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Models and Precision:

The Quality of Ptolemy's Observations and Parameters

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PREFACE

This investigation was originally undertaken as a dissertation presented to the Faculty of Yale University in 1966 in partial fulfillment of the requirements for the degree of Doctor of Philosophy. While working on it then, I held a Graduate Fellowship from the National Science Foundation whose support I gratefully acknowledge. Subsequently, Yale University has afforded me access to their facilities as a Visiting Fellow, which greatly assisted the preparation of this work for publication. In addition, I wish to record with special gratitude the financial support for the publication of this work provided by the Neugebauer Fund at The Institute for Advanced Study (Princeton).

It is also a pleasure to express my special indebtedness and gratitude to my former advisers and subsequent colleagues, Professors Asger Aaboe and Bernard Goldstein, whose suggestions and constructive criticisms helped greatly to focus, clarify, and improve this work during its several stages. I should also like to thank Professor Gerald Toomer for his encouragement and helpful comments, not to mention his splendid translation of the *Almagest*. Among the many others who have helped in the preparation of this work I am particularly grateful to the late Professor Otto Neugebauer for his reading and helpful criticisms of the rough draft of this book. I am also indebted to the late Professor Gerald Clemence for his help in obtaining information concerning Simon Newcomb, to the late Professor Derek de Solla Price for his always stimulating suggestions, and to the late Professor Abraham Sachs, who furnished me with much information about Babylonian astronomy and with translations of unpublished cuneiform texts.

Lastly, I wish to express my profound indebtedness and special thanks to Doctor Alan C. Bowen, who initiated and carried through the publication of this work as editor. Without his resourceful and persistent labors, guided by a discriminating and sensible judgment, this publication would not have happened.

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INTRODUCTION

The following study grew out of a survey of attempts by astronomers of the nineteenth and early twentieth centuries to determine the amounts of the Moon's secular acceleration and the retardation of the Earth's rotation. In the course of it, I found substantial differences among determinations by different investigators, and particularly between the values of the secular acceleration of the Moon's elongation obtained by Newcomb [1878, 1912] from his analysis of Ptolemaic and Arabian eclipse-data, and the results obtained by others from analyses of different ancient observations. The latter include observations of equinoxes, occultations, and lunar eclipse-magnitudes reported by Ptolemy, as well as references in ancient literary sources to events which could be interpreted as solar eclipses that were total at a specific place.

It became apparent that the differences between the various determinations of the accelerations in question were partly due to assumptions made by investigators about the quality of the available evidence, and especially to assumptions about the reliability of the observations which Ptolemy reports. Newcomb, for example, argued that the Ptolemaic eclipses afforded the only reliable data from antiquity, and that taken together these eclipse-reports gave a secure value for the acceleration of the Moon's elongation. Fotheringham [1915a, 1918, 1920] and Schoch [1930], on the other hand, regarded the lunar eclipses described by Ptolemy as too ambiguous, too inconsistent, or too suspect to be useful. Hence, they preferred to use either those other observations reported by Ptolemy but made by his predecessors, or the literary reports of total solar eclipses.

The latter approach was consistent with a tradition of critical scepticism about Ptolemy's abilities as a practical astronomer and even his integrity as a reporter, which became widely accepted during the eighteenth century and was most effectively articulated by Delambre in his *Histoire de l'astronomie ancienne* [1817]. The substance of this criticism was that Ptolemy was at best an inferior and clumsy observer, that his reports of both his own and his predecessors' observations were imprecise and often ambiguous, and that his general use of only a minimal number of observations in deriving the parameters of his models reflected an unsophisticated and indeed simplistic disregard of the inevitability of observational errors. Furthermore, the instances where Ptolemy confirms parameters obtained by Hipparchus and the general agreement between Ptolemy's models and his reported observations were both considered evidence that Ptolemy either selected or altered the original observations to obtain such agreement [cf. Lalande 1757, 421–422], or even that the observations were fictitious [Delambre 1817, i xxv–xxvi]. Finally, the common assumption, first articulated by Tycho Brahe [Dreyer 1918, 349], that Ptolemy's star-catalogue was merely a plagiarism of Hipparchus' was extended by Delambre, who argued [1817, i xxv–xxix] that much of the substance of the *Almagest* was really the work of Hipparchus which Ptolemy revised and presented without proper credit.

