9/26 equinoctial improvement together with the fact that (as discovered above, §B3) Hipparchos' last known Summer Solstice (–134/6/26 1/4) was also rightly discordant by about 1^{d}/4 with respect to the PH tables: we have a double suggestion that an astronomer as energetic as Hipparchos might well have tried to use his fresh data (both now more correct than his corresponding earlier material) for improving his original PH solar orbit and thereby creating an Ultimate Hipparchos orbit, a momentarily-hypothetical entity which I will henceforth refer to as the UH solar orbit.

C4 Fourteen Hipparchos Vernal Equinoxes survive (*Almajest* 3.1): first, –145/3/24 1/4; last, –127/3/23 3/4. (Note: the bounds are in the years ending at the PH & UH epochs, which independently suggests that those two VE data were utilized in the empirical foundations of the respective theories.) Unlike his Autumn Equinoxes: all are spaced Kallippically, i.e., at exact integral multiples of Y_{K}. Due to the 11^{m} excess of Y_{K} over the actual 365^{d}.2423 interval between Vernal Equinoxes at that epoch, these Hipparchan Vernal Equinox observations got 11^{m} more accurate every year: 9^{h} early in –145, but only 6^{h} early in –127. Thus, since the –142 Autumn Equinox and –134 Summer Solstice were both correct to about 1^{h}, Hipparchos by –134 had in hand solar data averaging only 4^{h} off reality (rms) — vs. his prior (PH) orbit's foundation, where the errors were nearly 2 times larger.[12]

C5 The gist of the foregoing is that Hipparchos' last fundamental observations shifted (vs. the 365^{d}1/4 interval Kallippic-Julian calendar) his Summer Solstice time & Autumn Equinox time back by 1^{d}/4 each, while producing no such shift in the Vernal Equinox. Since Spring (V) lasts from the VE to the SS, Hipparchos had found his final value for Spring's length, V_{U}, to be shorter by 1^{d}/4 than his PH orbit's value, V_{P}; since Summer (S) lasts from the SS to the AE (both shifted identically), he found no change in Summer.

C6 The famous season lengths from which Hipparchos had elicited his PH orbit's eccentricity e_{P} and apogee A_{P} were (*Almajest* 3.4):

$$V_{\mathrm{P}} = 94^{\mathrm{d}}1/2 \qquad \& \qquad S_{\mathrm{P}} = 92^{\mathrm{d}}1/2 \tag{11}$$

(Actual season lengths then: V = 94^{d}, S = 92^{d}1/3.) But the above-discussed shifts tell us that the UH figures Hipparchos later settled upon were:

$$V_{\mathrm{U}} = 94^{\mathrm{d}}1/4 \qquad \& \qquad S_{\mathrm{U}} = 92^{\mathrm{d}}1/2 \tag{12}$$

C7 Using the simple procedure of *Almajest* 3.4 (well explained by Neugebauer 1975 pp.58, 308, & 1221 Fig.53), one may find (from these 2 season lengths) the eccentricity e_{U} and apogee A_{U} of the final Hipparchos solar orbit, just as he would have derived it. His process started with the conversion of the Spring and Summer arcs from days into degrees of mean longitude, using mean solar motion F; from eq. 7:

$$F_{\mathrm{J}} = 360^\circ/Y_{\mathrm{J}} = 54000^\circ/54787^{\mathrm{d}} \tag{13}$$

Multiplying this motion times eq. 12 gave:

$$V_{\mathrm{U}} = F_{\mathrm{J}} \cdot 94^{\mathrm{d}}1/4 = 92^\circ 54' \qquad\qquad S_{\mathrm{U}} = F_{\mathrm{J}} \cdot 92^{\mathrm{d}}1/2 = 91^\circ 10' \tag{14}$$

Next were found (using Ptolemy's conventional 60^{p} = unity):

$$x_{\mathrm{U}} = 60^{\mathrm{p}} \cdot \sin\frac{V_{\mathrm{U}} - S_{\mathrm{U}}}{2} = 0^{\mathrm{p}}54' \tag{15}$$

$$y_{\mathrm{U}} = 60^{\mathrm{p}} \cdot \sin\frac{V_{\mathrm{U}} + S_{\mathrm{U}} - 180^\circ}{2} = 2^{\mathrm{p}}08' \tag{16}$$

So the UH eccentricity e_{U} was:

$$e_{\mathrm{U}} = \sqrt{x_{\mathrm{U}}^2 + y_{\mathrm{U}}^2} = 2^{\mathrm{p}}19' = 2^{\mathrm{p}}1/3 = 7/180 \tag{17}$$

And the UH apogee A_{U} was:

$$A_{\mathrm{U}} = \arccos\frac{x_{\mathrm{U}}}{e_{\mathrm{U}}} = \arccos\frac{0^{\mathrm{p}}54'}{2^{\mathrm{p}}19'} = 67^\circ 08' = 67^\circ \tag{18}$$

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[10] The Greeks used noninstrumental Babylonian observations of eclipses and stations; but none of these borrowings establish parametric dependence on Babylon; to the contrary, all the old Babylonian data were used with current Greek observations to deduce new Greek parameters.

[11] The *Almajest* used epoch Nab 1, while Hipparchos formally used Phil 1 (fn 7), as did the Handy Tables (fn 12). The constant difference is under $0'.1$.

[12] Errors of PH orbit in –145: VE, –10^{h}; SS, +2^{h}; AE, +6^{h}; rms = 7^{h}. (Due to rounding during the *Almajest* 3.4 mathematical deduction of the PH orbit, some of the 1^{d}/4-rounded founding data's errors are slightly different: VE, –9^{h}; SS, +3^{h}−; AE, +6^{h}.) Parallel UH errors in –127: −6^{h}; 0^{h}; +4^{h}; rms = 4^{h}. For any year, the UH−PH differences are: VE, +1^{h}; SS, −4^{h}; AE, −5^{h}. If we have ϵ_{U} = 180°05′ at –127/9/24 noon (eq. 28) and ϵ_{P} = 227°40′ at −323/11/12 noon (Phil 1; see Neugebauer 1975 p.984), then for all time the mean longitude difference $f_{\mathrm{U}} - f_{\mathrm{P}}$ is +4′.1 = –1^{h}.7 (found from eqs. 13 & 24).

C8 By comparison, *Almajest* 3.4 has for the PH solar orbit (after applying the foregoing procedure to the data of eq. 11):

$$e_{\mathrm{P}} = 2^{\mathrm{p}}1/2 \qquad A_{\mathrm{P}} = 65^{\circ}1/2 \tag{19}$$

And the real –130 values were:

$$e = 0.0351 = 2^{\mathrm{p}}1/10 \qquad A = 66^{\circ}1/2 \tag{20}$$

All these e are defined as double what is modernly called e, since Hipparchos' solar theory used the eccentric model. The UH values for e & A are both more accurate than the PH values. Also, A is better than e, in both orbits (PH & UH).[13]

C9 The ancients reckoned mean solar anomaly g from the apogee A; thus (using eq. 18):

$$g = f - A \quad \text{so} \quad g_{\mathrm{U}} = f_{\mathrm{U}} - 67^{\circ} \tag{21}$$

where f = mean longitude. The eccentric-model equation of center E is (using eq. 17)

$$E = -\arctan\frac{e\cdot\sin g}{e\cdot\cos g + 1} \quad \text{so} \quad E_{\mathrm{U}} = -\arctan\frac{\sin g_{\mathrm{U}}}{\cos g_{\mathrm{U}} + 180/7} \tag{22}$$

where, of course, the true longitude ϕ is:

$$\phi = f + E \tag{23}$$

and where

$$f = \epsilon + F\cdot d \tag{24}$$

(ϵ = mean-longitude-at-epoch; d = days since epoch.)

