# ‡1 R. R. Newton versus Ptolemy

## by Hugh Thurston[1]

Readers of *DIO* will be familiar with the controversy over Ptolemy's honesty and competence. R. R. Newton was his most prominent critic. Ptolemy's defenders were mostly a collection of established academics (called "the Muffia" by Dennis Rawlins).

It is in fact hard work to extract Newton's cogent arguments, and — most important — the calculations supporting them, from his 411-page book *The Crime of Claudius Ptolemy* (published by Johns Hopkins University Press in 1977) and his other books and papers. I have not seen elsewhere a succinct exposition of his book's reasoning, so I give one here.

References of the form Cx are to page x in Newton's book. References of the form Sx.y are to chapter y of book x of Ptolemy's *Syntaxis*.

## A The Length of the Year (C84 to 94)

**A1** The obvious way to find the number of days in the year is to divide the time-interval between two summer solstices by the number of years between them. (Or two equinoxes.)[2] Hipparchus found the value 365 1/4 − 1/300 days. This was six minutes too long. The error in the result is the error in the interval divided by the number of years. The greater the number of years, the better the result. Ptolemy should have improved on Hipparchus. He didn't.

**A2** Ptolemy calculated the average length of the year three times (S3.1), each time comparing an equinox or solstice (which he claimed to have observed) with an earlier one. These data are set forth in Table 1. In each of the three cases used in S3.1, Ptolemy obtained exactly the same year as Hipparchus. The times that he gives for his "observations" are badly in error [⊙1] — by over a day on average — and are in each case off by just the amount needed to yield Hipparchus's over-long result. Clearly Ptolemy did not observe the equinoxes and solstice; he calculated times for them from the earlier observations, using the length of the year that Hipparchus had found. And, if you do this calculation yourself, taking morning to be a quarter of a day after midnight, and (like Ptolemy) express your results to the nearest hour, you will get precisely[3] the times and dates that Ptolemy cited.

**A3** This reasoning was first given by J. B. J. Delambre (*Histoire de l'astronomie du moyen âge*, 1819, page lxviij). Newton went one step further than Delambre by also bringing in an equinox that Ptolemy claimed [S3.7] to have observed on 132/09/25. and showing that it was actually calculated in just the same way [⊙2 & ⊙3].

## B The Obliquity of the Ecliptic (C96 to 102)

In Ptolemy's time, the correct value of the difference between the noon elevations of the Sun at the two solstices (which is twice the obliquity) was 47°21′. In S1.12, Ptolemy quoted repeated observations in which he found that this angle always lay between 47°40′ and 47°45′. The error is unreasonably large for the instruments that Ptolemy said he used. However, Ptolemy's result is extraordinarily close to a value which he imputed to Eratosthenes (and himself adopted as accurate), namely, 11/83 of a circle. Ptolemy said that

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[Hugh Thurston died on 2006/10/29. The lead article of *DIO 14* (2008) is dedicated to his memory.]
End-notes (section ⊙, pp.14-17) and footnotes are by D. Rawlins.

[2] Though, see *DIO 1.1* ‡6 §A3.

[3] See extra irony at *DIO 1.2* §E1 and fn 64.

Table 1: Equinoxes & Solstices

| Event | Earlier observation | Ptolemy "obs" | Actual Time | Error |
|---|---|---|---|---|
| Autumn Equinox | −146/09/27 midnight | 132/09/25 14$^{h}$ | 09/24 05$^{h}$ | 33$^{h}$ |
| Autumn Equinox | −146/09/27 midnight | 139/09/26 07$^{h}$ | 09/24 22$^{h}$ | 33$^{h}$ |
| Vernal Equinox | −145/03/24 morning | 140/03/22 13$^{h}$ | 03/21 16$^{h}$ | 21$^{h}$ |
| Summer Solstice | −431/06/27 morning | 140/06/25 02$^{h}$ | 06/23 14$^{h}$ | 36$^{h}$ |

Superscript h stands for hour. Times are from midnight. Dates are Julian [⊙3].

Hipparchus also used it, but in fact Hipparchus used first 47°50′ and later 47°20′.[4] (See Dennis Rawlins's statistical study [⊙4], "An Investigation of the Ancient Star Catalog", *Publications of the Astr. Soc. of the Pacific* volume 94 (1982), pages 367 and 368.)

## C The Epicycle of the Moon (C112 to 130)

**C1** Hipparchus assumed that the Moon moved on a simple epicycle and Ptolemy's theory was, for full[5] Moons, equivalent to this. It is possible to use the time-intervals between three eclipses of the Moon and the corresponding differences in the longitudes of the Sun to calculate the radius of the epicycle.

**C2** S4.6 and 4.11 quoted four trios of eclipses which yield values of 5;13 and 5;14 (S4.6), also [⊙5] 5;15 and 5;15 (S4.11) on a scale on which the radius of the circle on which the epicycle travels is 60 units.

**C3** These values are incredibly[6] close to each other. Newton calculated the effect of altering the time of the middle eclipse of the first trio by one hundredth of a day (about 1/4 hour) and he found that it reduced[7] the value obtained to 5;06 (C122).

## D The Sun's Longitude (C145 and 147)

Ptolemy said in S5.3 that he observed the Sun on 139/02/09 (6:45 Alexandria Apparent Time) and found [⊙6] its longitude to be 318°5/6. The longitude at this time as calculated from his tables was also 318°5/6. In S7.2 he reported [⊙7] a similarly measured [⊙8] and calculated solar longitude for 139/2/23 (17:30 Alexandria Apparent Time) to be about 333° and 333°1/20, respectively. During Ptolemy's era, his tables for the longitude of the Sun were out (on average) by over a degree. Consequently, if the observations had been genuine the calculations would not have agreed with the tables.

## E The Half-Moon (C145 to 158)

**E1** The simple epicycle theory gives reasonable values for the Moon's longitude at full Moon, and for the maximum difference between the longitudes of the mean Moon M and of the Moon itself, namely 5°. This is too small at half-Moon.

[4] The actual value in Hipparchus's time was about 47°25′.

[5] Many of the references to full Moon throughout this discussion could as well refer to new Moon; though, of course, lunar eclipses occur only at full Moon.

[6] All results are remarkably near the round value 5 1/4, which suggests that all four results were forced to come out very close to Ptolemy's lunar epicycle radius, 5;15.

[7] This huge sensitivity to mere quarter-hour uncertainties is lethal to Ptolemy's pretended precision here, since much of the raw eclipse-time data are obviously good to only about 1$^{h}$ precision. See David Dicks at *DIO 4.1* ‡1 §D1 & fn 46.



**E2** From an observation (S5.3) allegedly performed on 139/02/09 (§D) Ptolemy made this difference 7°2/3 at half-Moon. However, in his calculations he said that the parallax was negligible. It was in fact −9′ on his own parallax theory. He should thus have found a difference of 7°31′, not 7°40′.

**E3** Ptolemy also quoted (S5.3) an observation by Hipparchus on −127/08/05 from which he again calculated the difference to be 7°2/3, once more neglecting parallax. But parallax was again −9′, with an effect in the opposite direction from §E2, so Ptolemy should have found 7°49′.

**E4** The two observations disagreed, but Ptolemy fudged his calculations to make them agree [⊙9].

## F The Final Theory for the Moon (C149 to 156)

**F1** In Ptolemy's final theory of the motion of the Moon (see Fig.VII.2 at C150), the mean Moon M moves round the Earth E at a constant speed. M maintains a constant distance not from E but from a point $C_1$ which moves round E at a constant distance at the same angular rate (as M) — but in reverse. The rate (and starting point) is chosen so that $C_1$ lies directly between E and M at full[8] Moon, but lies on the opposite side of E at either half-Moon. So the distance from the Earth to the Moon is $MC_1 + C_1E$ at mean syzygy, but $MC_1 - C_1E$ at half-Moon. This gives the lunar epicycle a larger apparent size at half-Moon and so increases the maximum angle between the mean Moon (M) and the Moon. The value of $C_1E$ is chosen to give the "right" value at half-Moon, namely, the 7°2/3 value of §E. It turns out [⊙10] that $C_1E$ has to be 10;19 on a scale on which the maximum distance is 60.

