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Ptolemy Revisited: a reply to R.R.Newton

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R.R.Newton and I not only have a fundamental disagreement as to whether we have sufficient knowledge and insight into Hellenistic astronomy to evaluate Ptolemy's character and motivation, but we also differ in our appreciation of Ptolemy's contribution to the advancement of science. Newton weeps because Ptolemy's success 'has probably caused us to lose almost all the vast body of accurate Hellenistic observations'. In contrast, I have noted that Ptolemy was the first astronomer to show how to convert specific numerical data into the parameters of planetary models. In later centuries we find frequent changes of parameters in the planetary tables, but exceedingly rarely do we find the observational basis for making these changes. I am sorry that Islamic and mediaeval astronomers did not learn their lessons well enough from Ptolemy, although at least they had a framework from which to work.

Newton complains that I have not squarely addressed certain arguments that he has mentioned repeatedly. He fails to recognize that I was not attempting a review or rebuttal of his position, but instead I tried to focus on certain other quite interesting aspects that need to be considered in evaluating Ptolemy's success.

Neither time nor space nor inclination permits a sentence-by-sentence analysis of Newton's position as set forth in the preceding paper. Let me skip over the first part of it, where he once more recapitulates his own position, and go straight to the point where he attacks my statement that Ptolemy's parameters seem generally more accurate than his data base. Newton immediately provides the material to refute his own position. We all know that, at the initial level of sophistication, the motion of the Moon is the most recalcitrant and intractable case in the celestial mechanics of our system, and that Mars, because of its comparatively high eccentricity and close approaches, shows the most clearly the deficiencies of any planetary model. As Kepler later wrote, 'This is why I consider it again an act of divine Providence that I arrived at Benatek at the time when [Longomontanus] was directed toward Mars; because for us to arrive at the secret knowledge of astronomy, it is absolutely necessary to use the motion of Mars; otherwise it would remain eternally hidden'. Yet it is precisely for Mars and the Moon that Ptolemy comes closest to choosing the parameters that realize the potential accuracy of his models, as R.R.Newton states. The Moon is the more complicated case, and that is why I chose to examine it in some detail in my paper, and to show that Ptolemy's parameters were demonstrably better than the particular observations he cites. There is a curious parallel here to Kepler's results with Mars, for him the most difficult of the planets, yet the one for which his ephemerides gave the most reliable results. It seems to me that if Ptolemy concentrated his attention on Mars – indeed, it was the only case where he could have readily established his equant-eccentric model – then it is here (or alternatively, in the lunar case) where we must seek our insights into Ptolemy's procedures.

R.R.Newton states that Ptolemy does not use the observations he quotes to find his adopted mean motion of Mars, and that Ptolemy uses six observations to find only four parameters (eccentricity of the deferent orbit, its apsidal line, the epoch, and the size of the epicycle). I argue that Ptolemy uses five observations to find five parameters including the mean motion. In *Almagest* IX.3 Ptolemy says he will first set down the mean periods as calculated by Hipparchus, although these have been corrected by procedures that he will demonstrate in due course. He then says that Mars goes 42 revolutions plus 3 1/6° in 79 solar years plus 3·22 days; as O.Neugebauer has pointed out, the 42–79 combination is the one found in the Babylonian goal year texts. Because Ptolemy's mean motion tables (*Almagest* IX.4) are set up in increments of Egyptian years ($365^{d}\cdot0000$) whereas the foregoing period is in terms of Ptolemy's solar year ($365^{d}\cdot2467$), I shall tabulate Mars' period in both units:

	Ptolemy's tropical years	Egyptian years
Almagest IX.3	1·88077	1·88204
Almagest IX.4	1·88077	1·88204
Almagest X.9	1·88077	1·88204
Modern value (1)	1·88086	1·88214