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[13] Toomer 1984 p.153 n.46 defends Ptolemy's copying Hipparchos' A_{P} ($65^{\circ}1/2$, in error by -6°, because obsolete after 280^{y} of equinoctial & apsidal precession), recommending the analyses of Petersen & Schmidt 1967, who assert (pp.74-83) that A_{P}'s original accuracy (at Hipparchos' epoch) was coincidental, as e_{P} was so poor. The point made is essentially true; however, the expected A error was under 4°, only 3/4 the expected e error. (See discussion below.) Thus, [a] Ptolemy's A error (-6°) was less excusable than indicated; and [b] the smallness of Hipparchos' A error (-1°) was fortunate, but not so unlikely as suggested on *ibid* p.83, which proposes at least a 14° interval in which A_{P} could easily fall by chance. This is a useful paper, but its pp.81-2 assume equal & independent (& large) errors for SS, VE, & AE, ignoring [a] IE error (which connects VE & AE errors; see above §A3 & §C1) as well as [b] superior SS accuracy (Rawlins 1985H). For predicting expectation-errors, we may compute using IE-related equinox error (from randomly mis-set IE) $u = 4^{\mathrm{h}}$ (R.Newton 1970 pp.11 & 15) and intrinsic SS random error $rs = 2^{\mathrm{h}}$ (Rawlins 1985H; also, contrast solstice & equinox accuracy in fn 12), adding in rounding errors (for $1^{\mathrm{d}}/4$ precision) rr = rms of deviations (uniform density in the interval $\pm 3^{\mathrm{h}}$) = $\sqrt{3}$ hrs. Since raw visual error in an equinox observation is trivial in the context of $1^{\mathrm{d}}/4$ rounding, it will suffice to set (the random equinox errors independent of u) $r_{\mathrm{V}} = r_{\mathrm{A}} = rr$; but for solstices, $r_{\mathrm{S}} = [rs^2 + rr^2]^{1/2}$hrs $= \sqrt{7}$ hrs. Empirical-observation expectations: $de/e = (F_{\mathrm{J}}/e)\cdot[(u\cdot\sin A)^2 + rr^2/2 + (r_{\mathrm{S}}\cdot\cos A)^2]^{1/2}$; $dA = (F_{\mathrm{J}}/e)\cdot[(u\cdot\cos A)^2 + rr^2/2 + (r_{\mathrm{S}}\cdot\sin A)^2]^{1/2}$. Thus, for Hipparchos' epoch (rendering overprecisely): $de/e = 4^{\circ}.7$ & $dA = 3^{\circ}.7$. (For Ptolemy's: $de/e = 4^{\circ}.8$ & $dA = 3^{\circ}.6$. Note that A is more accurate than e from A's proximity to SS, which lowers dA sensitivity to the dominant error-source u.) These standard deviations are statistically consistent with the actual UH orbit, where $de/e = +6^{\circ}$, and $dA = +1^{\circ}/2$. But the error in e_{P} is statistically significant for both epochs. (PH errors: $de/e = +11^{\circ}$ & $dA = -1^{\circ}$ for Hipparchos; $de/e = +11^{\circ}$ & $dA = -6^{\circ}$ for Ptolemy.) The difference here is that Hipparchos eventually corrected his PH errors by years of honest outdoor labor (resulting in the UH orbit), while Ptolemy couldn't be bothered to do more than plagiarize the PH orbit (unaware that it was doubly obsolete). It should be added that Kallippos' 330 BC solar theory was superior to either the PH or the UH orbit (Neugebauer 1975 p.627 n.9, van der Waerden 1984-5 p.116).

C10 From eq. 21-23, f at the cardinal points of the UH solar orbit may be calculated:[14] $f_{\mathrm{VE}} = -2^{\circ}03'$, $f_{\mathrm{SS}} = 90^{\circ}52'$, $f_{\mathrm{AE}} = 182^{\circ}03'$, $f_{\mathrm{WS}} = 269^{\circ}08'$. (PH: $f_{\mathrm{VE}} = -2^{\circ}10'$, $f_{\mathrm{SS}} = 90^{\circ}59'$, $f_{\mathrm{AE}} = 182^{\circ}10'$, $f_{\mathrm{WS}} = 269^{\circ}01'$.)

C11 Thus, we know the mean longitude f for any observed cardinal time. (E.g., once the UH orbit is adopted, an observation placing the SS at –134/6/26 6 AM empirically sets f for that moment equal to $f_{\mathrm{SS}} = 90^{\circ}52'$, only $0^{\circ}.1$ from the truth: $90^{\circ}46'$.) And the mean-longitude-at-epoch ϵ is thereby determined through eq. 24. (See fn 14. This is effectively the method of *Almajest* 3.7.) Since Hipparchan solar mean motion departs so little (under $2'$) from Kallippic during a decade, ϵ is only slightly affected by the exact choice of epoch among Hipparchos' final few years of observational labors.

C12 We recall (§B8) that Hipparchos defended his famous yearlength $Y_{\mathrm{J}} = 365^{\mathrm{d}}14'48''$ (eq. 7) on 2 different occasions near the end of his life; thus, his UH value for F was very likely that of eq. 13, namely F_{J}.

C13 So we have now four UH orbital elements (e_{U}, A_{U}, ϵ_{U}, F_{J}) empirically established and-or adopted by Hipparchos late in his career. These constitute a complete determination of the UH solar orbit.

D The UH Orbit Restored to Life

D1 When I first noticed the fact that two of Hipparchos' 3 solar orbit cardinal cornerstones had shifted (some years after he had in –145 arrived at his PH orbit), I performed some of the above UH calculations (eqs. 12-18) in rough fashion (1985/3/12, scribbling right on p.58 of my copy of Neugebauer 1975) — but was too dumb & ignorant to see any way of testing the outcome, lazily supposing at the time that any evidence would have been interred along with the UH orbit itself (since Ptolemy preserves only the PH tables & parameters).