**F2** The Moon moves (circling retrograde around M) on its epicycle at a constant speed not relative to the diameter through E but relative to a point K on a slightly different diameter. Following C150's Fig.VII.2, we specify $C_2$ as the point where this diameter when prolonged reaches the straight line through $C_1$ and E.

**F3** Ptolemy calculated the distance $C_2E$ from an observation of the Sun and Moon on −126/05/02 (S5.5) as follows. From the time of the observation he first calculated (from his S4.4 tables) the longitude of M. From this and the observed longitude of the Moon he then found the position of the Moon on the epicycle. From the time of the observation he also computed the distance the Moon has moved round on the epicycle from its zero-point K, and so he found K by simple subtraction and could then compute $C_2E$. He found it to be 10;18, equal to $C_1E$ to a high degree of precision.

**F4** From modern calculation, Newton found that, at the time of the −126/05/02 observation, the angle between M and the Moon was [⊙11] about 1°23′, not the 46′ that Ptolemy found (C156) by comparing the observed Moon to a value for M obtained from his tables. If used (in place of 46′), 1°23′ would have made $C_2E$ equal 14;40, not 10;18.

**F5** From an observation by Hipparchus on −126/07/07, Ptolemy computed $C_2E$ = 10;20 (also in S5.5). He achieved this almost perfect agreement (with $C_1E$ = 10;19) again by having a wrong value for the angle between M and the Moon (C156): 1°26′, instead of the actual angle at this time, 2°19′.

**F6** There can be no doubt that Ptolemy decided in advance to make $C_1E$ and $C_2E$ equal, and fudged the calculations or the observations to give this result. He did something very similar[9] in his treatment of Mercury and again in his treatment of Venus.

[8] At full Moon, $C_1$ will lie directly between E and M on the line SEM where S is the mean Sun. Same at mean new Moon, except that the line of mean syzygy is then SME.

[9] See below at §§P2 and Q2.

## G The Inclination of the Moon's Orbit (C184)

**G1** To find the angle between the Moon's orbit and the ecliptic (the inclination) Ptolemy measured the zenith distance[10] of the Moon when its longitude was near 90°, its ascending node was near the vernal equinox, and the Moon was crossing the meridian. He claimed (S5.12) that repeated measures of the zenith distance always found about 2° 1/8. Taking this as 2°07′, the obliquity of the ecliptic as 23°51′ (it was actually 23°41′) and the latitude of Alexandria as 30°58′N (it is 31°.2 N; Newton takes it to be 31°13′N), he found the inclination to be:

$$30°58' - 23°51' - 2°07'$$

That is, 5° exactly.

**G2** With correct values for the obliquity, latitude, and inclination (which actually varies between 5°.0 and 5°.3), Newton found that the zenith distance (for all the possible times when Ptolemy could have measured it) was never outside the range 2° 1/4 to 2° 1/2 instead of "always" near 2° 1/8.

## H The Distance of the Moon at the Quarters (C186 to C190)

**H1** Ptolemy's theory of the Moon's motion makes the mean Moon much closer to the Earth at the time of half-Moon (one quarter or three quarters of the way through the month) than at full or new Moon. If the mean Moon's distance (ME) is 60 at full Moon, then it is 39;22 (§F1) at the time of half-Moon. The radius of the epicycle is 5;15 (§C3). So the distance of the full Moon from the Earth can be as much as 65;15, and the distance of the half-Moon can be as little as 34;07. This is much too big a variation. Anyone who watches the Moon will know that its apparent size does not vary correspondingly.[11] (Even the notorious "Moon illusion" — which Newton does not mention — could only suggest that the distance is less when the Moon is near the horizon, not when it is half-full.)

**H2** Ptolemy estimated the distance of the half-Moon by measuring the zenith distance at sunset on 135/10/01. He says (S5.13) that he observed[12] the zenith distance to be 50°55′. But Newton found the correct value to be 50°14′. For the instruments which Ptolemy said that he was using, the error is unacceptably large.[13]

[10]The "zenith distance" is a celestial object's angular distance from the zenith; it is thus the compliment of that object's altitude above the true horizon.

[11] I.e., the Ptolemaic lunar theory implied that an Earth observer will see the Moon's angular diameter vary by a factor of nearly 2. Contra some Higher Opinion (see irony at C182f and *DIO 1.3* fn 284), Ptolemy actually believed in this impossibly counter-eyeball distance-variation — so much so that he arranged an equally impossible "observation" (§§H2-H3) to prove it. The Moon's actual distance-extremes (including equation of centre, all perturbations, and observer-nongeocentricity) are in a ratio of less than 7 to 6.

[12]Celestial coordinates based upon the observer's location (called "topocentric" coordinates) naturally differ from those based on the Earth's center ("geocentric" coordinates). (Only the latter were tabulated, but only the former can be observed.) Parallax is the difference between these coordinates. For the Moon, parallax is frequently as much as about a degree.

[13]If one takes Alexandria's latitude as 31°12′N, the zenith distance is 50°13′, so the discrepancy is 42′. This error is more than 2 1/2 times the semi-diameter (under 16′) of the actual body Ptolemy was allegedly measuring. And Ptolemy's purported instrument was a framing device (see C182 Fig.VIII.4), which would naturally use so snug a viewing frame that (as pointed out at C183 and C188-191) if he had actually used it: [a] the lack of serious lunar distance-variation would have been immediately obvious; and [b] the Moon would be *entirely outside* the frame during the 135/10/01 observation if Ptolemy pointed his instrument at the zenith distance he reports. [See also ⊙2.]



**H3** From his tables[14] Ptolemy calculated that the zenith distance as seen from the centre of the Earth was 49°48′, giving up a parallax of 1°07′ (50°55′− 49°48′) and a distance[15] of 39 3/4 times[16] the radius of the Earth. The correct distance at that date was over 60.[17] Clearly Ptolemy fabricated the observation to support his theory of the Moon's motion.

**H4** Ptolemy also calculated the distance of the Moon at this time, from his theory, to be 40;25 on a scale on which the greatest distance of the mean Moon was 60. This enabled him to calculate that the greatest, mean, and least distances of the full Moon were respectively 64 1/6, 59, and 53 5/6 times the radius of the Earth.[18]

## I The Distance of the Sun (C174, 194 to 199, and 202 to 203)

**I1** Ptolemy explained (S5.15) a geometrical method[19] for finding the distance of the Sun which is equivalent, in modern terms, to saying that the sum of the apparent radius of the Sun and the apparent radius of the shadow of the Earth on the Moon is equal to the sum of the parallax of the Sun and the parallax of the Moon.

**I2** He found the apparent radius of the Moon when it is at its greatest distance and the apparent radius of the shadow then from the magnitudes of partial eclipses on −522/07/16 and −620/04/22 and the calculated latitude of the Moon in the middle of each eclipse. (To calculate the time of the middle of the second eclipse from the observed beginning he used a semi-duration of an hour, though in S6.5 he used a semi-duration of 1/2 hour for an eclipse of the same stated magnitude.)[20] He found the apparent radius of the Moon to be 15′40″ and took this to be also the apparent radius of the Sun. The apparent radius of the shadow was 40′40″.

