

## Was Ptolemy a Fraud?\*

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The greatest surviving astronomical work from antiquity is the *Almagest* or *Syntaxis*, a mathematical treatise written in Alexandria around AD 145 by Claudius Ptolemy. In some of the manuscripts, the *Almagest* begins with the epigram, ‘I know that I am mortal by nature, and ephemeral; but when I trace at my pleasure the windings to and fro of the heavenly bodies I no longer touch earth with my feet: I stand in the presence of Zeus himself and take my fill of ambrosia, food of the gods’ (1). The epigram seems to place Ptolemy within the long series of scientists who have tasted the intoxicating pleasure of a splendid theory.

It is difficult to convey the elegance of Ptolemy’s achievement to anyone who has not examined its details. Basically, for the first time in history (so far as we know) an astronomer has shown how to convert specific numerical data into the parameters of planetary models, and from the models has constructed a homogeneous set of tables – tables that employ some admirably clever mathematical simplifications, and from which solar, lunar and planetary positions and eclipses can be calculated as a function of any given time. Altogether it is a remarkable accomplishment, combining in a brilliant synthesis a treatise on theoretical astronomy with a practical handbook for the computation of ephemerides.

Possibly for pedagogical reasons Ptolemy strove for even greater completeness by including descriptions of observational techniques, but in retrospect he has managed only to cast doubt on the veracity of his text. For example, the *Almagest* opens with the theory of the Sun’s motion. Ptolemy describes how the time of the equinoxes can be measured by watching the shadows on a bronze ring accurately aligned to the celestial equator, and he goes on to specify his own ‘very careful’ observation of an equinox on a date corresponding to AD 139 September 26 (2). Modern calculations show that the equinox actually fell some 30 hr earlier. Now we can imagine that Ptolemy was an excellent theorist but a clumsy observer; nevertheless, our suspicions are aroused when we discover that Ptolemy’s reported time agrees precisely with an extrapolation from an observation by Hipparchus 278 years earlier, an extrapolation depending on the slightly long length of the year rather arbitrarily arrived at by Hipparchus. Did Ptolemy fabricate his purported equinox observation? Perhaps.

This situation has been known for a long time, and over two centuries ago, in 1753, Tobias Mayer discussed it in a letter to Leonard Euler. What happened, according to Mayer, was that Ptolemy had to start somewhere, and he knew that the time-honoured Hipparchan parameters for the eclipse

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\*Based on a paper given before the Royal Astronomical Society, 1978 January 13.

theory worked very well. Thus, despite the ordered presentation of the *Almagest*, Ptolemy actually took the eclipse theory as his fundamental base and from there proceeded to the planetary theory, adopting the Hipparchan solar and lunar parameters in order to work out the planetary parameters. Mayer wrote, 'It can be that Ptolemy perceived the error of his solar tables in his observations of the equinoxes, which are the very last of his extant observations; but, because he had already built his whole system upon it, he perhaps preferred to discard his observations rather than to start all over again. Since, however, no one could object, he pretended that the erroneous equinoxes of his tables were true and observed. There are more recent examples of astronomers, from too great a love for their constructions, falsifying observations (in Lansberg and Riccioli for sure). Ptolemy, who perhaps did not imagine that anyone would ever be able to detect this deception, could easily have fallen into this error' (3).

Over the course of time Ptolemy's apparent fabrication of his equinox observations have been repeatedly rediscovered, for example, by the astronomer-historian Delambre (4), and about a decade ago these data were thoroughly analysed by John Britton in a doctoral dissertation at Yale. Britton noticed that atmospheric refraction would seriously confuse the equatorial ring method of establishing the equinox, so that in about half the cases the illumination would change from the upper- to the under-side of the ring on the day following the autumnal equinox, and in one quarter of the cases at vernal equinox the illumination would change on two successive days even if the ring were perfectly aligned. Ptolemy seemed aware of the problem, although not of all its causes, when he wrote 'And anyone can see an example of this in the bronze rings in the Palaestra [square for gymnastics], which are supposed to be in the plane of the equator. For in making observations we find such a distortion in their placement, especially the larger and older they are, that at times their concave surfaces twice suffer a shift in lighting at the same equinox' (5).

Ptolemy himself does not specify precisely how he made his equinox observations, although a bronze equatorial ring is implicated. It is quite likely that he checked the equinoxes only for plausible agreement with Hipparchus rather than 'most accurately' as he states. Britton summarizes the affair by saying, 'The conclusion that Ptolemy's equinox observations can have been scarcely no more than the results of computations is unsatisfying, but I can find no other explanation of the errors in his report of times and their agreement with Hipparchus' observations in length of the year. On the other hand, if Ptolemy set out to determine the times of the equinoxes using an equatorial ring, he could not have avoided encountering the difficulties and irregularities [of refraction]. Thus he might easily have concluded that he could make no secure improvement on Hipparchus' solar parameters' (6).

Concerning the single recent solstitial date given by Ptolemy, he made no claims for its accuracy except to say that he had *computed* it with care, nor did he specify a method of observations. While it is possible to deduce the time of the solstice fairly accurately by making altitude observations of the Sun some days before and after the actual solstice, when the Sun is still

changing perceptibly in declination, it is difficult to establish the time by observations adjacent to the date itself. It has been known since the end of the last century that the summer solstice date given by Hipparchus derives from the traditional parameters of the Babylonian system A solar theory. The solar motion on its slow arc (7) is  $28;7,30^{\circ}/m$  or, converting with the modern value of  $29^d.5306$  for the synodic lunar month,  $0.95241^{\circ}/d$ . Since the Sun began its slow arc around the beginning of March (to use the Julian calendar anachronistically) and finished in August, the interval from vernal equinox to summer solstice lay entirely within the slow arc. Ninety degrees at  $0.95241^{\circ}/d$  yields  $94^d.50$ , the precise interval adopted by both Hipparchus and Ptolemy.