Delambre's premise that Hipparchus was the real author of much of the *Almagest* gave him an easy explanation for many of the difficulties and inconsistencies which emerge when one examines the *Almagest* in detail. Not only did this assumption support Delambre's aspersions on Ptolemy's integrity, it also reinforced his criticisms of Ptolemy's abilities as a practical astronomer. Thus, by assuming that most of Ptolemy's results were either taken directly from Hipparchus or derived *sub rosa* from the latter's observations, Delambre could freely, if somewhat anachronistically, criticize Ptolemy's observations, his descriptions of his instruments, and, most forcefully, Ptolemy's methods for obtaining the values of his parameters.

Subsequent research has substantially qualified Delambre's estimate of Hipparchus' accomplishments and of the extent of Ptolemy's unacknowledged debt to Hipparchus [cf. Aaboe 1955, 1974; Neugebauer 1956 and 1975, 274–341; Swerdlow 1969, 1979; Toomer 1967, 1973, 1974, and 1980]. In particular, Kugler's discovery [1911, 111] that nearly all of the parameters for mean motion ascribed to Hipparchus were of Babylonian origin, destroyed most of the direct evidence supporting Delambre's thesis that Hipparchus was the superior practical astronomer and that Ptolemy depended heavily on his predecessor for his empirical results. Delambre's argument was further weakened by Vogt's [1925] careful demonstration that Ptolemy's star-catalogue could not have been simply taken from Hipparchan data with an adjustment in longitude for precession [cf. Neugebauer 1975, 280–284; Evans 1987].

The emergence of a more realistic, if still fragmentary, picture of Hipparchus' accomplishments reopens many questions about the practical astronomy of the *Almagest*, which hitherto had been conveniently answered by reference to hypothetical lost works of Hipparchus. The following study addresses some of these questions. In particular, it seeks, through an analysis of the solar and lunar observations reported in the *Almagest* and of the associated models, to gain a better understanding of both Ptolemy's abilities as a practical astronomer and of the role of observations in the development of his theory.

Specifically, in chapter 1, I examine Ptolemy's description of his determination of the obliquity of the ecliptic to ascertain whether Ptolemy could have confirmed Eratosthenes' value from real and independent observations. Since Ptolemy's (and Eratosthenes') value for the obliquity (23;51,20°) is too large by roughly 0;10°—corresponding to an error of 0;20° in the measurement of the double obliquity—it seems unlikely that independent observations should yield an identical, erroneous result. I show, however, that the peculiar motion of the Sun's shadow on the plinth, the instrument Ptolemy appears to have used, would tend to produce just the error found in Ptolemy's value if this behavior of the shadow were not taken properly into account.

In chapter 2, I investigate the solar observations Ptolemy reports and determine both the periodic and secular errors in his solar model. I first examine the observations and their errors and discuss the possible sources of systematic error in Hipparchus' and Ptolemy's observations. Here I show that both the relative accuracy of the times of Hipparchus' equinox-observations and the small systematic error in declination which they exhibit can be explained by supposing that Hipparchus knew at least one parameter of his solar model beforehand and that he used this to fix the alignment of his instrument.

Ptolemy's solar observations, in contrast, are systematically in error by roughly a day, and they do not exhibit a systematic error in declination. Previous investigators [e.g., Delambre 1817, i xxvii; Tannery 1893, 142 ff.; Fotheringham 1918, 420] have argued that the observations are too much in error and too consistent with Hipparchus' solar parameters to be independent observations. Consequently, I have investigated whether these errors could arise from a misalignment of the equatorial ring which Ptolemy appears to have used, or from the effects of refraction. Since neither effect would produce the errors found in Ptolemy's observations, I conclude that these errors could not have resulted merely from systematic observational errors. On the other hand, the irregularities in the Sun's apparent behavior which are due to refraction would have made it extremely difficult for Ptolemy to obtain a series of consistent equinox-observations, and so to make any appreciable improvement on Hipparchus' solar model by means of such observations. Thus, although it does not seem reasonable to accept

Ptolemy's solar observations as the results of careful, independent measurements, the irregularities in the Sun's apparent behavior and Ptolemy's need for solar tables to make other observations, would have given Ptolemy good reason to accept Hipparchus' solar model.