**I3** The parallax of the Moon (its greatest distance on Ptolemy's lunar theory being 64 1/6 times the radius of the Earth: §H4) was 53′35″, so the parallax of the Sun was 15′40″+ 40′40″− 53′35″, which amounts to 2′45″, giving[21] a distance 1250 times the radius of the Earth. Ptolemy obtained 1210, using a complicated geometrical calculation. The difference is inconsequential.

**I4** But in another chapter (S6.5) Ptolemy found the apparent radii when the Moon is at the least distance a full Moon can occur (which on his theory is 53 5/6 times the radius

[14]Ptolemy already had figures for latitude, obliquity, and lunar $i$ (all three in serious error); thus, simple arithmetic could give 30°58′+ 23°51′− 5° = 49°49′. (Correct figures would have given instead: 31°12′+ 23°41′− 5°18′ = 49°35′.) After Ptolemy's tiny corrections (1′ net, though 5′ would have been more nearly correct) for the slight imperfections of the nodal and solstitial idealities, he found 49°48′. (He should have gotten 49°30′.)

[15] Ptolemy makes it 39 3/4 Earth-radii, while Newton more precisely computes 39 5/6. (One may easily check the numbers by the law of sines: sin 50°55′/sin 1°07′ = 39 5/6 Earth-radii geocentric. So 1°07′ of non-horizontal parallax corresponded to a horizontal parallax of 1°26′ or 86′. (The math should have been: sin 50°14′/sin 0°44′ = 60 Earth-radii geocentric. Actual horizontal parallax at the time: 57′. Both figures happen to be extremely average values for the Moon.)

[16] This figure is geocentric, and it is in quite close accord with Ptolemy's final lunar theory, developed above. See §H1 and fn 11.

[17]Topocentric: a little under 60.

[18] Using 39;50 (fn 15) and 40;25 (§H4), one may easily confirm: 60·(39;50/40;25) = 59 Earth-radii. (See S5.13 and Gerald Toomer, *Ptolemy's Almagest* 1984 page 251 note 49.) Multiplying 59/60 times ± 5;15/60 yields ±5 1/6. Adding this to 59, one gets (in Earth-radii) 64 1/6 or 53 5/6.

[19]Easily understood from C174's Fig.VIII.2.

[20] The S6.5 eclipse Newton refers to is that of −140/01/27. See C194 note and Toomer *op cit* page 253 note 56 and page 284 note 23. Ptolemy gives the magnitude for both eclipses (−620 and −140) as 3 digits (or 1/4 of the lunar diameter), though the magnitude of the −620 eclipse was actually 2 digits. (It is 3 digits by Ptolemy's tables.) Its actual semi-duration was 47 timeminutes. The −140/01/27 eclipse was in fact 3 digits and about an hour's semi-duration, though Ptolemy uses a half-hour: §I4.

[21]The cosecant of 2′45″ is 1250.

of the Earth: §H4) from[22] eclipses in −173/04/30 and (fn 20) −140/01/27. (To calculate the time of the middle of the second eclipse he took the semi-duration to be a half-hour,[23] though his table correctly gives a semi-duration of an hour for an eclipse of this magnitude.) Ptolemy did not use these radii to find the distance of the Sun. Had he done so he would have found that the sum of the apparent radius of the Sun and the apparent radius of the Earth's shadow was less than the parallax[24] of the Moon at the given least distance.[25] This is, to second Newton's phrase, "physical nonsense" (C203).

**I5** Such an outcome only dramatizes what was already obvious from the delicate mathematics of §I3: this method of finding the distance of the Sun is so sensitive[26] to the exact values of angles which cannot be measured or calculated precisely that it will not — except by coincidence or fabrication — give a correct result. To obtain a result so far from the truth (the mean distance to the Sun is about 23,500 times the radius of the Earth),[27] but so close to a result previously obtained by Aristarchus and suspiciously close to the value that Ptolemy needed for his later and totally erroneous theory of the way in which the orbits of the celestial bodies fit together (*Hypotheseis ton planomenon*) would require an unacceptable coincidence. It must have been obtained by fabrication.

## J Positions of Stars (C211 to 212)

In S7.1 Ptolemy quoted some configurations of stars described by Hipparchus in his commentary on Aratus's *Phaenomena*. Newton investigated one. Hipparchus stated that $\beta$ Cancri (to give it the modern name) was 1 1/2 digits north-east of the centre of the line joining $\alpha$ Cancri and Procyon. Ptolemy said that he found the same result. He couldn't have done so. At this date $\beta$ Cancri was very close to the midpoint and north-*west* of it.

## K The Longitudes of Regulus and Spica (C217 to 218)

**K1** Ptolemy (S7.2) used an observation of the Moon on 139/02/23 (§§D & E2), which Newton showed to be fudged (C145 to 146 and 148 to 149), to find the longitude of Regulus. The result was exactly $2^\circ 2/3$ greater than the longitude that Hipparchus had measured 2 2/3 centuries earlier, giving a rate of precession equal to $1^\circ$ per century. This is consistent with Hipparchus's statement (S7.2) that precession is at least $1^\circ$ per century. (It was actually $1^\circ 23'$ per century.) So the longitude of Regulus in the catalogue was fudged.

**K2** If we add $2^\circ 2/3$ to Hipparchus's value for the longitude of Spica we get Ptolemy's value, namely $176^\circ 2/3$. The error is $-1^\circ 17'$. So this longitude was also fudged.

## L Declinations (C220 to 225)

**L1** In S7.3 Ptolemy listed the declinations of 18 stars measured by Timocharis or Aristyllus, by Hipparchus, and by Ptolemy himself [⊙14]. He chose six of the stars, found the change in declination between Hipparchus's time and his own for each of them, and showed that this was consistent with a precession of $2^\circ 2/3$ in the intervening 2 2/3 centuries.

[22] In fact, the −173 eclipse was not near perigee.

[23] See fn 20.

[24] At C203, Newton uses Ptolemy's own value (§H4) for the least distance of the Moon to compute lunar parallax = arccsc(53 5/6) = $63'52''$.

[25] Newton does the calculation (using the equation of §I1) at C203. Using shadow radius $46'$ (S6.5), lunar semi-diameter $15'40''$ (§I2), and lunar parallax $63'52''$ (fn 24), he has:
solar parallax = $46' + 15'40'' - 63'52'' = -2'12'$.

[26] See N. Swerdlow *Centaurus 14*:287 (1969).

[27] The precise mean distance to the Sun is 23454.8 times the equatorial radius of the Earth.



**L2** Ptolemy could have used up to 14 of the 18 stars. (The other four are too close to a solstice.) Accurate calculation of the rate of precession that would give the changes in declination in the six stars that Ptolemy used varied from $35''.1$ to $41''.1$ per year. The other eight gave values ranging from $45''.1$ to $64''.4$ — much better. (The correct value was $49''.8$ per year.) Pannekoek suggested that the observations were all genuine and Ptolemy had simply picked six which agreed with his (erroneous) $36''$ per year[28] value for the precession (*Vistas in Astronomy* volume 1 [1955] pages 60 to 66). But if he had, the standard deviation of the results from the stars used would have been larger than that from the others; in fact it is smaller, as it would be if they were fudged.

## M Occultations (C225 to 237)

In S7.3 Ptolemy discussed seven cases of occultation of a star by the Moon. For each case, he calculated the position of the Moon at the time, and thereby deduced the latitude and longitude of the star.

**M1** On −282/01/29 the Moon's northern half covered the eastern part of the Pleiades. On 92/11/29 the southern cusp of the Moon covered the southeastern part of the Pleiades. However (C223) on Ptolemy's figures for the Moon (longitude $33^\circ 15'$, latitude $4^\circ$),[29] it could not in fact have covered any of the Pleiades as listed by Ptolemy in his catalogue of stars.