Now an error of a day in establishing the time of the equinoxes results in an error of about a degree in the solar position, that is, in the deduced position of the invisible intersection of the ecliptic and equator. Ptolemy used positions of the Moon (at lunar eclipses and otherwise) to transfer his solar reference frame into the starry night-time sky. Hence his erroneous value in the solar position propagated into his fundamental coordinate system, including the positions given for the planets and for the stars in the catalogue, and in turn the faulty stellar positions led to a defectively small value of precession (8).

Within the past several years the geophysicist R.R.Newton has rediscovered these underlying difficulties in the *Almagest*, but he has gone much further than previous workers in analysing Ptolemy's data and even his motivations. Thus he has been led to conclude his recent book, *The Crime of Claudius Ptolemy*, by saying, 'The *Syntaxis* has done more damage to astronomy than any other work ever written, and astronomy would be better off if it had never existed. Thus Ptolemy is not the greatest astronomer of antiquity, but he is something still more unusual: he is the most successful fraud in the history of science' (9).

In coming to the defence of Ptolemy's scientific reputation, I shall concede at once that the *Almagest* poses some curious problems to the historians of science, and that Ptolemy's statements regarding observed quantities cannot always be taken at face value. R.R.Newton has systematically examined the observations reported in the *Almagest* and he finds (as I also do) quite appreciable errors compared to the actual positions of the celestial bodies at those times (10). These errors can be as much as a degree, and sometimes more. He also finds (and I generally confirm) that the reported positions agree very closely with Ptolemy's theory – generally well within 10 arcmin. Somehow, according to Newton, the match with theory appears just too good. In his opinion this means that Ptolemy simply made up these observations.

Yet it has always seemed to me that this hypothesis runs into a serious difficulty. How can Ptolemy's parameters, which seem generally more accurate than his data base, be derived from observations that are simply fabricated?

We can consider four hypotheses:

(i) Ptolemy borrowed good hypotheses from elsewhere (Hipparchus?)

Babylonians? A lost civilization?) and made up his theory to look as if he had done it all.

(ii) Ptolemy selected from a large data bank only those observations that fit the theory.

(iii) Ptolemy fitted his theory to a few preferred observations.

(iv) Ptolemy 'corrected' his observations so as to agree with a theory established from numerous observations not mentioned in his work.

R.R. Newton has suggested that the mean motions, for example, were simply borrowed from the Babylonians (11). In fact, O. Neugebauer has shown that it is Hipparchus who adopted the Babylonian values and that Ptolemy's mean motions represent the refinement possible with a longer temporal baseline (12). Whence, according to hypothesis (i), the apsidal lines, eccentricities, epicycle sizes, and so on could be borrowed remains a mystery. Perhaps they were brought by ancient astronauts, but until further evidence is forthcoming, the first hypothesis is unacceptable within our present framework of historical understanding.

Hypothesis (ii), that the excellent agreement between the stated observations and the Ptolemaic theory arises from deliberate observation selection, leaves open the source of the theoretical parameters. Hypothesis (iii) leaves open the question of choosing 'preferred' observations. The reason for supposing that there was a large data bank and/or preferred observations rests on the fact already mentioned that Ptolemy's parameters seem better than the recorded observations. Before analysing the hypotheses further, we must examine this point.

Let us first consider Ptolemy's lunar theory. The *Almagest* approaches the lunar theory in three stages. The first, which Ptolemy attributes to Hipparchus, employs an epicycle on a concentric deferent. The epicycle generates the basic orbital eccentricity, and also the advance of perigee. Fig. 1 shows the outer envelope of errors in longitude for the three stages, and is adapted from the work of Viggo Petersen (13). The systematic displacement in longitude is one of the manifestations of Ptolemy's problems with the fundamental coordinate system. As shown in the upper part of the figure, the Hipparchan model is tolerably successful at new and full moons, but the errors are unacceptably large in the intermediate parts of the orbit. The discrepancies at first and last quarter arise from what is now called evection, a term discovered by Ptolemy who understood it as a change in the effective eccentricity of the orbit and who accounted for it by introducing a crank mechanism in the centre of the orbit. The errors in this second stage are shown in grey in the lower part of the figure, and as may be seen, the fit is now tolerably successful at the quarters, although some problems remain in the octants. Observing the moon in the octants has apparently never been very popular with positional astronomers, and, for example, it was only very late in Tycho Brahe's observing career that he discovered the so-called 'variation' with an amplitude of 40 arcmin (14). In any event, Ptolemy, on the stated basis of only two observations, further modified his model to reduce the error envelope to the solid section in the lower part of the figure. The residual sinesoidal effect of Tycho's 'variation' is still plainly evident.