In *Almagest* X.9 Ptolemy compares an observation from 272 BC with one of his own from AD 139. The early observation, an occultation of β Scorpii by Mars, seems quite plausible since recalculations show that the very close approach did take place; Ptolemy was rather astute to pick such an observation for the earlier epoch, as this sort of phenomenon guarantees a good positional accuracy. Unfortunately, he had problems converting the Greek calendar to his own, possibly because of an ambiguity with leap days, and hence he was one or two days wrong in the dating; the occultation occurred during the day on January 16 rather than early in the morning of January 18 as he thought. Because of the long temporal baseline, such an error of a day affects the period by only one part in the fifth decimal. Of course, Mars was not at the same place in its epicycle on −271 January 18 and 149881·7 days later on +139 May 27, and therefore Ptolemy had to have the entire solar theory in hand to remove the effect of the epicyclic motion before he could establish the Martian mean motion. His procedure is rather foreign to contemporary tastes since he derives the synodic period rather than the sidereal period that is directly needed for setting up the mean motion table. Thus in *Almagest* X.9 he finds that the motion in the epicycle with respect to the centre of deferent is 192 cycles plus 61°43′, leading to the synodic period of $779^{d}\cdot938$; given Ptolemy's solar period, the mean tropical period ($686^{d}\cdot944$) is easily found as I have listed it above. Hence, I am convinced that Ptolemy has correctly used the early observation to derive the value of the mean motion he has claimed to find and which he used in the tables. R.R.Newton denies this, saying 'the reader can easily verify the point himself by doing the required arithmetic'. I have done this above with results contrary to his, but I must say that it was not easy to keep straight the periods in Julian years, sidereal years, tropical years, Ptolemaic solar years, Egyptian years, etc. Unfortunately, the *Crime* is not as lucid as the *Almagest*, so I cannot be sure if Newton has confused the units in tropical solar years of IX.3 with the units in Egyptian years of IX.4. In any event, it is far easier to verify a given period than to establish it in the first place, as Ptolemy must have done.

Since writing his *Crime*, R.R.Newton has decided that a sixth observation of Mars is involved; his Table 1 of the preceding paper shows two observations on 139 May 30, but the table does not show that both of the observations give the same position, so that it would of course have been impossible for Ptolemy to derive a sixth parameter from the 'additional' observation. Incidentally, it is quite remarkable to see Ptolemy deriving a highly accurate epicycle size from a pair of observations near opposition only three days apart. This seems to be another clear example where Ptolemy has introduced an observation for pedagogical purposes, but really used other quite different material to establish the actual parameter. I am prepared to believe that Ptolemy 'laundered' his Mars observations to make them consistent with his determination of the epicycle size from the other observations that gave greater leverage on the solution.

R.R.Newton's 'most serious objection' to my paper is that I have ignored two of his three most important 'proofs' of fabrication. I did mention and agree that the equinoctial and solstitial data appear to be calculated rather than observed as Ptolemy implies, although I discussed this situation primarily to indicate not only that this has been known for some centuries, but that there are also alternative suggestions as to why it came about. I felt, however, that the star catalogue or the apogee of Mercury lay beyond the scope of my previous paper.

I consider Newton's statistical demonstration concerning the distribution of fractions of degrees in the star catalogue to be the single most convincing and clever contribution that he has made. He has shown that the distribution for the longitudes closely approximate that for the latitudes, *provided a shift of 40 arcmin is made* for the longitude. This strongly suggests that the catalogue was set up on one reference system, and then updated by the simple addition of $n°40'$ to all the longitudes. Suppose that Ptolemy used an existing reference catalogue from Hipparchus in order to establish the relative places of additional stars, and then precessed the results to his own epoch by increasing the longitudes by $2°40'$. Had Ptolemy actually made his observations as stated, with an armillary, this would leave a tell-tale sinusoidal variation in the latitudes, as Dennis Rawlins has pointed out. Such a variation is not found. Hence, it seems to me intrinsically more reasonable to suppose that Ptolemy appropriated an existing catalogue, presumably derived from Hipparchus, rather than that he started from scratch as described in the

Almagest. I don't doubt that Ptolemy wanted his opus to be the 'complete' handbook, containing not only an extensive star catalogue but also instructions for how to make a list from first principles. In a similar way theoretical astronomers today write general textbooks in which they describe how telescopes work and how observations are made even if they have never made observations themselves. It is unfortunate if Ptolemy failed sometimes to distinguish between the theoretical and the observational, but this scarcely makes him a criminal.