**M2** On −293/03/09 the Moon's rim reached Spica, which was reported as $5'$ north of the centre of the Moon. Ptolemy ignored[30] this $5'$ when computing the latitude of Spica from the data. On −282/11/09 Spica exactly touched the northern rim of the rising[31] Moon. On 98/01/11 Spica was hidden by the Moon.

**M3** On −294/12/21 the Moon's northern cusp touched [⊙15] $\beta$ Scorpii. On 98/01/14 the southern cusp was in a straight line with $\delta$ Scorpii and $\pi$ Scorpii and as far from $\delta$ as $\delta$ was from $\pi$, and it covered $\beta$ Scorpii. However, if the Moon was either where Ptolemy placed it, or where it really was according to modern calculation, it could[32] not have covered $\beta$ Scorpii. (See Fig.IX.7 at C234.)

**M4** Ptolemy used the change in longitude between two (or three) observations of the same star to confirm his erroneous value for the precession (§L2). If we compute the longitude of each star at the relevant date from the longitude in the catalogue and Ptolemy's erroneous precession, and round off to the nearest $5'$, we get exactly the longitude that Ptolemy "found" from the observation in all seven cases. If Ptolemy had computed the longitudes in this way they were bound to confirm his value for the precession. Moreover all the latitudes also agree to within $2'$ (see Table IX.3 at C230) with the latitudes in the

[28] Same as the $1^\circ$/century value we recall from §K1.

[29] S7.3 has true position longitude $33^\circ 15'$, latitude $4^\circ$. But the calculation is riddled with problems, well analysed by Toomer *op cit* page 335 note 70.

[30] At page 112 of John Britton's 1967 Yale dissertation (fortunately published at last in 1992), Britton correctly notes that in fact the Moon's centre went right over Spica. So Ptolemy's ignoring the $5'$ turned out to be right. But, ironically, had he included the $5'$, he would have gotten Spica's latitude ($-1^\circ 54'$, vs. his value of $-2^\circ$) correct to $1'$ by accidental cancellation of errors.

[31] The waning crescent Moon's north cusp passed merely $2'$ south of Spica; however, this occurred not at the rising of the Moon but about $0^h.8$ later. Toomer *op cit* page 336 note 75 rightly points out that Ptolemy's conversion of $3^h 1/2$ seasonal hours into equinoctial hours confuses day and night and thereby found $3^h 1/8$ when he should have gotten $3^h 7/8$. C236 adds (correctly) that for the time Ptolemy states (2:30 Alexandria Apparent Time), the Moon and Spica were actually both below the horizon. (Also true by his own tables.)

[32] Anyone who simply looks at Scorpio can see that $\beta$ Scorpii is too far from the $\delta$-$\pi$ line for the Moon to touch both. Ancient readers could have realized this, without modern checks or any other special knowledge. [See ⊙16.]

catalogue (if we agree with Newton's suggestion (C228) that the 3°1/3 for the eastern star in the Pleiades was a scribal error for 3°2/3).[33]

**M5** The amazingly exact agreement of the observations[34] with the catalogue would be impossible if they were genuine: there are too many sources of error. When a star is hidden by the Moon, it can be anywhere behind the Moon. The times are given only to the nearest half-hour [⊙16], so even accurately observed events could be up to a quarter of an hour out, during which time the Moon moves about 8′. The errors in Ptolemy's tables for the longitude of the Moon have a standard deviation of 35′ (C238).

## N The Catalogue of Stars (C237 to 256)

**N1** S7.5 and 8.1 consist of a catalogue of slightly over a thousand stars, giving their longitudes and latitudes, which Ptolemy (S7.4) claimed to have based upon his own observations. Newton suggested that Ptolemy did not do this, but instead updated a catalogue compiled at the time of Hipparchus by adding the difference in longitude caused by precession. Ptolemy's erroneous value for precession (§K1) would make the typical updated longitude more than 1° too low.[35] This effect could account for the notorious systematic error that does in fact exist in the longitudes.

**N2** There is evidence that the catalogue was compiled at the latitude of Rhodes, where Hipparchus worked.[36] Every star listed was visible at Rhodes. Newton remarked[37] that many stars always below the horizon at Rhodes were visible at Alexandria (which is about 5° further south) but were not included in the catalogue. He did not name any, but Dennis Rawlins has filled the gap (Table II at page 364 of reference in §B). They include $\epsilon$ Carinae, $\lambda$ Centauri, $\alpha$ Gruis, $\alpha$ Indi, and $\alpha$ Phoenicis.

**N3** The fractional parts of the latitudes and longitudes are not distributed at random. In particular far more latitudes are whole numbers of degrees and far more longitudes end in 2/3° than would be expected. Newton produced a powerful argument that this implied that Ptolemy compiled his catalogue by adding 2/3° to the longitudes of an earlier catalogue. However, Shevchenko discovered that the dominance of 2/3° endings did not[38] occur in the southern constellations (*Journal for the History of Astronomy*, volume 21, pages 187 to 201). Therefore the figures that I give in Table 2 are for the northern stars not the figures for the whole catalogue which Newton gave. Consequently, Newton's reasoning applies to the northern stars but not to the catalogue as a whole.

**N4** Newton suggested that the instrument used was an armillary astrolabon graduated in whole degrees (perhaps in half-degrees),[39] and that the observer estimated the fractions for those observations that did not fall on a graduation. The large numbers of whole degrees in the latitudes would be accounted for if the eye were (C247) "attracted, so to speak, to the degree mark" and assigned more measurements to it than to invisible marks.

[33] See Toomer *op cit* page 335 note 71 and page 363 note 188. The star in question is #411 in the catalogue, evidently 27f Tauri (Atlas).

[34] The observations were supposedly performed independently via armillary astrolabon. See S7.4 and S5.1. For Newton's estimate of the mean longitude error (22′) of this instrument, see C216. Ulugh Beg's 15th century use of the same sort of instrument achieved similar accuracy.

[35] An error in precession of −23′ per century (§K1) will, in 2 2/3 centuries, grow to more than −1°.

[36] See ‡4 fn 12 for a full array of proofs that Hipparchus was the observer of the star catalogue.

[37] Delambre first pointed this out in his *Histoire de l'astronomie ancienne*, 1817, volume 2 page 284.

[38] Meaning that, in the southern constellations, the stars with 2/3 endings are outnumbered by the zeros. However, southern stars with 1/6 endings vastly outnumber those with 1/2 endings (in accord with the Newton distribution). This seeming dissonance is resolved at *DIO 4.1* ‡3, starting with the point emphasized at the conclusion of its fn 5.

[39] See *DIO 2.3* ‡8 §C8.



Table 2: Distribution of the 359 Northern Stars' Fractional Endings

| Coordinates | integral | 1/6° | 1/4° | 1/3° | 1/2° | 2/3° | 3/4° | 5/6° |
|---|---|---|---|---|---|---|---|---|
| Latitudes | 108 | 29 | 33 | 39 | 75 | 36 | 10 | 29 |
| Longitudes | 62 | 61 | 0 | 67 | 29 | 95 | 0 | 45 |
| See §N5 | 72 | 75 | 0 | 46 | 29 | 108 | 0 | 29 |

First column of data ("integral") is number of endings for whole degrees.