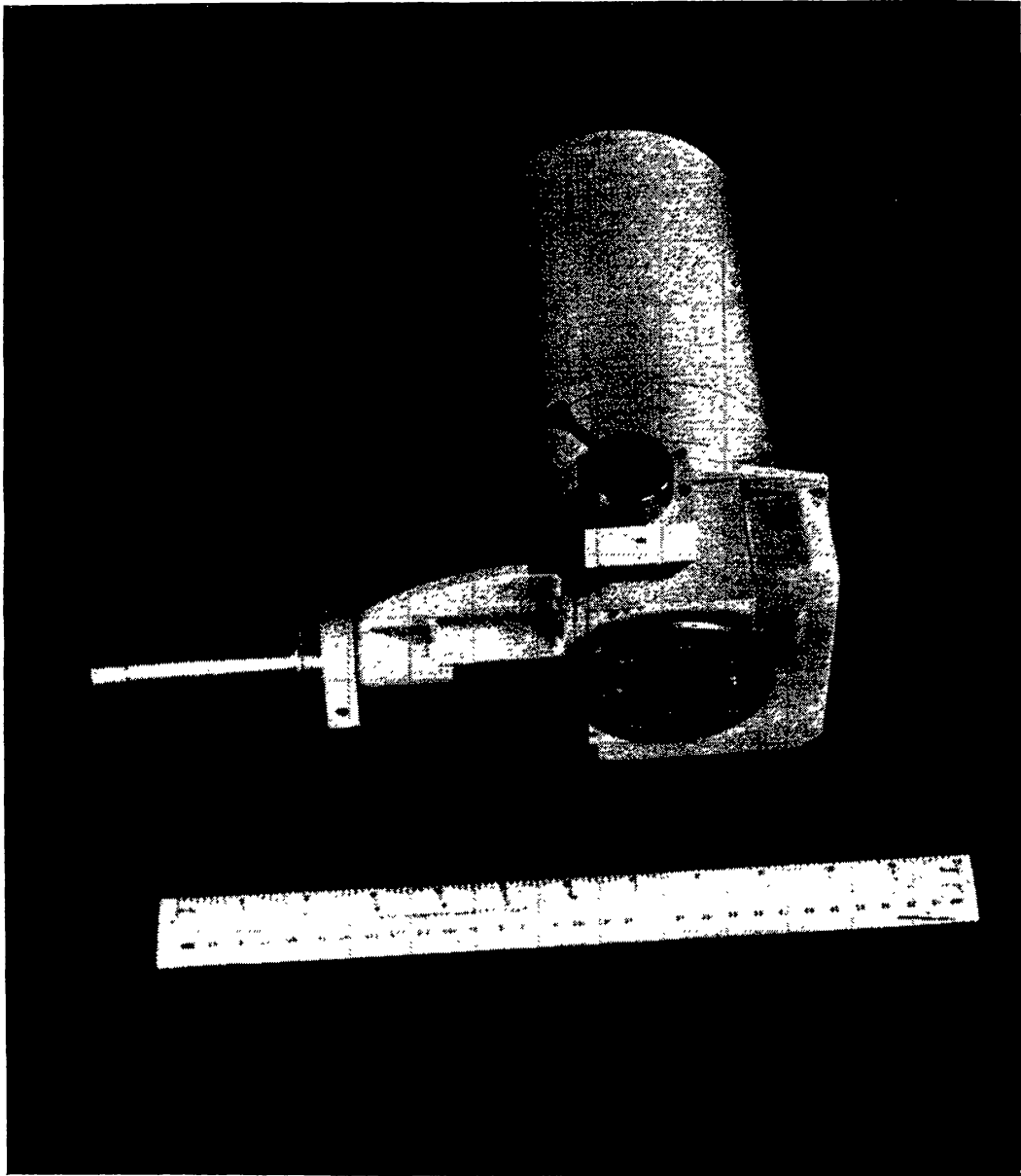


PLATE II. An 'Edinburgh' autoguider, with an image dissector as the sensing element (by kind permission of the Royal Observatory, Edinburgh), see ref. (35).

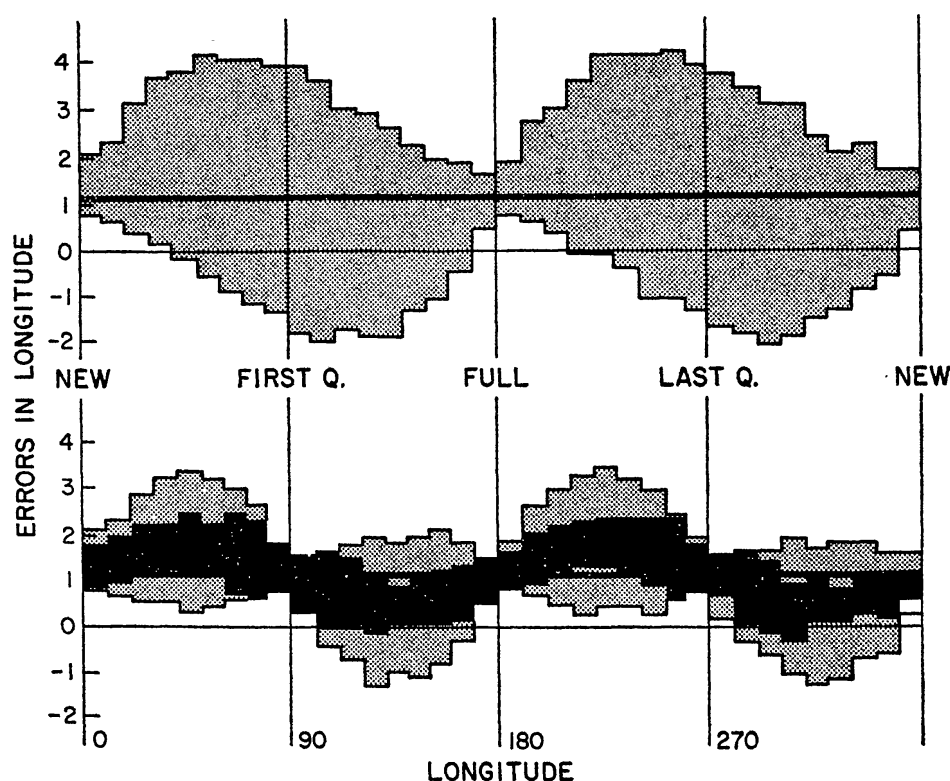


FIG. 1. The outer envelope of longitude errors in degrees for the three stages of Ptolemy's lunar theory, adapted from V.Petersen.

Plate I, from Apianus' *Astronomicum Caesareum* (Ingolstadt, 1540), shows the lunar mechanism. The direction to the Sun is toward the upper right. I have set the volvelles for the first of the two octant observations used by Ptolemy (from Hipparchus, 126 May 2). In the second form of the model the motion in the epicycle is reckoned from the line drawn from the Earth through the centre of the epicycle. Only a small change suffices to bring about the large improvement in the third form of the model, namely, measuring the motion in the epicycle from the line that originates in a point equal and opposite from the equant. Note that the observation is chosen at a very special octant time, so that the line of apsides is perpendicular to the crank mechanism, and the difference in models is maximized.