D2 But on 1986/5/15, while examining a list containing 3 very late Hipparchos lunisolar observations (R.Newton 1977 p.148), I was struck by some glaring discrepancies between Hipparchos' solar positions and values calculated from the PH tables. The magnitude (c.$1^{\circ}/4$) of the discords (and the fact that they peaked in the Summer) naturally reminded even me of the UH theory. These three solar position data are provided in *Almajest* 5.3&5, and each is there subsequently recomputed (seemingly by Ptolemy; vs. §H5), virtually correctly, to agree with the PH tables. The 3 Hipparchan data ϕ_{i} are as follows [with Ptolemy's corresponding reported PH recomputations beside in brackets]:

$$\phi_1 = 128^{\circ}7/12\ [128^{\circ}1/3] \quad \text{at} \quad -127/8/5\ 1/4 \tag{25}$$

$$\phi_2 = 37^{\circ}3/4\ [37^{\circ}3/4] \quad \text{at} \quad -126/5/2\ 1/4 \tag{26}$$

$$\phi_3 = 100^{\circ}9/10\ [100^{\circ}2/3] \quad \text{at} \quad -126/7/7\ 2/3 \tag{27}$$

D3 These 3 (unbracketed) solar true longitudes were Hipparchos' own calculated values, each used for setting ring 5 of his astrolabe (reference-object ring; see Fig.1 and Appendix A of Rawlins 1982C)[15] for a daytime measurement of the lunar longitude.

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[14] E.g., $f_{\mathrm{AE}} = 182^{\circ}03'$ in eq. 21 produces $g_{\mathrm{AE}} = 115^{\circ}03'$; this in eq. 22 yields $E_{\mathrm{AE}} = -2^{\circ}03'$. Therefore, from eq. 23, we obtain $\phi_{\mathrm{AE}} = f_{\mathrm{AE}} + E_{\mathrm{AE}} = 182^{\circ}03' + (-2^{\circ}03') = 180^{\circ}$, which is the very definition of the AE. Presuming an accurate Hipparchos AE observation at –127/9/26 1/2: from eq. 24, mean-longitude-at-epoch $\epsilon_{\mathrm{U}} = 182^{\circ}03' - F_{\mathrm{J}}\cdot 2^{\mathrm{d}} = 180^{\circ}1/12$ for UH epoch Phil 197 (eq. 28), 2^{d} earlier. (I suggest in §F4 that this is the Star Catalog's formal epoch. Compare *Almajest* 7.3, 5.3, and 3.1 dates.) PH's ϵ_{P} from –145/9/27 1/4 AE: $\epsilon_{\mathrm{P}} = 182^{\circ}10' - F_{\mathrm{J}}\cdot(-6572^{\mathrm{d}}1/4) = 180^{\circ}$ exactly (instead of $\epsilon_{\mathrm{U}} = 180^{\circ}05'$) at –127/9/24 epoch (correct within $1'$), a neat number which could help explain later general preference for the PH orbit.

[15] Doubtless without the slightest relation to vengeance, the 1987/8&11 issues of the allegedly space-tight *Journal for the History of Astronomy* (*JHA*) spent a chaotic 81 pp. (using contributions by 3 authors) — over 25% of the entire *JHA* regular 1987 output! — attacking Rawlins 1982C (& R.Newton 1977 pp.245-254). All this was arranged and

D4 The solar $\phi_{\rm i}$ are the only such records we have from Hipparchos that were computed at a known date[16] and all are from the conclusion of his empirical work. Indeed, they are embedded in the very last three precisely dated observations we have inherited from him. So they are ideal for testing the theory of the existence of the UH orbit.
D5 From any ϵ determined by $1^{\rm d}/4$-rounded Hipparchan cardinal-point observations (Kallippic-interval-accordant with the improved data of §C4) for about the year –130, we calculate $\phi_{\rm i}$ values from the UH orbit (for the 3 times given in eqs. 25-27) and thereby encounter the delightful result that in all 3 cases the computations agree to about $1'$ with the values given by Hipparchos and relayed in *Almajest* 5.3&5. For context, it is important to realize that 2 of these 3 longitudes were formerly believed to be grossly discrepant (ϕ_1 by $+15'$ and ϕ_3 by $+14'$; see eqs. 25 & 27) because they were supposed to have been calculated from the PH solar tables of the *Almajest*.
D6 Though computations of E can be rough by about $1'$ from tabular interpolation, I will nonetheless be precise (using the rigorous eq. 22) while here seeking the epoch Hipparchos adopted for the UH orbit. Examining the reported fractions of degrees (eqs. 25-27), we can see that $1'$ differences are important in this search because: had ϕ_1 come out equal to $128°36'$, it would have been expressed as $128°3/5$, not $128°7/12$ as reported in *Almajest* 5.3; were ϕ_2 equal to $37°47'$, *Almajest* 5.5 would have $37°4/5$ rather than $37°3/4$; had ϕ_3 been $100°52'$ or $55'$, *Almajest* 5.5 would say $100°5/6$ or $11/12$, instead of $100°9/10$.
D7 These considerations, and awareness of the ancient practice of adopting mean-longitude-at-epoch ϵ rounded to the nearest $1°/12$ (a point much developed in Rawlins 1985K), assist in delimiting possible epochs. The most probable candidate[17] occurs in 128 BC (noon here refers to Alexandria or Rhodos local apparent noon):

$$\epsilon_{\rm U} = 180°1/12 \ \text{ at } \ -127/9/24\ 1/2 = \text{Nab 621 or Phil 197 Thoth 1 noon} \qquad (28)$$

This $\epsilon_{\rm U}$ was off reality by $+4'\pm1'$ in –127; same error as PH's $\epsilon_{\rm P}$ in –145. (See fn 13 & data of fn 12. Mean equinox error is in both cases about $-1^{\rm h}1/2$, which is $+4'$ in f.)
D8 Note: –431, –279, & –127 are at two-Kallippic-cycle intervals. So, Hipparchos presumably intended to found his own calendar: $304^{\rm y}$ after Meton, at the epoch –127.[18]

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published (at vast expense in effort, funds, & page-space) just as an unexpected new independent proof of Rawlins 1982C's central thesis appeared (§F5). Nice timing. The self-evident flaw, in the sole coherent pro-Ptolemy point made by the *JHA* assault, was swiftly exposed by K.Hertzog *QJRAS 29*:279; 1988/6. (This only goaded Ptolemists into 3 more try-anything rear-guard meanderings, attempting to alibi Ptolemy on the Star Catalog matter, all 3 appearing in the 1990 output of O Gingerich's incurably partisan *JHA*. See also Graßhoff 1990.) *JHA*'s massive 1987 offensive was launched though: [a] no undoing DR errors are found, and, in a perfect expression of the wellknown British sense of fair play, [b] DR is barred from appearing in the very *JHA* that attacks him. In a 1983/3/3 letter, the *JHA*'s coolheaded Editor-for-Life (EfL) told DR never again to submit a paper to the extremely handsome *JHA* and intimated a libel action — all because DR had committed the unforgivable offense of pointing out the baselessness of a 1982/10 *JHA* paper. (See ‡8 fn 35, and ‡1 fn 25.) To replace *JHA*-referee-approved-&-accepted Rawlins 1999 (which the EfL personally despised & so had already held up for nearly a year), the recently-received 1982/10 paper (suitably mild in its criticism of Ptolemy) had been suddenly rushed to press by the EfL over the protests, of *JHA*'s own 2 referees, that its conclusions were unbelievable. (Yet further prescient EfL timing: just after EfL's suit-threat, the honest author's creditable retraction arrived on the desk of a now-even-further-enraged EfL. At this contretemps, the EfL's formerly hurried pace suddenly went glacial, thus postponing the retraction's publication until the 1984/6 *JHA*!) Just another enlightened episode in a proud Hist.sci community's ongoing demonstration of its academic idealism.