Table 3: Distribution of 359 Random Fractional Endings

| integral | 1/6° | 1/4° | 1/3° | 1/2° | 2/3° | 3/4° | 5/6° |
|---|---|---|---|---|---|---|---|
| 60 | 45 | 30 | 45 | 60 | 45 | 30 | 45 |

**N5** In more detail: because the fractions that appear are not evenly spaced we would not expect[40] the fractions to appear equally often if the odd fractions were distributed at random and allotted correctly to the nearest fraction that does occur. In fact, we would expect the 359 fractions for the latitudes to be distributed as in Table 3 [where the sum is 360 only because of rounding]. If we add 2°2/3 to each of these fractions (and drop the integral part of the result) we obtain: 2/3°, 5/6°, 11/12°, zero, 1/6°, 1/3°, 5/12°, 1/2°. Newton said (C250) that the "rules of the catalogue"[41] did not allow 11/12° or 5/12°. He did not say why, but (C252) if we add the 11/12° stars to the zeros and the 5/12° stars to the 1/3° stars, and apply this procedure to the distribution of latitudes in Table 2, we get the distribution in the bottom row of Table 2. It is similar[42] to the actual distribution for the longitudes.

**N6** Newton's suggestion explains not only the distribution of the fractions and the systematic error of −1°.1, but also the otherwise-puzzling absence of 1/4° and 3/4° endings in the longitudes.

**N7** N.B. Rawlins has used similar reasoning (*DIO 4.1* ‡3 §§D-E) to show that the coordinates of the stars in the southern sky were found, not with an armillary astrolabon, but with an instrument that measured zenith distances.[43]

## O Conjunctions (C262 to 265)

Ptolemy recorded a near-conjunction of each planet with the Moon and a star.

**O1** On 139/05/17, 4 1/2 hours before midnight (S9.10) the longitude of Mercury (found by comparing it with Regulus) was 77°1/2. Mercury was 1°1/6 east of the centre of the Moon, giving the Moon a longitude of 76°1/3. Its longitude calculated from Ptolemy's tables is 76°1/3.

**O2** On 138/12/16, 4 3/4 hours after midnight (S10.4) the longitude of Venus (found by comparing Venus with Spica) was 216°1/2. Venus was on the straight line joining $\beta$ Scorpii (whose longitude in the star catalogue is 216°1/3) to the centre of the Moon

[40] See C247 or *DIO 4.1* ‡3 §B4.

[41] See C250. Newton did not explain, but the implicit question is (*DIO 4.1* ‡3 §C1): who'd not look askance at a set of 359 stars containing a great many with 5/12 or 11/12 endings, but none with 1/12, 1/4, 7/12, or 3/4 endings? That 2/3 (or 1/6) had been added to all endings would be obvious and so would reveal the method of appropriation.

[42] A $\chi^2$ test (*DIO 4.1* ‡3 §C4) upon the last two rows of Table 2 shows that the discrepancies are not statistically significant.

[43] See ‡4 §A23.

(whose calculated longitude was 216°3/4) and 1 1/2 times as far from the Moon as from $\beta$ Scorpii. This gives Venus a longitude of 216°1/2, exactly as observed.

**O3** On 139/05/30, 3 hours before midnight (S10.8) the longitude of Mars (found by comparing Mars with Spica) was 241°3/5. Mars was 1°3/5 east of the Moon, whose calculated longitude was 240°, giving Mars a longitude of 241°3/5, exactly as observed.

**O4** On 139/07/11, 5 hours after midnight (S11.2) the longitude of Jupiter (found by comparing Jupiter with Aldebaran) was 75°3/4. Jupiter was directly north of the centre of the Moon and so had the same longitude. The calculated[44] longitude of the Moon was also 75°3/4, exactly as observed.

**O5** On 138/12/22, 4 hours before midnight (S11.6) the longitude of Saturn (found by comparing Saturn with Aldebaran) was 309°04′. Saturn was 1/2° east of the northern tip of the crescent Moon. The calculated longitude of the Moon (neglecting the equation of time and using a parallax[45] mistaken by 8′) was 308°34′, giving Saturn a longitude exactly (to the nearest minute!) as observed.

**O6** The agreements between the observed and calculated[46] longitudes could not have happened by chance.

## P Mercury (C287)

**P1** Ptolemy calculated the apogee of Mercury in his own time twice and — also twice — the apogee 400 years earlier. He concluded [⊙17] that it had increased by 4°. If we calculate from Ptolemy's data to one decimal place in degrees and add 4° to the earlier values, we obtain 189°.9, 190°.3, 189°.8, 190°.0. The correct value during the 4 century period under consideration was never less than 220°. From genuine observations it is not possible to obtain values so close together and so far from the truth.

**P2** In Ptolemy's theory of the motion of Mercury the centre of the epicycle revolves at a constant distance from a point F — which itself revolves (retrograde) at a constant distance round a fixed point D — and at a constant rate round a point Z situated between D and the Earth E. Ptolemy needed the values of the three small distances, EZ, ZD, and DF (see C260 Fig.X.3), which Newton called *eccentricities*. Ptolemy calculated them from his observations and made them all equal. Newton calculated the values they should have had, i.e., the values that made Ptolemy's mechanism fit reality most closely, and found them to be far from equal.[47]

## Q Venus (C313)

**Q1** Ptolemy found the apogee of Venus twice, and the two values were only 2′ apart, although they were in error by about 4°. Newton commented "the probability that Ptolemy's agreement could have happened by chance is so tiny that we do not need to estimate it."

[44]C263-264 suggests Ptolemy here made errors in the calculation of the equation of time and of parallax when computing the Moon's true and apparent longitude. But in fact Ptolemy's figures here agree well (±1′) with accurate calculations from his tables.

[45]Parallax used at S11.6 = −66′; actual parallax = −58′; computed from Ptolemy's S2.13 Lower Egypt parallax tables = −74′. Britton (*op cit* pp.140-141) was the first to reveal that, at the time of this observation, Saturn was actually behind the Moon, "thus, like the previous observation [§O2], the circumstances which Ptolemy describes could not have been observed at the time which he reports." [S11.6: "Saturn sighted with respect to" Aldebaran.] So, a skeptic might wonder whether the original (pre-manipulation) version of this record was just that of a plain occultation.

[46] From lunar tables with mean error 35′. See §M5. [And ⊙16.]

[47]See C278 Table X.2.



**Q2** Ptolemy's theory of motion for Venus needs two small eccentricities: the distance from the Earth to the equant and the distance from the equant to the centre of the deferent. He calculated them from observations and made them out to be equal. This is important because Ptolemy assumed that this equality applies to all the remaining planets and for each of them took it to be part of the theory of motion. They should not be equal, as Newton found (see C311, Table XI.2) by calculating the values which fit reality most closely. (And, I might add, as Kepler had earlier found.)

## R Epicyclic Anomaly (C320 to 321)

**R1** For each of the planets Ptolemy calculated the epicyclic anomaly from an old and a new observation; and from the change in anomaly and the interval between the two dates he calculated the change in anomaly per day. This is done by dividing the total change in anomaly by the number of days in the interval. But the degrees per day that Ptolemy quoted and tabulated (S9.4) do not agree with the values obtained by division. For example, for Jupiter, division gives[48] (in sexagesimals)

0;54,09,02,45,08,57

whereas Ptolemy quoted[49]

0;54,09,02,46,26,00

The difference is tiny, but we are not talking about accuracy; we are talking about whether a division is right or wrong. It is wrong.[50]

**R2** Newton suggested (C325 to 327) that for each of the planets Ptolemy calculated[51] the anomaly at the later observation from the anomaly at the earlier observation and the rate that he quoted. Rounding would account for the small discrepancy. Ptolemy neglected to calculate from the "observation" he quoted to verify whether it did give the change per day that he quoted.