The success of Ptolemy's final lunar model may be seen in Table I by comparing his derived parameters with the terms in the modern lunar theory (15). Note the excellence of the fit for the equation of the centre and the evection. Ptolemy's second theory differs from the third by the inclusion of a spurious term,  $\sin 2\bar{\eta} \cos \bar{a}$ , which is eliminated by the small geometrical change in the models. I personally find it unbelievable that Ptolemy could have improved his theory so appreciably merely from these two octant observations. Ptolemy must have used many more observations, whereas those in the *Almagest* serve only to derive most directly the parameters after the inequality of motion had been extensively analysed. Since he had no theory of errors, he could cope with the multiplicity of data by including in the *Almagest* only the minimum number of observations required to determine

the parameters. It is possible that he picked these two as being representative of the average results from other observations. The fact that two somewhat erroneous observations fit the theory so well would then be explained by his use of carefully selected data according to hypothesis (ii), and not by fudging the observations. An alternative is that Ptolemy, who was unquestionably an able mathematician, skilfully fit his theory through the chosen observations, according to hypothesis (iii).

TABLE I  
*Lunar parameters*

|                                                                 | Modern                                                                                  | Ptolemy<br>(Stage 3)                            |
|-----------------------------------------------------------------|-----------------------------------------------------------------------------------------|-------------------------------------------------|
|                                                                 | ° ,                                                                                     | ° ,                                             |
| Equation of Centre                                              | $\begin{cases} -6 & 17.3 \sin \bar{\alpha} \\ + & 12.8 \sin 2 \bar{\alpha} \end{cases}$ | $\begin{matrix} -6 & 14 \\ + & 19 \end{matrix}$ |
| Evection                                                        | $-1 \quad 16.4 \sin (2 \bar{\eta} - \bar{\alpha})$                                      | $-1 \quad 16$                                   |
| Variation                                                       | $39.5 \sin 2 \bar{\eta}$                                                                | —                                               |
|                                                                 |                                                                                         | (Stage 2)                                       |
|                                                                 |                                                                                         | ° ,                                             |
| Spurious term                                                   | — $\sin 2 \bar{\eta} \cos \bar{\alpha}$                                                 | $-1 \quad 16$                                   |
| [ $\bar{\alpha}$ = Mean anomaly $\bar{\eta}$ = Mean elongation] |                                                                                         |                                                 |

Table II illustrates a clear case of fitting a theory precisely through very bad observations, done not by Ptolemy, but by Copernicus 1400 years later. Copernicus, like Ptolemy, gives only four Martian observations from his own century, and two of them are very bad indeed, far worse than Ptolemy's data. Nevertheless, Copernicus has succeeded in fitting his Mars model to this wretched data within 10 arcmin (16). It is interesting to notice that his successor, Erasmus Reinhold, was able to perform the same geometric operation with slightly greater precision, but it apparently never occurred to Reinhold to question the underlying data itself.

TABLE II  
*Copernican Mars positions*

|                  | Observed<br>— Actual<br>° , | Observed<br>— Theory<br>(Copernicus)<br>° , | Observed<br>— Theory<br>(Reinhold)<br>° , |
|------------------|-----------------------------|---------------------------------------------|-------------------------------------------|
| 1512 June 5      | — 20                        | —9                                          | —2                                        |
| 1518 December 12 | +1 17                       | —9                                          | —3                                        |
| 1523 February 22 | +2 16                       | —2                                          | —5                                        |
| 1512 January 1   | + 2                         | +4                                          | +4                                        |

Let us next look at Ptolemy's value for the perigee of Venus. Venus has a very low eccentricity, so it is not easy to find the orientation of its orbit. The model requires the vector sum of *ae* (semi-major axis times the orbital eccentricity) for the Earth and for Venus; because *ae* for the Earth's orbit is so much larger, it predominates, as may be seen in the small vector triangle

inset into Fig. 2. Ptolemy claims to find the perigee line from the two pairs of matched elongations. These are plotted on the figure (which is shown anachronistically in a heliocentric layout). Ptolemy's result of  $235^\circ$  (for c. AD 130) is astonishingly good, with an error of only  $3^\circ$ .

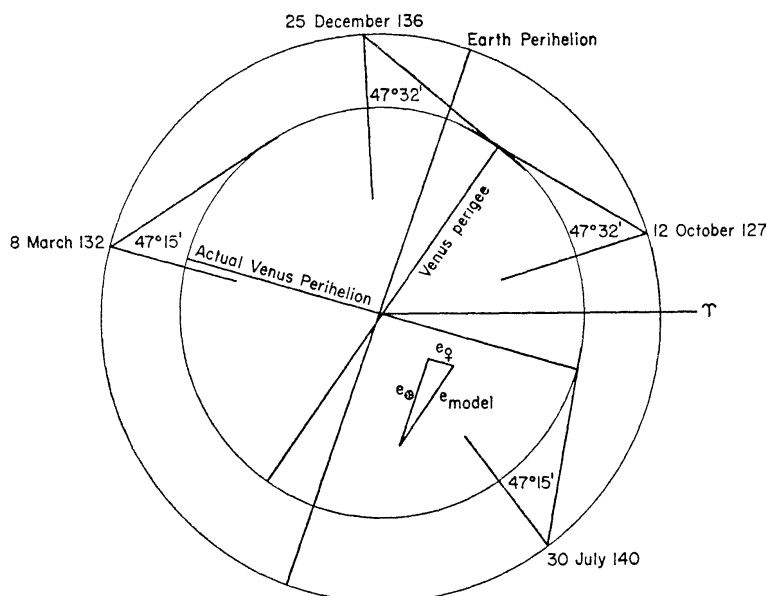


FIG. 2. Ptolemy's observations for the Venus perigee shown (anachronistically) on a heliocentric scheme. The inset shows an enlargement of the central vector triangle. The commensurability of the periods of Venus and the Earth prevented Ptolemy from picking arbitrary sighting points.