[16] The specialized Hipparchos equinox-solstice data of *Almajest* 3.1 are observed, not calculated. Previously, we did not know when the six solar positions of *Almajest* 4.11 were computed. They are all consistent with the PH solar orbit, so we may now say that these calculations preceded –127/8/5 (see §E1). [Misread corrected *DIO 1.3* fn 198.]

[17] Hipparchos' computation of $\epsilon_{\rm U}$ is reconstructed in fn 14. Note that the *Almajest* wrongly assumes Rhodos' longitude equals Alexandria's (Toomer 1984 p.225 n.16) and uses the equation of time solely for the Moon (not the Sun, though this habit was perhaps inadvertently reversed for the –126/7/7 observation; Toomer 1984 p.230 n.23).

[18] Hipparchos' cycle = 4 Kallippic cycles = $304^{\rm y}$ = $111035^{\rm d}$ (Heath 1913 p.297 or Neugebauer 1975 pp.297 & 624). If this cycle started at the epoch of eq. 28, then he figured it & Kallippic cycles from Thoth 1, as suggested at

D9 Below, I calculate (via eqs. 13, 21-24, 28) the UH solar longitude $\phi_{\rm i}$ ($f_{\rm i}$ & $E_{\rm i}$ computed precisely before $1'$ rounding), for each of the 3 times given in eqs. 25-27 (result then rounded according to ancient astronomical convention):

$$f_1 + E_1 = 130°33' - 1°58' = 128°35' = 128°7/12 = \phi_1 \qquad (29)$$

$$f_2 + E_2 = 36°41' + 1°05' = 37°46' = 37°3/4 = \phi_2 \qquad (30)$$

$$f_3 + E_3 = 102°08' - 1°15' = 100°53' = 100°9/10 = \phi_3 \qquad (31)$$

D10 Each of these UH results is identical with the corresponding reported Hipparchos value (eqs. 25-27), which leaves no doubt that the UH orbit really existed and that Hipparchos himself used it to compute these three $\phi_{\rm i}$ at the time — just before going outside to observe the Moon. And we mustn't forget that our success in connecting Hipparchos' three $\phi_{\rm i}$ data to the UH orbit also reconfirms the dependence of ACT #210 upon him, since it was that invaluable Babylonian text which provided us (§B1) the hour of the Hipparchos –134 solstice and thereby made possible our complete reconstruction here of the UH orbit.[19]
D11 Likewise, the false hour 6 AM (eq. 6) reported in *Almajest* 3.1 (c.150 AD) for Meton's solstice is shown to have been accepted between 135 and 68 BC (§B9), though (Rawlins 1985H) it was not known to Kallippos (330 BC) or Aristarchos (280 BC).[20]

E The UH Orbit's Accuracy & Fate

E1 The UH theory of the Sun was adopted by Hipparchos sometime between –134/6/26 (eq. 6) and –127/8/5 (eq. 25). It roughly halved the rms error of the old PH solar tables relayed in the *Almajest* — and virtually eliminated the prime source of error for eclipse-times, since the periodic error in the UH solar motion was very nearly matched by the then-unknown annual term of the lunar motion. The impressive accuracy of the UH eclipse theory must (if the solar orbit empirical foundation was indeed equinox-solstice observations) be partly just chance; but it is striking nonetheless. During eclipses, the largest term of the lunar theory's longitude error (sign convention: Hipparchos-minus-real) was annual: $-14'\sin g$, where g = solar anomaly. The next-biggest missing syzygial lunar terms possess amplitudes $5'$, $4'$, $3'$, and $2'$. The predominant term of the UH solar longitude error was $-13'\sin g$ (vs. the corresponding PH orbit error term: $-23'\sin g$; see §F3 & §F1); and no other UH solar error term's amplitude exceeds $1'$. Thus, since the $-14'$ and $-13'$ terms virtually cancel, the UH theory predicted eclipses with (noting the other terms, & using eq. 10):

$$\text{error} = \sqrt{(5'^2 + 4'^2 + 3'^2 + 2'^2)/2} \cdot M/360° = 10^{\rm m} \qquad (32)$$

(vs. $16^{\rm m}$ rms error for the PH orbit's eclipse predictions).
E2 Whether the UH theory was ever published is doubtful. Ptolemy's innocence of it proves nothing.[21] But there is other evidence.

Toomer 1984 p.214 n.72 (though with a $1^{\rm y}$ base discrepancy: fn 27). If the traditional SS was used instead, then the epoch was the UH (& real) SS at $-127/6/26\ 0^{\rm h}$. (Against SS-base: [a] The entailed $\epsilon_{\rm U} = 90°52'$, which is not near a rounded fraction of a degree. [b] The interval since −431/6/27 1/4 is $1^{\rm d}/4$ short of $111035^{\rm d}$. [c] Fn 27.)

[19] The foregoing analyses, down to this point and through §E1, were briefly set forth in a 1986/5/19 letter to Curtis Wilson, 4 days after the discovery of eqs. 29-31.

[20] The eq. 6 Meton date was known to both men (Rawlins 1985H). Also known in 109 BC (R.Newton 1977 p.95). The original Meton solstice was correctly recorded as occurring on the Athenian day starting −431/6/27 6 PM; but typical calendar-convenient adoption of the day-start as SS (rather than the actual SS hour, −431/6/28 10 AM) produced the usual negative truncation-error in the recorded SS (a practice 1st recognized at Rawlins 1985H): $-16^{\rm h}$ in the −431 instance. (See below at §E5.)

[21] E.g., he also never knew that the mature Hipparchos had recomputed his prior klimata table on the basis of a correct obliquity value, not the erroneous one Ptolemy attributes to him: Rawlins 1982C p.368. Note that even

E3 We know (§B9, §D11) that someone in roughly 100 BC used the Meton solstice of −431, with the exact same (terribly incorrect) dawn hour later reported by Ptolemy. There is a problem here (justly emphasized by R.Newton 1977 p.95): how could the famous Meton solstice's hour (eq. 6) have been in perfect agreement with the PH tables (even while in outrageous discord with the real sky: -28^{h} error!) — though the PH tables did not exist and were not accurate until nearly 3 centuries later? The coincidence has suggested to some (R.Newton 1977 p.96 & Rawlins 1985H, contra §E5 here) that the Meton solstice's conveniently false hour was not observed but was fabricated sometime after –145 from the PH tables.[22]

E4 Regardless (& I now doubt fabrication here: §E5), the Meton date & hour of eq. 6 existed well prior to Ptolemy (as found in §B1), who is not responsible for any of the confusion regarding the Meton solstice. (R.Newton 1977 p.96 earlier guessed he was not. Rawlins 1985H demonstrated it.) And, though the eq. 6 date was used continuously, the eq. 6 hour first appeared between 280 BC and 68 BC (§D11), probably about 146 BC (Hipparchos: §E5).