## S Final Remark

**S1** To my mind, the most remarkable thing about the whole affair is not Newton's intemperate language, arising no doubt from frustration at not being able to use data from the *Syntaxis* in his own researches, coupled with disdainful treatment by the historical (not astronomical) establishment. Nor is opposition to Newton particularly surprising; the establishment often digs in its heels and puts on blinkers when a radical and ingenious proposal is set forth. No: the remarkable thing is that Delambre's devastating and irrefutable proof that Ptolemy lied about his "observations" of the equinoxes and solstice was ignored for so long.

**S2** For that matter, Christian Severin's similar accusation, without the calculations, was ignored or disbelieved for much longer. In 1639, in *Introductio in Theatrum Astronomicum*, L i f 33, he wrote: *Non tantum erasse ilium dixit observando sed plane finxisse observatum quod ex Hipparcho computaverit*.

[48]A result accurate to the last place is given by Toomer (*op cit* p.669): 0;54,09,02,45,08,48.

[49]The null sixth sexagesimal place is absent in Ptolemy's first rendition, the *Canobic Inscription*. See *DIO 2.1* ‡3 fn 23.

[50] The sources of Ptolemy's tabular planet speeds, out to full 6-sexagesimal-place precision were 1st revealed by Rawlins [⊙18] & A.Jones (*DIO 11.2* p.30 & ‡4 eqs.31&45).

[51]This would be the same procedure used to fabricate the four solar observations [⊙2]. Again, Ptolemy computed data from theory instead of theory from data. At the very least, one can protest that this is mathematics, not science. And see *DIO 1.2* fn 99 for what establishment opinion thought of such behavior — before it became undeniable that Ptolemy had engaged in it.

# ⊙ Notes by DR

**⊙1** [Note to §A2.] The remarkably large latenesses of Ptolemy's alleged observations average more than a day. (See Table 1.) The errors for all 4 of Ptolemy's claimed solar observations: $+33^{\rm h}$ (132 AE), $+33^{\rm h}$ (139 AE), $+21^{\rm h}$ (139 VE), $+36^{\rm h}$ (140 SS). Notice that the observations' errors exhibit close (but not perfect: ⊙2) fidelity to both the mean and periodic errors of his adopted Hipparchan solar theory. (See similar stellar effect at ⊙8.) Ptolemy's solar tables are believed to be those of Hipparchus because the tables' mean errors are huge, $-1^\circ.1$ for his own era, though nearly null for Hipparchus's time. (This is true not only for the Sun, but for the Moon, all five planets, and the stars.) Because of the accumulated effect of the incorrect length of the Hipparchus year (§A1) on which the solar tables were based, these tables' error increased by $6^{\rm m}$/year (*idem*), or $1^{\rm h}$/decade. So, in the 26 decades between Hipparchus and Ptolemy, the mean error grew to about $26^{\rm h}$ (over a day late), which represents $-1^\circ.1$ of mean solar motion — and that is just the mean error of Ptolemy's solar tables. Notice, too, that there is a periodic error (also from Hipparchus) which has the considerable amplitude of $0^{\rm d}.4$. Its effect, too, is faithfully reflected in Ptolemy's "observations", which is why, though the different observations average over a day of lateness, predictably (akin to Gingerich's finding: *DIO 1.3* fn 223) theory-responsive errors occur on either side of that mean. For Ptolemy's epoch (137.547), the tables' error in time $t$ or in Ptolemy's solar longitude $\lambda$ can be expressed as:
$\Delta t = +26^{\rm h}1/4 + (10^{\rm h}3/4)\sin(\lambda - 38^\circ)$ in hrs; $\Delta\lambda = -65' - 25'\sin(\lambda - 44^\circ)$ in arcmin.

**⊙2** [Note to §A3.] You can use the AD 139 equinox as an example of how Ptolemy fabricated all his solar observations by simple arithmetic: since there are 285 years from −146 to 139, just multiply 285 times Hipparchus's year (§A1), and add the product to the Hipparchus observed autumn equinox, −146/09/27 $00^{\rm h}$. As an equation, this is (⊙3):

$$-146/09/27\ 00^{\rm h} + 285\cdot(365^{\rm d}1/4 - 1/300) = +139/09/26\ 07^{\rm h}12^{\rm m} \qquad (1)$$

a result which is (if rounded to the nearest hour) just equal to Ptolemy's very erroneous "observed" autumn equinox. See Table 1. All the other "observed" equinoxes in this table may also be computed by the elementary arithmetical method of eq. 1. Note that Ptolemy did not even use the tables of the *Syntaxis* for his fabrications. (See *DIO 1.2* fn 166.) If one goes to that slight trouble, the results are discrepant (vs. his "observations") by $1^{\rm h}$ in some cases. So we know that Ptolemy used mere arithmetic for these fabrications. Note that the errors are so enormous that the "observed" and actual solar disks do not even touch, much less overlap. Likewise for the lunar "observation" discussed at §§H2-H3.

**⊙3** [Note to §A2, Table 1, & ⊙2.] All dates here are in the Julian calendar (yearlength = $365^{\rm d}1/4$). For solving eq. 1, there is an easy method (not Ptolemy's, since he used the Egyptian yearlength, $365^{\rm d}$): adding 285 Julian years to the Hipparchus −146/09/27 equinox yields the original time of the year plus a quarter day (since 285 is 1 mod 4); so, the intermediate result is: 139/09/27 $06^{\rm h}$. Adjusting for Hipparchus's yearlength, we need only alter the foregoing calculation by −285/300 days or $-22^{\rm h}48^{\rm m}$, which is $01^{\rm h}12^{\rm m}$ minus $1^{\rm d}$; so the fabricated autumn equinox obviously must be 139/09/26 $07^{\rm h}12^{\rm m}$.

**⊙4** [Note to §B.] On receiving an offprint of this paper, B. L. van der Waerden, author of a Springer mathematical statistics text, offered a prediction which has proven sadly prescient (1982/7/24 letter to DR): "I am afraid that most historians will not understand your mathematics and — even worse — not be willing to admit that you are right, but I assure you that your mathematics is OK and that your conclusions are sound and extremely interesting." This is the same analysis that Ptolemy's defenders are still seeking an escape from. See *News Notes*; also *DIO 2.3* ‡8 §C & J.Evans *History & Practice* . . . 1998 pp.268f.

**⊙5** [Note to §C2.] The S4.11 pair's implicit epicycle-radii are effectively computed by Newton (C122 Table VI.3) from his own and Ptolemy's reductions of the data. (Table VI.3 gives the equation of center $E$, but the epicycle radius is easily computed, since it is equal to $60\cdot\sin E$.) We here cite the latter calculations. (The notation here, common for scholars of ancient astronomy, says that the four measures of the epicycle's size were, in the specified units: 5+13/60, 5+14/60, 5+15/60, 5+15/60, respectively.)