The word 'astonishingly' is used advisedly, for when we make a critical examination of these four observations, comparing Ptolemy's values with the actual positions as recomputed today (17), we find nothing like the aesthetic symmetry that marks the *Almagest*'s presentation. In fact, Ptolemy has sometimes reported his solar and Venusian observations rather roughly, often to an accuracy of only a quarter of a degree, and these do not even agree with the theoretical values derived from the tables of the *Almagest*. It stretches our confidence beyond its breaking point to suppose that Ptolemy established the very sensitive geometry and the perigee within  $3^\circ$  of the correct value by these two pairs of identical elongations. Either Ptolemy determined the perigee from other data and then by incredible luck was able to select from his data bank the pairs that just happened to match, or else the data has been 'laundered'. In other words, hypothesis (iii) seems untenable (although I shall consider it further below), but hypotheses (ii) and (iv) are possible. However, if Ptolemy had adjusted his numbers for the elegance of his presentation, according to hypothesis (iv), then where was the basis for such a good determination of the perigee?

In my opinion the only reasonable explanation is that Ptolemy had a large number of elongation observations, none very precise, but sufficient to find the perigee line. Probably the pedagogic standards of his day (of which we know very little) dictated a simple presentation of his results, so he set forth a

minimum number of symmetrically placed points. However, for this planet, Ptolemy was faced with a special problem in finding a symmetrical set of elongations, because there is a rhythmic interlock between the period of the Earth and Venus. As a consequence, Venus tends to repeat its pattern of greatest elongations with the same five terrestrial positions. In Ptolemy's day this pentagonal array was not symmetrical with the perigee line, and consequently he could not actually use greatest elongations. It was therefore not easy for him to obtain consistency, and for the dates chosen Venus was not quite at its greatest elongation. Hence, substantial discrepancies remain between the putative positions of Venus and those predicted by the tables in the *Almagest* itself (Table III).

TABLE III

|                    |                 | Mean<br>Sun | Venus  | Elongation |
|--------------------|-----------------|-------------|--------|------------|
| 132 March 8·75     | Ptolemy         | 344 15      | 31 30  | 47 15      |
|                    | Almagest theory | 344 14      | 31 24  | 47 10      |
|                    | Modern          | 345 18      | 32 9   | 46 51      |
| 140 July 30·25     | Ptolemy         | 125 45      | 78 30  | 47 15      |
|                    | Almagest theory | 125 42      | 79 10  | 46 22      |
|                    | Modern          | 126 48      | 80 40  | 46 08      |
| 127 October 12·25  | Ptolemy         | 197 52      | 150 20 | 47 32      |
|                    | Almagest theory | 197 51      | 150 27 | 47 24      |
|                    | Modern          | 198 54      | 151 50 | 47 04      |
| 136 December 25·75 | Ptolemy         | 272 4       | 319 36 | 47 32      |
|                    | Almagest theory | 272 3       | 319 50 | 47 47      |
|                    | Modern          | 273 8       | 320 2  | 46 54      |

When we turn to Ptolemy's treatment of the superior planets, we find four observations per planet from his own lifetime. As illustrated in Fig. 3, these data are reasonably accurate with respect to his coordinate system (that is, with the systematic precessional error of  $1^{\circ}\cdot1$ ), but they agree even better with the predictions of Ptolemy's theory. The differences between the longitudes as computed from the tables in the *Almagest* and the modern calculations are shown by the continuous error curve on the diagram. The four observations suffice to define four orbital elements: the eccentricity, direction of perigee, time of perigee passage, and the size of the epicycle. There is no redundancy of data, and in principle Ptolemy, like Copernicus after him, could have derived the parameters for the orbits from these points, so that the error curve would necessarily pass through the data points, that is, according to hypothesis (iii) (18).

For Venus (Fig. 4) the situation is somewhat more complicated. Four observations (those diagrammed in Fig. 2 and shown with solid circles in Fig. 4) are used to demonstrate the direction of perigee, and five observations determine the three remaining orbital elements. As may be expected *if the observations are not fudged*, three of these points fall precisely on the error curve, and two do not.

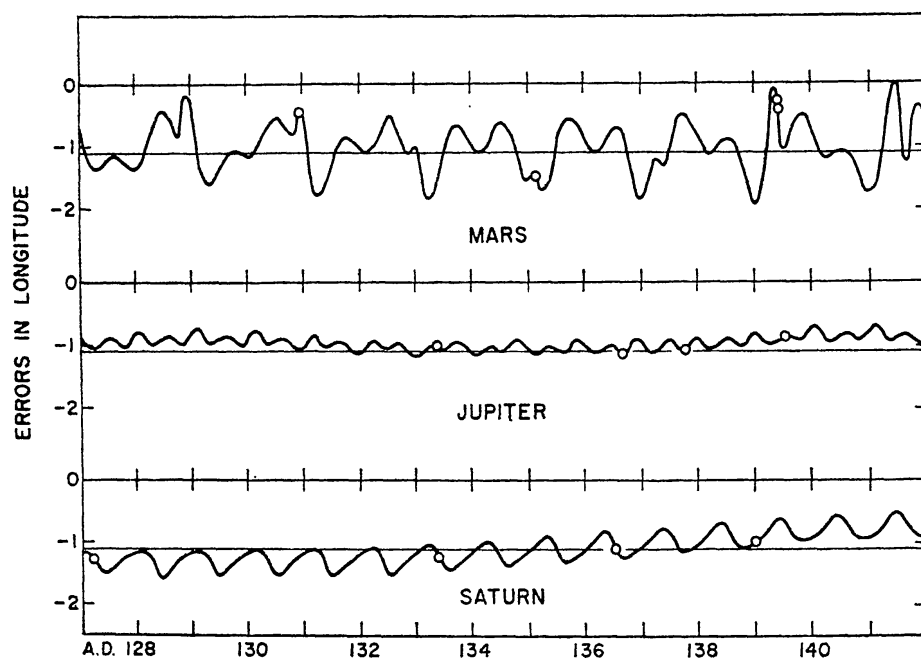


FIG. 3. Errors in the longitudes in degrees predicted from the *Almagest's* planetary theory, with the observations reported by Ptolemy shown as circles.