After discussing the errors in Ptolemy's and Hipparchus' solar observations, I consider whether Ptolemy's solar tables were identical with those of Hipparchus. I conclude that they were not, although Ptolemy may have used a Hipparchan equinox-observation as the basis for the epoch of the Sun's mean motion.

Finally, I determine the errors in Ptolemy's solar model. Although the secular part of this error has been analyzed by others [e.g., Kepler 1627, praec. 196; Lalande 1766, 467; Ideler 1806, 107], the influence of this error on Ptolemy's reductions of most of his other observations made it desirable to re-determine it using modern solar elements. This error, which is 1;5° in the year +135, is compounded by an additional error from Ptolemy's solar inequality of roughly ±0;25°, which must be taken into account when one compares his reduced observations with modern computations.

One of the principal problems addressed in chapters 1 and 2 is whether real observations could plausibly have agreed with previously determined but erroneous parameters and, thus, whether Ptolemy's statements about his own observations and procedures are credible. In the case of the Moon, however, the problem is quite different. In the first place, Ptolemy's own lunar observations are not clearly distinguishable from those of his predecessors, since the latter are not significantly more accurate or consistent than Ptolemy's. Secondly, although two parameters in Ptolemy's lunar model—the mean motion in elongation and the Moon's maximum latitude—are identical with those used by Hipparchus, other parameters such as the mean motions and epochs of the arguments of anomaly and latitude, the ratio of the diameter of the Moon to the diameter of the Earth's shadow, and (most probably) the radius of the Moon's epicycle, are different from those of Hipparchus. Consequently, the question of whether real observations (in the case of the Moon) could confirm a predetermined, but erroneous set of parameters does not arise. On the contrary, what is intriguing is that all of Ptolemy's lunar parameters are quite accurate, while the observations from which he derives them are often imprecise and inaccurately reduced. Accordingly, the question here is whether Ptolemy's lunar parameters were derived solely from the observations which he reports or whether some other explanation for their accuracy must be found.

Chapters 3 and 4 address this question. In chapter 3, I investigate the quality of the lunar observations which Ptolemy reports, and determine the errors in the observations themselves and in the data which result from

Ptolemy's reductions. These observations fall into three groups: lunar eclipses; occultations; and measurements of the Moon's elongation from the Sun, stars, or planets. Although not all these observations were used by Ptolemy to specify the parameters of his lunar model, I have included them as additional evidence of the quality of lunar observations in antiquity.

The eclipse-reports are the most accurate of the three groups. They exhibit an average error in the Moon's elongation of roughly ±0;6°, while that found for the measurements of elongation is around ±0;20°. The occultations show an average error in the Moon's sidereal position of around ±0;10°, and thus are somewhat less accurate than the eclipse-observations, but far more accurate than the direct measurements of the Moon's elongation. On the whole, the errors of the observations agree well with what we would expect from careful observations made with the techniques available in antiquity. Furthermore, the errors are well distributed with regard to sign and show no systematic deviation from modern computations.

In addition to observational errors, the data resulting from Ptolemy's reductions are also affected by numerous other errors, frequently of computation; and one of the purposes of chapter 3 is to determine the additional error engendered by Ptolemy's reductions. In each group of observations the average additional error from this source was found to be around ±0;4°. In the occultations, however, Ptolemy's reductions introduced a large systematic error of roughly −0;25°.

In chapter 4, I compare the parameters of Ptolemy's lunar model with their modern equivalents in order to assess the errors in Ptolemy's parameters. I then compare these errors with what we would expect from the average errors in Ptolemy's reductions of his observations and from the procedures by which he derives his parameters. For each of the eight parameters so tested, I found that Ptolemy's value is significantly more accurate than we would expect. In particular, it is striking that the actual values of Ptolemy's lunar arguments at his own time are extremely accurate, even though the methods by which he derives them do not favor observations made at his own epoch over others made at earlier epochs.