**⊙6** [Note to §D.] Toomer *op cit* page 224 note 11 remarks that Ptolemy's language may indicate use of the analog-computer ecliptic-ring-shadow method for determination of the solar longitude via armillary astrolabon (the following assumes an accurately-fashioned instrument): the observer spins the armillary astrolabon equatorially, turning fused ecliptic rings #3 (ecliptic ring) & #4 (colure ring) in Rawlins 1982 Fig.1 (or Toomer *op cit* page 218 Fig.F) until the sunward part of ring #3 casts its shadow symmetrically upon the inward part of the opposite side of itself. He then freezes the equatorial motion and, within the clamped rings #3-#4, turns ring #2 (the latitude ring) until its shadow is cast upon itself; then the intersection of rings #2&#3 will automatically be at the graduation on ring #3 corresponding to the Sun's actual longitude. (Which ensures that all the instrument's rings are correctly oriented with respect to the actual sky.) We will call this the Fundamental Method. But S5.1 suggests another method, for locating non-solar objects: first set the reference-object ring (#5) upon the *tabular* solar position on ring #3 and then spin the rings #3-#4-#5 unit until the shadow of ring #5 is upon itself. (If the tabular solar position is accurate, all the instrument's rings are now oriented to reality, so the unit is clamped.) Ring #2 within is now quickly spun & used to locate the quarry object. We will call this the Tabular Method. Ptolemy's defenders prefer assuming his use of the Tabular Method for good reason: if the Fundamental Method would produce correct longitudes, then his claim that he used it (while consistently acquiring data which always agreed with his tables instead of reality) convicts him of fabrication. But it turns out that this defense doesn't hold, because Ptolemy states (S5.1) that the Tabular Method caused ring #3 to cast its shadow "exactly on itself" — just as in the Fundamental Method. But *whichever* of the two methods Ptolemy chose, he would typically find a huge conflict with the other, because his solar tables' average error was a *degree*. The fact that he was unaware of this flagrant and persistent contradiction raises a question regarding whether he ever actually used an armillary astrolabon. (See also ⊙9, *DIO 2.3* ‡8 C26[b], & *DIO 4.1* ‡3 fn 7. And see a similar problem at Rawlins 1982 page 367: Ptolemy's erroneous geographical latitude is incompatible with the Ancient Star Catalogue's data.)

**⊙7** [Note to §D.] J.Włodarczyk (*J. Hist. Astron. 18*:173; 1987) suggests that the 139/02/23 solar longitude $333^\circ1/20$ was calculated from Ptolemy's tables (which give $333^\circ04'$ for this time) — i.e., that he used the Tabular Method instead of the Fundamental Method. (Both described above in ⊙6.) Assuming that the observation was made right at sunset (with a full $34'1/2$ of mean horizontal refraction), the effect upon the Fundamental Method's measure of solar longitude would have been $+0^\circ.8$ — but the $333^\circ$ longitude's error was actually $-0^\circ.7$. This enormous discrepancy ($1^\circ1/2$) proves that the Fundamental Method was not used. Włodarczyk believes that Ptolemy instead merely set the reference ring (ring #5) on tabular longitude $333^\circ$. The above-noted $-0^\circ.7$ error of Ptolemy's solar tables, added to a $0^\circ.6$ astrolabon mis-aim in longitude caused by refraction of the Sun's light, could lead to the measured 139/02/23 longitudes of the Moon and Regulus being read as much as $1^\circ.3$ low. (Ptolemy's Regulus longitude was off by $-1^\circ.6$ in 139 AD.) But most of Ptolemy's defenders (Britton *op cit* p.144 provides a welcome exception) have overlooked Ptolemy's equally crucial *2nd* observation of 139/02/23 — the very one which actually locates Regulus, an observation *which can only be trivially affected* (c.$1'$) *by refraction*. Ptolemy reports (S7.2) that he observed Regulus $57^\circ1/6$ east of the Moon, whereas the actual difference (including refraction and parallax) at this time ($18^{\rm h}$ Alexandria Apparent Time) was $57^\circ.7$. This huge error (twice the lunar semi-diameter) was *required*, to get Regulus's longitude to come out "right", i.e., *exactly* $2^\circ2/3$ more than Hipparchus's Regulus longitude, which (unfortunately for Ptolemy) happened to be enormously wrong in the negative direction. So Ptolemy had to make a gross negative error to match Hipparchus's. (Thus, Regulus became Ptolemy's most negatively misplaced principal star. As with the solar errors remarked at ⊙1, Ptolemy mimics not only Hipparchus's mean errors but his fine errors.) Again, the consideration to be stressed here (which Włodarczyk *op cit* page 182 quotes, under the misimpression that he has undercut it) is Newton's at C254 (see also C218, 147-148): "the value [of Regulus's longitude] that Ptolemy obtains cannot be explained by experimental sources, no matter what their size. The crucial point is the exact agreement with preassigned values; and exact agreement, occurring time after time, cannot be the consequence of errors in measurement." (See ‡4 fn 11.)

**⊙8** [Note to §D.] The wording at S7.2 (like that at S5.3: ⊙6) implies use of the Fundamental Method (which cannot have happened: ⊙6). By the way, Newton is wrong in doubting (*QJRAS 20*:383 [1979] page 391 footnote) J.Dreyer's calculation of the effect of refraction upon the Fundamental Method. Newton accounts only for $\omega$, the effect on astrolabon spin-orientation, but neglects $\xi$, the direct effect of refraction upon longitude. If we call $\psi$ the angle (tabulated at S2.13) between the ecliptic and the vertical, and call $\eta$ the angle between the ecliptic and the circle of constant declination (where $\sin\eta = \sin\epsilon\cos\alpha$, for $\epsilon$ = obliquity and $\alpha$ = right ascension), then for refraction $r$, we have: $\omega = r\sin\psi/\tan\eta$ and $\xi = r\cos\psi$. Longitude error $\Delta\lambda = \omega + \xi$. But a much simpler equivalent expression can be formed by using $\theta$, the angle between the vertical and the circle of constant declination. (With $\cos\theta = cos\phi \sin HA/cosh$, where $\phi$ = geographical latitude, $HA$ = hour angle, and $h$ = altitude, we can find $\psi$ [useful for ecliptical parallax problems] by simply remembering that $\psi = \theta - \eta$.) Then we have just: $\Delta\lambda = r\sin\theta/\sin\eta$. (Which obviously breaks down near the solstices, where it is well-known that the Fundamental Method is inapplicable.) This equation is most clearly understood by seeing that $\Delta\lambda$ must be simply due to refraction's effect upon declination (as Dreyer said), this because the instrument is an analog computer of solar longitude (strictly) as a function of declination. Note: this computer will of course misread if the ecliptic ring is not tilted (with respect to the equator) by the correct obliquity. Ptolemy's $+11'$ obliquity error is therefore one more reason why one questions (also at ⊙6) whether he ever used an astrolabon. If his putative instrument's ecliptic ring #3 was actually tilted at his $\epsilon$ instead of the correct $23°40'.7$, he would surely have found that his solar longitudes obtained by the Fundamental Method were in error by large amounts — several *degrees* near the solstices.

**⊙9** [Note to §E4.] A remarkable feature of the perfect agreement Ptolemy finds is that the annual error in his (Hipparchus's) lunisolar theory is near its absolute maximum of $0°.2$ for both the S5.3 observations and thus for both of his seemingly precise calculations (from these data) that Moon-minus-M equalled exactly 7°2/3. Such considerations emphasize how rigged these calculations had to be in order that (even while degraded by $0°.2$ errors in parallax and in lunar elongation, among other effects), they nonetheless gave precisely the same answer down to the arcminute. (It is worth remarking that both the Hipparchus and Ptolemy half-Moon observations were taken at just the right time: in both cases, Moon-minus-M was actually very near maximum.)

**⊙10** [Note to §F1.] For ease and clarity of manipulation, we may set $MC_1 = R$ and $C_1E = \rho$. Ptolemy arbitrarily established a scale in which M-to-E's maximum (syzygial) distance $MC_1 + C_1E$ or $R + \rho$ is defined as 60 units. Then he simply solved for $\rho$ in the equation $(5;15)/(60 - 2\rho) = \sin 7°2/3$. The equation forces $\rho$ to equal 10;19. This in turn implies that $R$ must be $60 - \rho$ = 49;41, which makes M-to-E's minimal (half-Moon) distance equal to $R - \rho = 60 - 2\rho$ = 39;22.