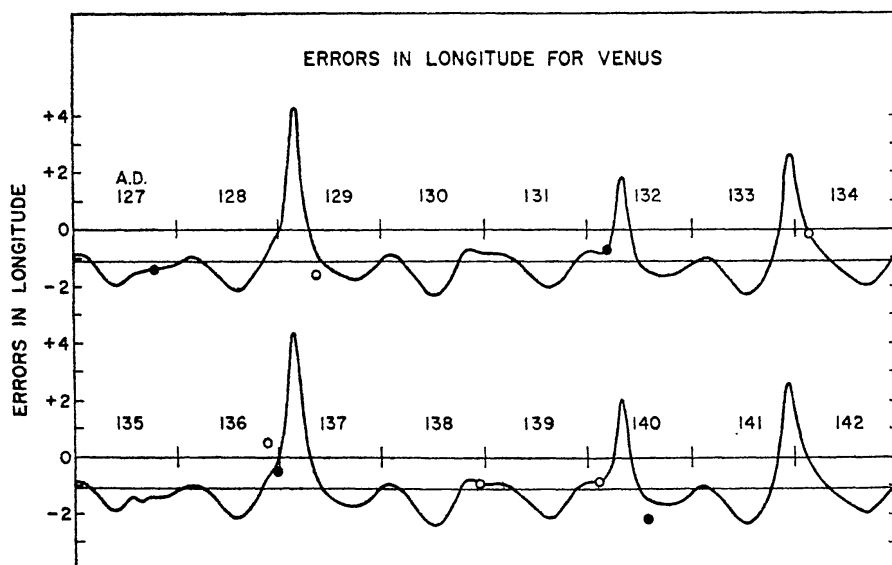


FIG. 4. Errors in the longitudes in degrees predicted from the *Almagest's* theory for Venus, with the observations reported by Ptolemy shown as circles. Solid circles represent the symmetrical observations used to find the perigee line. The large errors occur at inferior conjunction when Venus is lost in the Sun's glare.

In the case of Mercury (Fig. 5) six of Ptolemy's own nine observations fall within 6 arcmin of the error curve. Six observations are used to determine the apogee and (spurious) double perigee. Without dwelling on the details of this latter bizarre situation (19), I can remark that Ptolemy must surely

have put credence in some specific observations here, or he would not have ended up with such an unnecessarily complicated mechanism for Mercury. An attentive analysis (20) shows that Ptolemy has no redundant observations here except possibly among the four symmetrical ones used to establish the apogee line, and three of these deviate from the error curve.

It is clear that Ptolemy's data are presented for specific geometrical configurations. His observations are surprisingly bad while his final parameters are amazingly good. Finally, we have the disconcerting fact that most of Ptolemy's reported observations, faulty as they are, agree almost perfectly with his theory. In the foregoing discussion I have mentioned three possible reasons for this remarkable state of affairs. In many instances Ptolemy, a redoubtable mathematician, could have derived his parameters with the

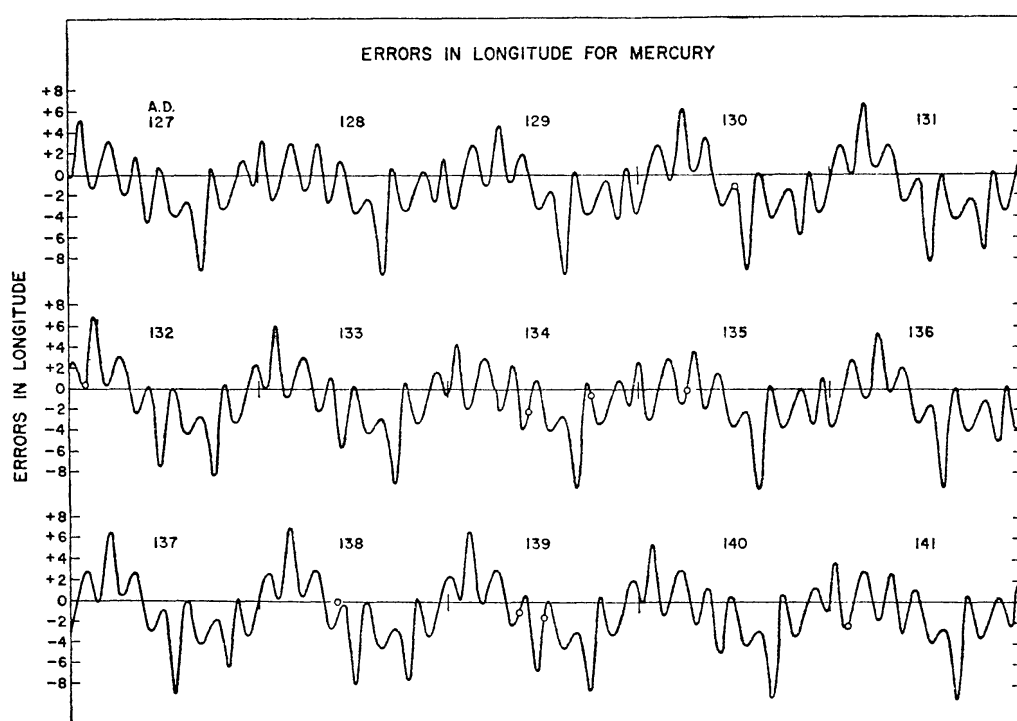


FIG. 5. Errors in the longitudes in degrees predicted from the *Almagest's* theory for Mercury, with the observations reported by Ptolemy shown as circles.

minimum number of observations, thereby forcing the theoretical positions to match the given data (hypothesis (iii)). One striking feature of Fig. 3 suggests that, plausible as this hypothesis may be, it is incorrect. In principle the observations for Mars ought to be as accurate as those for Jupiter or Saturn, as in no case does Mars move fast enough for the timing of the observation to be critical. Yet the four reported observations of Mars show the same large scatter that the theory for this recalcitrant planet exhibits. Jupiter and Saturn, with more accurate theories, also have less scatter in the observations. This strongly suggests that the recorded 'observations' depend on the theory, and not *vice versa*.

Alternatively, Ptolemy could have selected from a much larger group those observations that happened to fit what he considered to be his best parameters (hypothesis (ii)). Finally, he could have 'corrected' his observations in some unspecified manner (hypothesis (iv)). By strict contemporary standards either the selection or the hidden correction of data is considered reprehensible, though not necessarily fraudulent. In reality, weighting or rejecting some experimental data is probably the rule rather than the exception. It is instructive to look briefly at two well-documented non-astronomical examples.