E5 Rawlins 1985H innocently explains (& thus accepts as real) the date of the Meton solstice. (See above, §D11.) I have since decided that it is not necessary to assume fabrication for the hour either, because this can be accounted for as merely a Hipparchan warp of prejudice. When constructing his PH solar orbit (146 BC; fn 7), Hipparchos would have been delighted to confirm the lunisolar-calendar-convenient false tropical yearlength of Aristarchos-Sudines (Rawlins 1999; Hipparchos later rounded this value trivially, to eq. 7). That encouraged Hipparchos to read "morning" for Meton's reference to his solstice having occurred at the "start" (αρχην) of the day[23] (by which Meton meant 6 PM, since the Athenian day began at dusk). This hypothetical Hipparchos miscue would append a -12^{h} misinterpretation-error to the -16^{h} truncation-error (Rawlins 1985H) that had already attached to the Meton solstice, probably from the outset (−431; fn 20) and certainly by 330 BC (*idem*). All of which left the now-notorious total of -28^{h} off: a gross error — but the 6 AM Meton hour adopted (eq. 6) was attractively consistent with the PH solar theory (which was based on Hipparchos' solar observations in –145, and the by-then long-established 3rd century BC Aristarchos-Sudines yearlength effectively preserved by Hipparchos in eq. 7; see fn 22).

E6 When he died c.127 BC, Hipparchos was presumably working at an improved lunar theory (thus the quadrant observation of *Almajest* 5.3 and the octant data of *Almajest* 5.5),[24] perhaps planning to publish it and the UH solar orbit together as a lunisolar unit. Instead, his PH solar tables became standard throughout the pagan world community, even as late as the 4th century era of Julian the Apostate and Theon of Alexandria. Had Hipparchos ever issued something so basic as an improved solar orbit, such would likely have long since been generally adopted in place of the PH calendar. It is regrettable that Hipparchos probably never published the UH orbit, since its periodic errors were barely half those of the PH solar tables that became canonical among astrologers for the worst part of a millennium.

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nonmathematician Strabo was aware of the later klimata table: see the admirable analysis by nonmathematician Diller 1934, which Neugebauer 1975 p.734 n.14 typically damned as incompetent & "absurd" — a cocksure denigration published, ironically, just before Diller's triple independent vindication by Rawlins 1982C p.368 and Nadal & Brunet 1984 p.231 n.17.

[22] Pre-empirical Hipparchan adoption of PH's eq. 7 was perhaps via Sudines, c.240 BC (Rawlins 1999, & see Neugebauer 1975 p.624 & 574).

[23] See, e.g., the possibly-revealing *Almajest* 3.1 language at Ptolemy's 2nd mention of this solstice's hour. Toomer 1984 p.139 innocently obscures the matter by presumptively translating αρχην as "dawning" (just as I suspect Hipparchos did). All other translators scrupulously retain the original meaning: see Manitius 1912-3 1:144; also Halma 1:163, and Taliaferro (Great Books v.16) p.82.

[24] Can one imagine a genuine observer (which Ptolemy pretends to be, throughout the *Almajest*) using 3-century-old data to establish fine details of the Moon's oscillations about its mean motion? Equally obvious giveaway symptoms of Ptolemy's innocence of real astronomy (e.g., fn 2 & fn 37; and Rawlins 1985G & Rawlins 1987) make equally little impression on the equally indoor Muffia.

F Unexpected Fruit

On 1986/11/20, about 2 months after sending the foregoing discussion (nearly as it appears above) to B.van der Waerden & R.Newton, I followed up with a letter to R.Newton (copy to BvdW), from which most of the rest of this section (& the next) is taken, with some revision. The letter carried news of a pleasant discovery: fresh confirmation (1986/10/29) of the Ultimate Hipparchos Orbit.

F1 The prime dubious point in my detailed analysis of Hipparchos' Ancient Star Catalog (Rawlins 1982C)[25] was its attribution (pp.366-371) of the Sample A (zodiacal stars) longitude error curve (solid line in Fig.3, *ibid*) to pre-solar-theory use of raw equinox observations for zero point: if intelligently applied, this method would more likely produce a zigzag or step-function error curve, not the sinusoid that is the case (a point I found puzzling at the time: Rawlins 1982C p.370). But the Hipparchos (PH) solar theory periodic error was about $-23'\sin(f-62^\circ)$, while $(-12' \pm 1')\cdot\sin(f-92^\circ \pm 3^\circ)$ was the Star Catalog's periodic error (*ibid* p.376 Table IV). The amplitudes were incompatible. So, believing (when I wrote Rawlins 1982C) that there was but one Hipparchos solar orbit, I could make no progress in relating the Sample A longitude error curve to a Hipparchos solar orbit error curve.

F2 [Subsequent to this 1986/11/20 letter, a fresh DR study of the Catalog zodiacal stars, using the constellations as weighted normals and dropping discordant Cap, finds (for –127): mean error $z = -10'\pm1'$ (vs. $z = -8'\pm1'$ from the analyses of Rawlins 1982C pp.367&9), periodic error $(-14' \pm 1')\cdot\sin(f-101^\circ \pm 6^\circ)$. This solution is slightly different from — though statistically consistent with — the Rawlins 1982C solution just given in §F1. Both solutions are quite incompatible with the PH orbit's error curve (§F1), though their amplitudes are close to that of the UH orbit.]

F3 The Rawlins 1982C incompatibility problem now evaporates. The Ultimate orbit (UH) periodic error curve was about $-13'\sin(f-71^\circ)$. The amplitude's match to that of the Star Catalog error curve is lovely! Also, I see that the epoch I proposed (eq. 28: –127/9/24 1/2), for the UH orbit, in the ms (i.e., the above paper, §A-§E, sent RRN & BvdW 1986/9/16), is almost exactly the anciently accepted date of the Star Catalog.[26]

F4 For, in a previously disputed[27] passage, *Almajest* 5.3 says –127/8/5 is in the 50th year of the 3rd Kallippic cycle, which is the very same Kallippic calendar year Ptolemy believes

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[25] Revision to another overt Rawlins 1982C speculation: most of the Catalog (outside of Samples C & A) was observed using Hipparchos' pre-135 BC solar theory & obliquity. (And a few areas' star positions were based on rounded transit data, e.g., Ara, PsA inf, and parts of Argo & Cen.)

[26] The UH epoch, –127/9/24 noon (eq. 28), was 264 Egyptian yrs (264^{E}) before Antoninus 1 = +137/7/20 noon, Ptolemy's star data epoch (*Almajest* 7.4), whereas Ptolemy says in *Almajest* 7.2 that the interval was about 265^{E}. *Almajest* 7.1 says about 260^{E}; *Almajest* 7.3 says 265^{E}, but the concomitant use of 2°2/3 precession implies 267^{E} & thus a Star Catalog epoch of –130 (which I believe was the Catalog epoch effectively adopted by Ptolemy when he dealt with precession corrections; see Rawlins 1985K & fn 14).