The case of the Mercury model is quite a curious one, but here Newton seems to ignore the fundamental observational constraint that plagued Ptolemy (2). At sunset in September, when the ecliptic runs below the equator to the southwest, Mercury at eastern elongation will set before it is dark enough to measure, and similarly at sunrise in March with Mercury at western elongation, the sky will be too bright before Mercury is high enough above the horizon. 'It is simply not true that older observers could not have located Mercury accurately at maximum elongation when the mean sun is at [the apogee or perigee points],' states Newton in his *Ancient Planetary Observations* (p. 464), but as counterexamples he goes on to cite an *evening* observation in April and a *morning* observation in October!

Poor Ptolemy! Because he couldn't get the symmetrical observations he needed, he blew the interpretation, coming up with an apsidal line 30° wrong. R.R.Newton has no such observational constraints, of course. He has picked from the almanac 51 longitudes at 80-day intervals, and has compared them with Ptolemy's predictions. Never mind that Mercury can be properly observed over only a small fraction of its trajectory, and not even at all of the maximum elongations. Newton's computer clearly shows him that Ptolemy's Mercury model would be improved if some of the parameters approached zero.

In fact, according to Newton's analysis, not only does Ptolemy continually cheat, but he is incompetent as well. For example, because he likes to keep things simple, Ptolemy places the so-called equant point opposite from the Earth an equal distance beyond the centre, whereas the modern computer shows that he would have had better success (with the longitudes, that is) if the equant had not been equally spaced.

Why then, does Ptolemy make such hard work of Mercury, with its extra wheel and very different organization? Why didn't he keep things simple by adopting the same model throughout? And if he loved forging observations, why didn't he just invent a September evening observation for Mercury and a March morning observation? At the level of accuracy to which Ptolemy is working, it is entirely the limited access of observations and not at all the eccentricity of Mercury's heliocentric orbit that has given the trouble. Clearly there are some very serious observational constraints involved that Newton has tacitly ignored. Ptolemy's Mercury model is so convoluted, in fact, that I can only believe that Ptolemy has taken mediocre observational data much too seriously, rather than that he has fudged perfectly good data to come up with something so different from all the other planetary cases.

R.R.Newton deserves credit for bringing so forcibly to our attention the inconsistencies and anomalies in Ptolemy's work. He has opened up some highly intriguing questions. Nevertheless, I believe, as J.D.Mulholland put it succinctly in another context (3), 'What is wanted in history is neither a measure of plausibility nor of ridiculousness. One wants to understand the evolution of ideas: *why* a particular ideas was presented by a particular person at a particular time and how that idea related to the social cultural and intellectual milieu of the time and place. The question of truth or falsity is practically irrelevant'. Although I consider the Mercury case to be a strong counterexample to Newton's argument, nevertheless, I feel that it is far more interesting to find what aspects of the situation misled Ptolemy, rather than to brand him a criminal. As for R.R.Newton's litigation concerning Ptolemy's nefarious motives, I still have to adopt that peculiarly Scottish verdict: *not proven.*

REFERENCES

(1) The mean motion of Mars per tropical century given by P.K.Seidelmann, L.E.Doggett & M.R.De Luccia, 1974. *Astron. J.*, **79**, 58, has been converted from tropical years of $365^{d}\cdot24220$ to units of Ptolemy's tropical solar years and Egyptian years.
(2) See my 'The Theory of Mercury from Antiquity to Kepler', *Actes du XII Congrès International d'Histoire des Sciences* (Paris), **3A**, 57–64, 1971.
(3) *J. Hist. Astron.*, **11**, 69, 1980.