The nineteenth-century American anthropologist, Samuel George Morton, a scholar of the highest reputation, measured the cranial capacities of some hundreds of human skulls. At that time virtually everyone assumed that brain size was directly correlated with brainpower, a conclusion that is nowadays entirely discredited. Morton's results, published in 1839 (and more extensively in 1849), confirmed the expectations of the day: Caucasians had the biggest brains, American Indians the next, and Negroes the smallest. However, Morton, unlike Ptolemy, published not only his results but also his original data, and recently S.J. Gould re-examined the entire lot (21). He found Morton's data to be 'a patchwork of assumption and finagling controlled, probably unconsciously, by his *a priori* assumptions'. For example, Morton, anxious not to distort his results by full inclusion of his over-abundant samples of small Hindu skulls, deliberately excluded 14 specimens, a selection that increased the average size of the Caucasian group. Gould's re-averaging of all the data shows no significant differences in cranial capacity of the three races; nevertheless, he was able to find 'no indication of fraud or conscious manipulation'. He writes, 'Unconscious finagling is probably the norm. . . . We measure greatness not by "honesty", but by insight,' but he adds, 'I do not condone or excuse finagling just because I regard much of it as intrinsic to scientific activity'.

Another fascinating example of the selection of experimental data has been brought to light with Gerald Holton's inspection of Robert A. Millikan's laboratory notebooks, which record the measurements for his famous oil-drop experiment (22). Despite Millikan's published statement that '*this is not a selected group of drops, but represents all of drops experimented on during 60 consecutive days*', the notebooks are peppered with such remarks as '*Publish this surely, beautiful!*' and '*Error high will not use*'; eventually 58 out of 140 drops were selected as 'the drops experimented on'. The others were considered abortive non-experiments.

At many stages in his research, Ptolemy must have been forced to choose and select between conflicting data. Yet not until the work of Kepler would an astronomer record the redundant and contradictory observations from which his theory was actually wrought. But can the observations actually found in the *Almagest* be adequately accounted for by a sophisticated selection from existing data? Although this is possible (and I certainly think that the evidence is strong for the existence of such a data pool), I am now inclined to believe that the most likely scenario for Ptolemy's procedures is neither the precise fitting of the given observations nor an exacting selection from existing data, but hypothesis (iv), namely, that he has 'corrected' his observations in some fashion which, regrettably, he has not bothered to state.

Because Ptolemy's parameters are generally pretty good, we must assume that he had a substantial data base beyond what is specifically preserved in his treatise. But a redundant data base with random observational errors must have yielded conflicting parameters that depended on the particular minimal combination used. Ptolemy probably realized this, and quite likely he noticed that in using different combinations, certain results appeared more frequently, giving him the basis for a preferred set of parameters. At the same time, he might have realized that most, if not all, of his observations contained accidental observational errors. Although my suspicions remain highly speculative, I suspect that Ptolemy, convinced in the intrinsic soundness of his theory, simply replaced the only partially trustworthy observations by what he perceived to be the 'correct' data.

Thus Ptolemy, like many of the brilliant theoreticians who followed him, was perfectly willing to believe that his theory represented Nature better than the error-marred individual observations of the day. As one of America's Nobel laureates remarked to me, any good physicist would do the same today. Let me cite two parallel examples from more recent times.

Isaac Newton, thanking Flamsteed for a set of lunar observations wrote: 'I am of opinion that for your Observations to come abroad with [my] Theory . . . would be much more for their advantage and your reputation then [*sic*] to keep them private till you dye or publish them without such a Theory to recommend them. For such a Theory will be a demonstration of their exactness and make you readily acknowledged the Exactest Observer that has hitherto appeared in the world' (23).

For a second example I turn to Einstein. One of his students has related an interesting incident that took place in 1919: 'Once when I was with Einstein in order to read with him a work that contained many objections against his theory . . . he suddenly interrupted the discussion of the book, reached for a telegram that was lying on the windowsill, and handed it to me with the words, "Here, perhaps this will interest you". It was Eddington's cable with the results of measurement of the eclipse expedition. When I was giving expression to my joy that the results coincided with his calculations, he said quite unmoved, "But I knew that the theory was correct"; and when I asked, what if there had been no confirmation of prediction, he countered: "Then I would have been sorry for the dear Lord – the theory *is* correct" ' (24).

It is marvelous to find these foremost theoreticians so clearly voicing their belief in the primacy of theory over observations. As for Ptolemy, we can deplore his lack of explicit comment concerning his procedures; but the circumstances, rather than affording the occasion for moral judgments on his motivations, should challenge historians of astronomy to the task of reconstructing Ptolemy's pioneering trail to the most complete mathematical achievement in ancient astronomy. When Newton and Einstein are generally considered frauds, I shall have to include Ptolemy also. Meanwhile, I prefer to think of him as the greatest astronomer of antiquity.