[27] Since –127/8/5 is in the 51st year of the 3rd of Kallippos' original tropical-year cycles, calendar-specialists have been tempted to alter the *Almajest* 5.3 text. Toomer 1984 p.224 n.13 (& p.13) carefully makes it clear that these attempted emendations of the unambiguous text have no support in the *Almajest* mss. As I realized only very recently (1988/12/5), the discrepancy that has upset scholars for so long entirely vanishes if, in Hipparchos' calendar, the 50th Kallippic year was Egyptian in length & ended in –127 not at the SS but at Thoth 1 — which is a natural consequence of eq. 28 (see fn 18). Toomer 1984 p.214 n.72 perceptively makes just such a suggestion for Hipparchos' Kallippic dates of 201-200 BC (*Almajest* 4.11) — but is forced by the data to set forth a scheme which (unlike that I propose above for –127) has the tropical & Egyptian versions of the same-number Kallippic years only barely overlapping, which suggests that it is off by 1^{y}. By coincidence, as Toomer 1984 p.224 n.13 rightly realizes, his numbering-scheme differs by 1^{y} from the foregoing one — which exhibits far better same-number overlapping and, as we found (above), perfectly explains the hitherto-troubling –127 Kallippic date at *Almajest* 5.3. Toomer's 1^{y} calendaric discrepancy may just be from an ancient confusion about the 201-200 BC data (presumably due to the switch from SS to Thoth 1). Or, conceivably, a Hipparchan numbering shift occurred between 200 BC & 128 BC, due to the difference in length of Egyptian & Hipparchan years (possibly with respect to a longer cycle, say 8105555^{d}). In any case, we cannot now improve on the closing remark of Toomer 1984 p.224 n.13.

was the Star Catalog's epoch, as we see from the *Almajest* 7.2 date of Hipparchos' Regulus longitude ($119°5/6$, identical to the Catalog value).

F5 The relating (§F3) of these error-curves: [a] adds yet another obvious proof[28] to the overflowing arsenal of evidences (e.g., R.Newton 1977 p.250, Rawlins 1982C) that Hipparchos (UH), not Ptolemy (PH), was the Catalog's true observer, and [b] has made possible the completion of my reconstruction of Hipparchos' Catalog compilation process. I noted (Rawlins 1982C p.373) that Ptolemy's alleged use in *Almajest* 7.2 of a huge elongation from Sun to Regulus (when determining Regulus' longitude) was folly since it only accentuates (by accumulation) the physical imperfections in the astrolabe's ecliptic ring.[29] (And, of course: had principal stars — or ordinary catalog stars — been fixed using elongations of large and thus virtually random size, as Ptolemy falsely indicates in *Almajest* 7.2, then there would be virtually no periodic error at all in the Catalog.) Hipparchos did the job the right way, keeping the elongation to a minimum — thus unwittingly preserving the UH solar theory error curve's amplitude (as we saw above in §F1-§F3: $13'$ agrees very nicely with $12'$ or $14'$, both $\pm 1'$), as well as keeping the UH-to-Catalog phase shift fairly small (c.20°-30°).

G Hipparchos' Observing Routine

Hipparchos' astrolabe procedure for locating his principal stars' positions with respect to the Sun (using the Moon as a stepping stone, as described in *Almajest* 7.2):

G1 Hipparchos virtually always found his Sample A principal stars at sunset, not sunrise.[30] (That accounts for the phase shift being positive with respect to the Star Catalog phase of §F1: $92° - 71° = +21°$; or, for the alternate solution of §F2: $101° - 71° = +30°$.) Which tells us something about his sleeping habits!

G2 In RA, the principal star being observed was (on average) about $1^{\text{h}}1/2$ ($= 22°1/2$) or 2^{h} ($= 30°$) east of the Sun. This explains very nicely the shift in phase from 71° (UH orbit) to 92° or 101° (Star Catalog). And it tells us that the stepping-stone Moon was ordinarily a very young waxing crescent (c.2^{d} old), right next to the desired star.

G3 Each step in the principal-star-fixing-process involved setting ring 5 on the reference object (Sun in Step 1, Moon in Step 2 — see Rawlins 1982C App.B), then clamping the unit comprising rings 3 & 4, i.e., freezing axis dd and quickly turning ring 2 to line it up so that the desired star (being located by this procedure) seemed visually to "adhere" to ring 2's side (as *Almajest* 5.1 speaks of ring 5's use). (No need for sighting the star through pinnules; too time-consuming, and latitude already known from older Sample A$'$: Rawlins 1982C pp.367 & 369.)

G4 The longitude of each catalog star is based on 3 astrolabe observations (except the few principal stars: 2 observations each):[31] Sun to Moon; Moon to principal star; principal star to ordinary star being cataloged. (See Rawlins 1982C App.B.) For 128 BC (eq. 28: more exactly, –127/9/24 1/2 = Besselian date –126.278), there is a systematic longitude discrepancy (between the Star Catalog & the UH orbit) of about $-13' \pm 3'$: the $-9' \pm 2'$ mean Star Catalog error[32] (average of the two z estimates of §F2) minus the UH orbit's own mean $+4' \pm 1'$ error (§D7). According to eq. 1 of Rawlins 1982C p.361, the longitude differential $-13' \pm 3'$ corresponds to systematic net lateness $57^{\text{s}} \pm 13^{\text{s}}$. So, since there are 3 observations involved for each star (as just explained), we see that Hipparchos' average time between clamping axis dd and fixing ring 2 onto any desired celestial object was $19^{\text{s}} \pm 8^{\text{s}}$. (I have here conservatively tended to round these random error calculations on the high side.) Reasonable.

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[28] Which millennium will see Muffia acknowledgement of this in any of its various captive journals?

[29] Ring 3. See Rawlins 1982C Fig.1 (or Toomer 1984 p.218 Fig.F, where ring 3 is unfortunately drawn not quite perpendicular to axis ee). And note that near-syzygy is the region where Hipparchos best knew the Moon's motion (though his lunar theory is used only differentially for astrolabe star-locations, as in *Almajest* 7.2).

[30] Tiny Sample B (14 principal stars: Rawlins 1982C pp.366-7) is much less consistent than Sample A, so it may be hybrid. The poor definition of Sample B's phase may also have been affected by separate (non-A) positionings of some stars (e.g., Regulus) and-or by a hypothetical traditional demand that the longitudes of Aldebaran & Antares (cardinal ecliptical stars) be exactly 180° apart — which, incidentally, they really were, within $1'$, for roughly 1500^{y} starting about 300 BC. Since these 2 stars can never be seen simultaneously from the Mediterranean area, this striking knowledge (precisely embedded in Hipparchos' Star Catalog) provides yet another hint suggesting the existence of accurate empirical ancient astronomy.

[31] For –127, the z for Samples A and B are indeed roughly in a ratio of 3 to 2; however, Sample B is not large enough to permit us to call this a statistically significant confirmation.

G5 It's remarkable that all this detailed knowledge about a wellknown Hellenistic astronomer might never have come to light, were it not for a single precious Babylonian cuneiform text: ACT #210.