These findings strongly suggest that Ptolemy was not entirely candid in describing the procedures by which he determined his parameters, for the relatively high accuracy of each of these parameters cannot be explained satisfactorily by assuming that Ptolemy was merely lucky or that he relied on Hipparchus' results. The most plausible explanation for the accuracy of these parameters is that they were the result of some average of many determinations from a much larger number of observations than Ptolemy describes. This conclusion departs sharply from the traditional view that Ptolemy's procedures for analyzing observations and deriving the parameters of his models were quite unsophisticated. Indeed, it seems likely that the procedures he actually followed were much closer to modern procedures than has been thought.

The question remains, Why should Ptolemy have described procedures for determining his parameters less sound than those which he actually employed? While it is impossible to answer such a question with certainty, it is my view that the *Almagest* was not intended to be a historical account but rather a pedagogical treatise. In general, Ptolemy takes great care to make his demonstrations and determinations conform as nearly as possible to the standards of logical rigor encountered in Greek mathematics. Hence, he may reasonably have concluded that the interests of clarity and rigor were better served by examples of how his results were obtained than by a lengthy, and necessarily non-rigorous, discussion of his procedures for obtaining parameters from discordant observations.

One corollary to this conclusion is that Ptolemy almost certainly selected the observations which he reports because they yielded just the values of parameters which he wished to demonstrate. This is not to say that Ptolemy tampered with the reports of the observations or that he made intentional errors in their reduction and analysis. Indeed, he would have had no need to do so, since among a large number of determinations a few could be expected to illustrate almost any desired value for a parameter (as long as this value was approximately correct).

Since the investigations described above draw heavily on comparisons of Ptolemy's observations with modern theory, it seemed desirable to use a consistent set of lunar and solar elements throughout instead of the variety of elements used by previous investigators. Consequently, I have adopted the elements used by the Nautical Almanac Office [1961, 98, 107] with two modifications. These modifications affect only the apparent secular accelerations of the Sun and Moon, the modern values for which I found to be based on an erroneous analysis by de Sitter [1927]. In appendix 1, I discuss previous determinations of these parameters and derive the revised values which I have used throughout this work. In appendix 2, I give those corrections which reduce the elements used by earlier investigators to the elements I have adopted.

Since this study was first completed in 1966, our resources for understanding Ptolemy and the *Almagest* have been importantly affected by four major works. These include two extensive commentaries on the *Almagest*, one by Olaf Pedersen in 1974 entitled *A Survey of the Almagest*, and the other by Otto Neugebauer in 1975 as part of *A History of Ancient Mathematical Astronomy*. The primary objective of both Neugebauer's and Pedersen's commentaries is to describe the methods, models, and functions which Ptolemy employs in the *Almagest*, with emphasis on textual and internal evidence and a few comparisons with calculations from modern astronomy. Consequently, most of the substance of the present study is not duplicated in either work.

A third major resource for understanding Ptolemy and the *Almagest* is Gerald Toomer's superb English translation, *Ptolemy's Almagest*. Prior to its publication in 1984, the best modern translation was Manitius' [1912-1913]; Halma's French translation [1813–1816] and Taliaferro's mediocre English translation [1952] both suffer from textual and interpretative inadequacies. Apart from being the first good English translation of the *Almagest*, Toomer's version also comes with several hundred (noted) corrections to Heiberg's text, with the result that we now have as secure a text as we are likely to. Moreover, Toomer has extensively annotated the translation and includes accurate values for many calculations, thus supplementing the commentaries by Pedersen and Neugebauer.