**⊙11** [Note to §F4.] C156 reconstructs the actual angle Moon-minus-M as $1°23'$, but Newton says in a footnote that this modern calculation is rough, subject to an uncertainty of probably less than $10'$. DR finds $1°17'$, which is indeed within $10'$ of Newton's result. (For his calculation in §F5, Newton gets $2°19'$. DR finds likewise.) Note that in Hipparchus's time, tabular M virtually agreed with actual M. So, the error in Moon-minus-M should be effectively the same as the error of observation of the lunar elongation. That this is not true is due partly to the periodic error of the Hipparchus lunisolar theory and partly to errors introduced by Ptolemy's manipulation of the reductions of both S5.5 calculations. (See the large $0°.2$ discrepancy correctly computed by Toomer, *op cit* page 230 n.23.) Also, Hipparchus's observations of the elongation (angular distance of topocentric Moon from Sun) are consistently underestimated by a large amount. Were these errors heavily due to a misunderstanding of what was actually being observed by Hipparchus? (The hypothesis that follows suggests poor coordination between observers and mathematicians in Hipparchus's school. But we saw the same in Tycho's group at *DIO 3* §N. For a different sort of Hipparchus-circle disjunction, see *DIO 1.3* eqs.23&24.) If we include the −127/08/05 observation of S5.3 (§E3) with the S5.5 pair under study here, we have a nearly contemporaneous trio (the same trio whose solar positions are analysed in *DIO 1* ‡6, ‡9 §G10, and *DIO 6* ‡3 §D). The observational errors in Hipparchus's elongation measurements were: $-23'$ (−127/08/05), $-23'$ (−126/05/02), and $-26'$ (−126/07/07). (These figures are based upon *DIO* calculations, controlled here [and elsewhere in this paper] by close comparison to more exact values generated for *DIO* by Myles Standish of JPL, Cal Tech.) The fact that all these errors make the elongation much too *small* is provocative, especially since the effect existed regardless of which side of the Sun the Moon was on. A simple theory can begin to illuminate this odd circumstance: those using Moon-shots for navigation know that it is in general better to observe the Moon's limb than its centre. (The Moon was crescent for all 3 of the observations considered here.) If we assume Hipparchus's three elongation observations measured the angle between the solar centre and the lunar limb, we are left with errors of but $-8'$, $-6'$, and $-11'$, respectively, in absolute elongation. Analysis of his star catalogue has shown that, after the armillary astrolabon was oriented (⊙6) and clamped in place, Hipparchus took an average of $19^s$ to complete a stellar observation, during which time the Earth turned enough to affect a longitude reading by about $-4'$. (See *DIO 1.1* ‡6 §G4.) The effect would decrease measurements of absolute elongation for eastern elongation and increase it for western. Correcting the three residuals for this effect, they become $-12'$, $-10'$, $-7'$. With or without such correction, this is a remarkably steady data-set for antiquity. And it is possible that the observer (unwisely) used the Sun's limb instead of centre. If, on this speculative theory, we apply solar semi-diameter ($16'$), the residuals fall to merely $4'$, $6'$, $9'$ (mean error $6'$). Anyway, with or without solar-semi-diameter correction: residuals of ordmag $0°.1$ are more than satisfactory here. (The slight systematic bias could be due to a personal equation in nonsymmetric measurements. E.g., if the solar limb was the intended reference point, the observer may have aimed ring #5 in such a way as to just barely allow the Sun's limb to glimmer over the ring's plane.) The results' evident consistency here argues in favor of the probability that the $\Delta T$ (barely over $3^h$) used for these calculations is correct to within a very few timeminutes, since any error in $\Delta T$ will have an effect (about $1'$ per $2^m$ error in $\Delta T$) upon the 3rd residual (eastern elongation) virtually equal to that for the other two (both western), but opposite in sign.



**⊙12** [Note to §I4.] Taken literally, a negative $2'.2$ solar parallax (fn 25) would mean, since csc($2'.2$) = 1563, that the Sun's distance is about 1600 Earth radii but — in the opposite direction from which the Sun's light is arriving! Evidently, Ptolemy's cosmology was even more original and advanced than even his most adoring modern historian-promoters have hitherto perceived.

**⊙13** [Note to §K1.] Ptolemy's stated star catalogue epoch (S7.4) is 137/07/20 (137.547). (Though, see ⊙14.) The epoch of Hipparchus's star catalogue was about 2 2/3 centuries earlier. For indications that his exact chosen epoch was −127/9/24 1/2 (−126.278), see *DIO 1.1* ‡6 §§F and G4. The Hipparchus and Ptolemy longitudes of Regulus were 119°5/6 and 122°1/2, respectively. Errors for the given epochs: $-35'$ and $-1°33'$, respectively. Hipparchus's longitude of Spica: 174° (error $-19'$).

**⊙14** [Note to §L1.] The likely epochs of the several astronomers' star observations: Timocharis roughly 300 BC; Aristyllus c.260 BC (see p.263 of *Isis 73*:259 [1982], *DIO 4.1* ‡3 fn 40); Hipparchus c.130 BC (see ⊙13); for Ptolemy's declinations, the epoch indicated by the stellar data is AD 159±8 yrs (*DIO 4.1* ‡3 Table 3). As to whether Ptolemy made these accurate observations (which are not fabricated), see page 236 item (2) of D.Rawlins *Amer J Physics 55*:235 (1987) and n.17 of *Isis* ref (above in this note).

**⊙15** [Note to §M3.] Conjunction occurred about two hours later than the time Ptolemy accepts from Timocharis. The corresponding longitudinal discrepancy is huge: about $0°.8$ (more than triple the $15'$ lunar semi-diameter). And, at conjunction, the northernmost part of the Moon slipped past $\beta$ Scorpii at a distance of $11'$ to the south (more than 2/3 of the Moon's semi-diameter); so the reported contact never occurred at all.

**⊙16** [Note to §M5.] An $8'$ lunar motion in a half-hour is seemingly trivial, but it is nonetheless quadruple the size of the minuscule ($2'$) discrepancies implicitly claimed by Ptolemy (§M4). In fact, for all but one of the seven cases, the precision is an hour; thus, a $\pm 1^h/2$ error-range ($16'$ of motion) is built into six of the calculations. Note that (unlike Newton's point regarding the lunar tables' $35'$ mean error), this was *obvious to any careful reader from antiquity to the present*. (For other problems with Ptolemy's work that were discernable all along, see fn 32, *DIO 1.1* ‡7 §§A3-B1, *DIO 4.2* ‡6.) [See also *DIO 2.1* ‡2 §H9 & resulting *DIO 1.1* ‡6 fn 37.]
Even more serious in connection with his use of lunar occultations (see also fn 46) are the errors in his tables for the longitude of the Moon, which have a standard deviation of $35'$ (C238), more than 10 times the $2'$ agreements he managed to arrange on the basis of computations from these tables.
[For similar outrageously-overneat data-agreements in a modern hoax: see *DIO 10* §G2.]

**⊙17** [Note to §P1.] In brief, Ptolemy used observations (real or no) contemporary and ancient to him, to prove that Mercury's apogee precessed at the 1° per century rate of stellar precession. This becomes critical because he then assumed that the other four planets' apogees precessed at the same rate. By contrast (a contrast that is curious for a work attempting to synthesize astronomy), he kept the solar apogee fixed. This feature presumably represents an assent to the by-then near-sacred nature of Hipparchus's solar orbit.

**⊙18** [Note to fn 50.] Correct DR solutions to Ptolemy's mean motions of Mercury, Venus, & Saturn were transmitted to Owen Gingerich & R.Newton in 1980. [DR missed on Mars & Jupiter; however, his general (then-heretical) thesis that period-returns explained all the planets' mean motions was luckily redeemed — and placed beyond all doubt — when Alex Jones deftly solved Mars' & Jupiter's motions at a stroke in 2003 Sept: see the Jones source cited at fn 50.]