H Postscript

Two prior scholars deserve credit for getting close to discovering the UH orbit.

H1 Hartner 1979 p.18 analysed[33] Y_{B} and went as far as realizing that a factor of 99 was involved in its remainder, supposing that the originators of Y_{B} had founded it by using data separated by 99^{y} and expressed to a precision of 2^{h} (or 8^{h}). Had he added in the Greek habit of rounding to 6^{h}, he would have tripled 99^{y} to find 297^{y}, which would have probably led to his finding the dependence of ACT #210 upon Greek data.

H2 I only recently noticed that Britton 1967 pp.45-47 actually proposed that the 3 solar data of *Almajest* 5.3&5 show that Hipparchos had a different solar orbit than that used by Ptolemy. But Britton then for some reason states (p.47) that these 3 data do not provide enough information to reveal the orbit. So I tried a solution based just upon the 3 Hipparchan ϕ_{i}, & thereby discovered that some idea of the UH orbit could in fact have been attained from them alone.

H3 Any alteration in the mean motion F would have a negligible effect upon the spacing of these data; thus, the precise values of only 3 solar orbital elements are contingent upon the three ϕ_{i} (unbracketed values in eqs. 25-27), a situation which permits a determination of these elements from the data. Allowing for $1'$ error in the 3 data, our solution finds: eccentricity $e = 2^{\text{p}}19' \pm 02'$; apogee $A = 69° \pm 2°$; mean-longitude-at-epoch $\epsilon = 180°01' \pm 04'$.

H4 The results of the solution are statistically consistent with the UH orbit. However, since all 3 data are bunched in only about 1/4 of the zodiac, the unavoidable $1'$ data uncertainties introduce disappointing lassitude into some elements. (One cannot expect here the precision attained above in §C7, where we used sharply defined seasonlengths covering 1/2 the zodiac.) However, the solution for e is utterly incompatible with the PH orbit. (Thus, just from the 3 *Almajest* data he was commendably the first to propose the significance of, Britton could have found at least this element of the UH orbit to high precision, and could additionally have realized that this orbit's A was probably higher than the PH value.)

H5 I do not criticize either Hartner or Britton, especially since a 2nd look at these 3 data, triggered by reading Britton's near-miss, turned up (1988/7/7)[34] a highly revealing

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[32] I tentatively used a similar constant on p.369 of Rawlins 1982C to determine (assuming null systematic error in z) the "formal" epoch of Sample A as about –136, a figure I now withdraw.

[33] On 1982/1/15, O Gingerich stated approvingly that Hartner 1979 was regarded by the Muffia as symptomatic of its author's incipient senescence. (Like van der Waerden, Hartner always tried to ignore such clutter, to credit Neugebauer for his contributions.) Just customary Muffia intellectual generosity — and this from a clique that still (Neugebauer 1975 p.528) thinks Y_{B} is sidereal!

[34] Looking back to my first discovering the connection of ACT #210 to Hellenistic data, I see that my evolving awareness of the evidence for the UH orbit encompassed at least 6 independent discoveries, accomplished over more than 6 years of research: §A6 1982/1/28, §B1 1985/1/25, §D1 1985/3/12, §D10 1986/5/15, §F3 1986/10/29, & §H5 1988/7/7. The molassian slowness of wit thus revealed, is still another reason why I am disinclined to be unsympathetically critical of predecessors working at problems which turned out to be related to the UH orbit.

item which I had myself previously overlooked.[35] Ptolemy provides what purport to be his own PH orbit computations of the three *Almajest* 5.3&5 solar data, finding agreement with Hipparchos' values in but 1 case, as already noted. But he here makes an awful blunder: in this single agreeable instance (observation #2), the mean longitude f_2 he displays (at *Almajest* 5.5, in a context of $1'$ precision) is $36^\circ 41'$, which is the UH value (eq. 30) on the nose! (The UH theory gives precisely $36^\circ 40'.6$; eqs. 13, 24, & 28.) And this f_2 is patently incompatible with the PH tables Ptolemy allegedly used in his computations. (The PH tables of *Almajest* 3.2 give mean longitude $36^\circ 36'.4$; using the Phil 1 $\epsilon_{\rm P}$ of fn 12 yields $36^\circ 36'.5-$.)

H6 The truth of the matter is self-evident: Ptolemy, a plagiarist of occasionally catastrophic carelessness (R.Newton 1977, Rawlins 1985G p.266, Rawlins 1987),[36] learned ahead of time that the 2nd of the three *Almajest* 5.3&5 Hipparchos solar data (for ϕ_2; eq. 26) did not disagree with the PH orbit. (The UH-minus-PH discrepancies in f & E happen to nearly nullify each other at this point in the solar orbit. Sheer accident, but likely seen by Ptolemy as just a case where Hipparchos didn't miscompute, since Ptolemy clearly saw the discrepant values, ϕ_1 & ϕ_3, eqs. 25 & 27, as mere calculating errors by Hipparchos.) Believing therefore that ϕ_2 didn't require recomputation, he in this sole case simply copied Hipparchos' figures (for ϕ_2 & f_2) directly into the *Almajest* without alteration.

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[35] Yet another illustration of my manifold limitations is afforded by my persistent blindness to the explanation for Ptolemy's 1° shift in the Hipparchan lunar nodal motion (over his $311784^{\rm d}$ interval, an equation I imparted to O Gingerich on 1983/6/6), given right in *Almajest* 4.9, which I prejudicedly ignored until having it pointed out by the generally excellent analysis of Hamilton, Swerdlow, & Toomer 1987. (Equation & interpretation preview-published in Toomer 1984 p.205 n.51 and Swerdlow & Neugebauer 1984 p.405 n.5. My 1986/5/19 & 6/19 notes to Hamilton, voluntarily acknowledging the superiority of HS&T's interpretation and requesting the date of Hamilton's discovery of the $311784^{\rm d}$ equation, have not been replied to.) Toomer&co were unquestionably the 1st modern scholars to realize that Ptolemy's Canobic Inscription preceded the *Almajest*. This recent paper's perspicacity is blemished only by repetition (p.65) of the suggestion by Swerdlow & Neugebauer 1984 p.405 n.5 that the Canobic Inscription's Mercury model might be normal (i.e, no crank), which if true would require this model to be utterly incompatible with Ptolemy's own crucial (*Alm*-model-foundation-stone) *Almajest* 9.10 Mercury position of 139/5/17, allegedly observed (1st-hand by him) more than 7 years before the CanInscr model was published (see Rawlins 1987 pp.236-237). Indeed, without a crank, tabular Mercury never attains (within $0^\circ.4$) geocentric longitude $77^\circ 1/2$ (reported in *Almajest* 9.10) at *any* time during its 139/5 swing around its stationary point. Thus, as a check would swiftly have shown HS&T, the math of *Almajest* 9.10, applied with a null crank radius, necessarily produces an imaginary solution for Mercury's synodic longitude ($24^\circ 56' + \arcsin[22^{\rm p}56'/22^{\rm p}30']$).