These works by Neugebauer and Pedersen and Toomer have substantially enhanced both our resources for understanding Ptolemy and our appreciation of his accomplishments as an astronomer, mathematician, and author of the *Almagest*. Contemporaneously, R. R. Newton published several books which attempt to prove an extreme and opposite conclusion. This is that Ptolemy was at best a mediocre astronomer [1977, 364] who fabricated all but a few of the observations reported in the *Almagest* [1977, 344–346, 364, 378], thereby committing an elaborate fraud resulting in the destruction of much 'valid Greek astronomy' [1977, 362]. Consequently, Newton concludes [1977, 379] that the *Almagest* 'has done more damage to astronomy than any other work ever written'.

This is 'the crime of Claudius Ptolemy' that Newton alleges at length in his book of that title [1977], a crime said to have been 'committed by a scientist against his fellow scientists and scholars,... that has forever deprived mankind of fundamental information about an important area of astronomy and history' [1977, xiii]. This book expands upon several earlier works [Newton 1970, 1973, 1974a–b, 1976] published in connection with efforts to determine the effective accelerations of the Sun and Moon. In turn it is followed by two related works [Newton 1982, 1985a–b] which discuss the origins of Ptolemy's parameters and tables, and which further amplify Newton's thesis that 'Ptolemy was the most successful fraud in the history of science' [1977, 379].

Newton's work has been widely criticized—see, e.g., Swerdlow's excellent review [1979] of *The Crime of Claudius Ptolemy* [1977]—and, indeed, it affords ample ground for criticism. In general, it is a biased and unrelieved polemic against Ptolemy, composed in a vexatious and intemperate style (as may be sensed from the foregoing quotations), and it contains many errors of fact and comprehension, as well as inconsistencies in both argument and logic. Indeed, were it not for the sheer scope of Newton's work, the zealous energy it reflects, and the emotional language it employs, I suspect that few would have paid it much heed. As it is, however, Newton's work has come to represent a counterview of Ptolemy's contributions which has proven difficult to dislodge.

I have not attempted, nor is there space, to present a critical analysis of Newton's work here. In general, I think that his main conclusion with respect to Ptolemy's stature and achievements as an astronomer is simply wrong, and that the *Almagest* should be seen as a great, if not the indeed the first, scientific treatise. Furthermore, I am inclined to wonder if Newton's unrelenting animus towards Ptolemy may not arise from the fact that the observations in the *Almagest* do not support the (anomalous) accelerations that Newton [1969, 1970, 1972, 1979–1984] finds from other data and seeks to have accepted. Finally, I note from errors scattered throughout Newton's work, that he has relied extensively on Halma's Greek text by way of Halma's or Taliaferro's translations, and that he evidences little familiarity with either Manitius' superior translation or Heiberg's far superior text. While consistent with Newton's acknowledgment [1985, 53] of having 'small Latin and less Greek', this contrasts with his purported methodology and raises a small but important question regarding his own candor.

For all its deficiencies, however, Newton's work does focus critical attention on the many difficulties and inconsistencies apparent in the fine structure of the *Almagest*. In particular, his conclusion that the *Almagest* is not a historical account of how Ptolemy actually derived his models and parameters is essentially the same as mine, although our reasons for this conclusion and our inferences from it differ radically.

In revising this study, I considered how best to treat Newton's work, which raises important issues regarding virtually all the material discussed here. In the end, I decided that to address these issues directly would substantially change both the character and scope of this work, a work completed some years before Newton's first publications on these subjects. Consequently, I have purposefully omitted what would otherwise have been extensive references to Newton's writings, preferring that this study address Ptolemy rather Newton.

In preparing this work for publication, I have made a number of changes to the 1966 text. The most important of these is the replacement of all previous translations of passages from the *Almagest* with Toomer's translations. Generally, this has improved the clarity and consistency of such extracts, but in no instance has it changed the substantive conclusions relating to any given passage. Other changes include corrections of calculations and the updating of notes and references to acknowledge relevant material published since 1966. This resulted in some expansion of the material on the secular accelerations [appendix 1], though its principle conclusion remains the same. Finally, I have made minor revisions throughout the text in the hope of improving its clarity. The net effect of these changes is modest in scope and either technical or literary in nature. Thus, the findings and conclusions of this study are essentially unchanged from those in my original dissertation.