## NOTES AND REFERENCES

- (1) The authorship of the epigram is not completely established, but Franz Boll was inclined to attribute it to Ptolemy in 'Das Epigramm des Claudius Ptolemaeus', *Sokrates (Jahresberichte des Philologischen Vereins)*, 9, 2–12, 1921; Professor P. Kunitzsch has drawn my attention to this article.
- (2) Ptolemaeus, *Handbuch der Astronomie*, III.1, p. 142, K. Manitius, translator, 1912. Leipzig. See also Ptolemy, *The Almagest*, R.C. Taliaferro, translator, 1952. *Great Books of the Western World*, 16, p. 81, Chicago.
- (3) Mayer to Euler, 22 August 1753; adapted from the translation by Eric Forbes, 1971. *The Euler-Mayer Correspondence (1751–1755)*, p. 75, New York.
- (4) 'Did Ptolemy do any observing? Are not the observations he tells us he has made just calculations from his tables and some examples that serve for a better understanding of his theories?' J.B.J. Delambre, 1817. *Histoire de l'Astronomie Ancienne*, I, p. xxv, Paris.
- (5) *Almagest*, III.1, p. 79.
- (6) Britton, John, 1967. *On the Quality of Solar and Lunar Parameters in Ptolemy's Almagest*, p. 44, doctoral dissertation, Yale University.
- (7) Neugebauer, O., 1975. *A History of Ancient Mathematical Astronomy*, p. 372, New York. Apparently F.X. Kugler first noticed this relationship, as pointed out by Professor B. Goldstein; see Y. Maeyama, 'On the Babylonian Lunar Theory', *Archives Internationales d'Histoire des Sciences*, 28, 21–35.
- (8) The precession can be independently derived from observations of stellar declinations, and it has been noticed by Newton and earlier by A. Pannekoek, 1955. 'Ptolemy's Precession', *Vistas in Astronomy*, 1, 60–66, that the 12 declinations recorded but not used by Ptolemy would lead to a correct value, whereas the six he selected confirmed the erroneous smaller value of 36" per annum. Raymond Mercier, 1979. *The British Journal for the History of Science*, 12, 216, writes: 'Neither, however, has realized that the value of 36" per annum would follow from soundly observed occultations if they were reduced in the way indicated by Ptolemy. This reduction depends on the tropical longitude of the sun according to Hipparchus, and it was just this motion which entailed the erroneous rate of precession.' 'One can see that Ptolemy cannot be faulted really if he preferred the results based on occultations to those based on the more difficult measurements of stellar declinations.'
- (9) Newton, Robert R., 1977. *The Crime of Claudius Ptolemy*, p. 379, Baltimore.
- (10) Newton, Robert R., 1976. *Ancient Planetary Observations and the Validity of Ephemeris Time*, Baltimore.
- (11) Newton, Robert R., 1978. On a Pedagogical Motivation for Ptolemy's Fabrication of Data, *Archeoastronomy Bulletin*, 2, 7–9.
- (12) Neugebauer, O., 1956. Notes on Hipparchus, In: *The Aegean and the Near East*, pp. 292–296, ed. Saul S. Weinberg, Locust Valley, N.Y.
- (13) Petersen, Viggo M., 1969. The Three Lunar Models of Ptolemy, *Centaurus*, 14, 142–171.
- (14) Thoren, Victor E., 1967. Tycho Brahe's Discovery of the Variation, *Centaurus*, 12, 151–166.
- (15) The lunar terms in Table I are taken from O. Neugebauer, 1975. *A History of Ancient Mathematical Astronomy*, pp. 1106 ff, New York, which in turn rests on Karl Stumpff, *Himmelsmechanik*, Bd. 1 (Berlin, 1969), pp. 38 ff, but neither author gives the specific form of Ptolemy's spurious term.
- (16) These Copernican data are discussed in far greater detail in Owen Gingerich, 1978. Early Copernican Ephemerides, *Studia Copernicana*, 16, 403–407. Subsequently it occurred to me that perhaps Copernicus had faked the two bad observations because he found it impossible to fit them satisfactorily with a reasonable variation in his parameters. An investigation has now shown that better observations on the dates in 1518 and 1523 could have been readily accommodated.

- (17) Barbara Welther and I have written a FORTRAN program that produces ephemerides according to the *Almagest*, the Toledan and Alfonsine tables, *De revolutionibus*, etc. as well as modern values, the latter based on a computer code generously supplied by Professor Peter Huber formerly at the ETH in Zurich and now at Harvard. I wish to thank Miss Welther for carrying out the computer runs and plotting for this paper.
- (18) Newton, Robert R., *op. cit.*, note 11, states, "Then [J.P.Harrington] writes: "Often Ptolemy gives only the minimum number of observations necessary to determine the parameters of his models." If this were the case, the observations would necessarily agree exactly with his model, to the accuracy of the calculations. Harrington has apparently relied on a statement that Gingerich and several others have made. This is the statement that Ptolemy has five parameters in the model he uses for each of the outer planets, and that he determines these parameters by means of five observations. The first part of this statement is correct, but the second part is not. Ptolemy does not determine the mean motion in anomaly for any of the planets from the observations he quotes . . . '.

In my opinion R.R.Newton is insensitive to the astronomical situation with which Ptolemy was wrestling, and he therefore misidentifies the parameters Ptolemy is determining. Ptolemy realized that the solar motion is intimately connected with the epicyclic motion of the superior planets. Hence he *assumed* the motion in anomaly (i.e. in the epicycle) from the solar theory, and he employed a fifth, ancient observation (thereby improving the accuracy with a long time interval) to confirm his fifth independent parameter, the mean motion of each planet.

- (19) See Gingerich, Owen, 1971. The Mercury Theory from Antiquity to Kepler, *Actes du XII<sup>e</sup> Congres International d'Histoire des Sciences*, III A, 57-64.
- (20) From these six observations used initially to determine the apogee, Ptolemy also deduces the size of the epicycle and eccentricity, and from two of the remaining ones he establishes the size and position of a small crank device in the centre of the orbit. Finally, he uses the ninth datum in conjunction with an observation from antiquity to establish the period of the epicycle.
- (21) Gould, Stephen Jay, 1978. Morton's Ranking of Races by Cranial Capacity, *Science*, 200, 503-509.
- (22) Holton, Gerald, 1978. Subelectrons, presuppositions, and the Millikan-Ehrenhaft dispute, In: *The Scientific Imagination: Case Studies*, pp. 25-83, Cambridge. (See especially pp. 63-71.)
- (23) Newton to Flamsteed, 16 February 1694-5, No. 494. In: *The Correspondence of Isaac Newton*, IV, p. 87, Cambridge, 1967.
- (24) Quoted by Holton, Gerald, 1973. *Thematic Origins of Scientific Thought*, pp. 236-7, Cambridge, from a manuscript by Ilse Rosenthal-Schneider. The telegram in question came from Lorentz rather than Eddington; it survives among the Lorentz papers in Leiden. Helen Dukas, Einstein's longtime personal secretary, tells me that she has confirmed this attitude of Einstein from another independent source.