[36] For a potentially agnostic interpretation of the Mars period relation of Rawlins 1987, see Swerdlow 1973. This review (in *Isis*, the US' most prestigious Hist.sci journal) is so joyfully busy with the cute details of expressing its author's characteristically amiable & openminded reaction to dissent that, despite his distaste for "careless and unreliable" , "absurd", and "intelligence-insulting" scholarship (HamSwerdlow 1981 p.60-2 — published in *JHA*, which pretends to reject mss for strong language!), Swerdlow inexplicably [a] inverts his central conditional, and [b] attacks van der Waerden 1970 for taking Indian use of s as suggesting heliocentrism, though van der Waerden 1970 actually (p.30) instead points to λ.

[Note added 2003. The 1991 original of this footnote used DR's now-discredited argument that the *Almajest* 9.4 Mars tables were based upon an integral number of longitudinal (heliocentric) instead of synodic cycles. Though the tables indeed turn out (*DIO 11.2* p.30 & ‡4 fn 21) to be based upon longitudinal (not synodic) revolutions, the mechanism is not that originally proposed by DR.

For firmer indication of ancient heliocentrism's vitality (and counter-evidences), see: *ibid* §§G3-G4. See also Rawlins 1987 p.238 item IV[c], or here at ‡7 fn 8 & §§F3-F4, and *Almajest* 3.1's reference to "the school of Aristarchos".]

H7 The upshot is embarrassing for Ptolemy & the unfalsifiably ineducable[37] Hist.sci archons who have (originally with the best of intentions, one assumes) by now spent decades irrevocably committing their insecure reputations for sound judgement to the outlandishly ironic proposition that Ptolemy was the Greatest Astronomer of Antiquity (Princeton Institute's Neugebauer 1975 p.931, echoed verbatim by Harvard-Smithsonian's Gingerich 1976 & Gingerich 1980 p.264) and who have consistently fled a decade of challenges (e.g., Rawlins 1987 p.236) to face-to-face debate of the Ptolemy Controversy. But Ptolemy's giveaway f_2 oversight is fortuitously useful in that [a] it demolishes the sole glimmer of a potential last-ditch counterargument to the UH orbit's reality (namely, that at least one of Ptolemy's PH calculations agrees with Hipparchos: ϕ_2, eq. 30), and [b] it preserves unsullied the original rendition of Hipparchos himself — and this is a wonderful further verification of the UH orbit's use by Hipparchos: we actually glimpse the details of his UH mathematics, as he converted (eq. 30, using eqs. 21-23) a mean longitude ($f_2 = 36^\circ 41'$; §H5) into a true longitude ($\phi_2 = 37^\circ 3/4$). This is the sole surviving fragment of such eccentric-model solar computation by the very astronomer whose better-known PH solar tables (also eccentric-model) were used longer than any others in history.

DIO preprint distributed 1990/10/22 at American Astronomical Society meeting (Planetary Sciences Division), Charlottesville, VA. (Minor revisions since.) Basis of talk at AAS meeting 1991/1/14 (Philadelphia). Abstract in *Bulletin AAS 22.4*:1232 (1990).

References

Asger Aaboe 1955. Centaurus *4*:122.
Almajest. Compiled Ptolemy c.160 AD. Eds: Manitius 1912-3; Toomer 1984.
J.L.Berggren & B.Goldstein 1987, Eds. *From Ancient Omens to Stat Mech*, Copenhagen.
John Britton 1967. *On the Quality of Solar & Lunar Param in Ptol's Alm*, diss, Yale U.
J.Dickey & J.Williams 1982. EOS *63*:301.
Aubrey Diller 1934. Klio *27*:258.
Wm.Dinsmoor 1931. *Archons of Athens* . . . , Harvard U.
Gerd Graßhoff 1990. *History of Ptolemy's Star Catalogue*, NYC.
O.Gingerich 1976. Science *193*:476.
O.Gingerich 1980. QJRAS *21*:253.
N.Hamilton, N.Swerdlow, & G.Toomer. At Berggren & Goldstein 1987 p.55.
W.Hartner 1979. JHA *10*:1.
Thos.Heath 1913. *Aristarchus of Samos*, Oxford U.
Franz Kugler 1900. *Babylonische Mondrechnung*, Freiburg im Breisgau.
Karl Manitius 1912-3, Ed. *Handbuch der Astronomie* [*Almajest*], Leipzig.
R.Nadal & J.Brunet 1984. ArchiveHistExactSci *29*:201.
O.Neugebauer 1955. *Astronomical Cuneiform Texts*, London.
O.Neugebauer 1957. *Exact Sciences in Antiquity*, 2^{nd} ed, Brown U.

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[37] The Greatest Astronomer of Antiquity's fabrication of allegedly empirical data was so frequent, flagrant, & inept that he even perpetrates the nonpareil hilarity of assigning 2 different dates to the same "observation". (The 136 AD greatest evening elongation of Venus: 136/12/25 & 136/11/18, *Almajest* 10.1&2; see R.Newton 1985 p.10 & van der Waerden 1988 p.292. Muffiosi are typically impervious to their contextual problem: is it just coincidental that the very same astrologer whom skeptics have been pointing to for centuries as astronomy's most obvious faker has now been newly caught at the funniest muffed "observations" in the history of the field?) As I put it recently in the *American Journal of Physics* (Rawlins 1987 p.236): "That is, Ptolemy . . . states that he *observed* first-hand the *same celestial event* on two different occasions *thirty seven days apart* — a blunder unique in astronomical annals, and the coup-de-bloop for the notion that Ptolemy was a legitimate scientist." (A 1987/4/12 van der Waerden letter comments on this paper's detailing of a few among Ptolemy's various deceptions, emphasis in original: "*excellent*. The arguments — some of which are new . . . — are exposed with such a force and [clarity] that from now on nobody can shut his eyes to the clear facts." See also van der Waerden 1988 Chaps. 14, 19, & 20.) Nonetheless, Swerdlow & Neugebauer 1984 (p.377) and Toomer 1984 (p.469 n.1) swear that, when double-dating his Venus "observation", *Ptolemy knew exactly what he was doing*. Aren't they just adorable?

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 1982. *Origins of Ptolemy's Astronomical Parameters*, U.Maryland.
R.Newton 1985. *Origins of Ptolemy's Astronomical Tables*, U.Maryland.
Viggo Petersen & Olaf Schmidt 1967. Centaurus *12*:73.
D.Rawlins 1982C. Publications of the Astronomical Society of the Pacific *94*:359.
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 in-prep A.
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*.
Alan Samuel 1972. *Greek & Roman Chronology*, Munich.
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 & O.Neugebauer 1984. *Mathematical Astronomy in Copern*, NYC.
Gerald Toomer 1984, Ed. *Ptolemy's Almagest*, NYC.
B.Tuckerman 1962&64. *Planetary, Lunar, & Solar Pos*, AmPhilosSocMem *54*&*56*.
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 1984-5. ArchiveHistExactSci *29*:101, *32*:95, *34*:231.
B.van der Waerden 1986. Isis *77*:103.
B.van der Waerden 1988. *Astronomie der Griechen*, Darmstadt.