New Haven, Connecticut
June, 1992

Models and Precision: The Quality of Ptolemy's Observations and Parameters

If Ptolemy in continuing the same observations nearly 300 years after Hipparchus, had been content to publish a general history, if he had not moreover changed the positions of the stars in the Catalogue, and instead of establishing the elements of the movement of the planets with the aid of hypotheses and from a small number of observations, had he discussed and collected faithfully all that which could be brought to bear on the mean motions, the nodes, the inclinations, the aphelia, and the eccentricities or greatest equations of the orbits of the planets, it is certain that astronomy would be much further advanced than it is today, and we would know the laws of the celestial motions much better at present. But he was less interested in rendering his *Almagest* or *Syntaxis* useful to astronomers than in making it available to the ordinary man and the calculators. And since the true way to perpetuate this sort of work is to annihilate all the observations which would be contrary to it, it has happened that except for only the observations which he was obliged to use in the construction of his tables, the other astronomical observations have been lost.

Le Monnier

Ptolemy and the Obliquity of the Ecliptic

Ptolemy's determination of the obliquity of the ecliptic [*Alm.* i 12] illustrates some of the problems one encounters in trying to understand the interplay between the observations and the parameters Ptolemy adopts in the *Almagest*. The determination is straightforward and requires no previously developed theory for the reduction of the observations. If z_w is the noon zenith-distance of the Sun at winter solstice [see Figure 1.1], and z_s the noon zenith-distance at summer solstice, then

$$z_w + z_s = 2\phi$$
$$z_w - z_s = 2\epsilon,$$

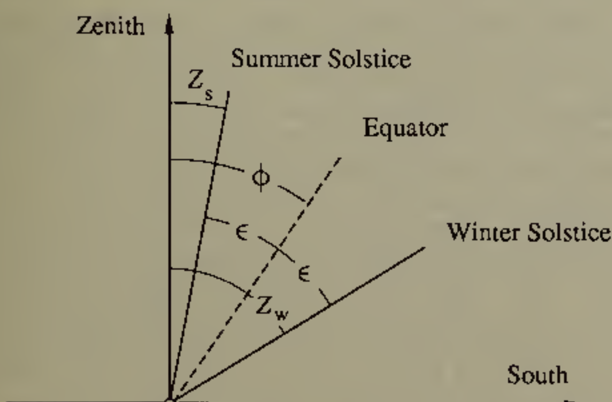

Figure 1.1

where ϕ is the latitude of the place of observation and ϵ is the obliquity.

After describing the procedure for finding the zenith-distance of the Sun on the meridian by means of a plinth or quadrant [see Figure 1.2], Ptolemy writes [*Alm.* i 12: Toomer, 63]:

> From observations of this kind, and especially from comparing observations near the actual solstices, which revealed that, over a number of returns [of the Sun], the distance from the zenith was in general the same number of degrees of the meridian circle at the [same] solstice, whether summer or winter, we found that the arc between the northernmost and southernmost points, which is the arc between the solstitial points, is always greater than $47\,{}^2/_3{}^\circ$ and less than $47\,{}^3/_4{}^\circ$. From this we derive very much the same ratio as Eratosthenes, which Hipparchus also used. For [according to this] the arc between the solstices is approximately 11 parts where the meridian is 83.

Three points in this passage are noteworthy. First, Ptolemy states explicitly that the angle measured was the zenith-distance and not the altitude. Second, he reports that the zenith-distances measured were nearly always the same and thus denies a solar motion in latitude.[1] Finally, Ptolemy says, in effect, that from several years of his own observations he confirmed Eratosthenes' value for the obliquity,

$$2\epsilon = \frac{11}{83} \cdot 360^\circ = 47;42,39,2\ldots^\circ$$
$$\epsilon = 23;51,20^\circ,$$

which is the value Ptolemy [*Alm.* i 15] adopts in his table of declinations.

---

[1] According to Theon of Smyrna [Dupuis 1892, 289, 313], Eudoxus made the Sun move on a sphere whose axis was inclined to that 'through the middle of the [zodiacal] signs'. Theon says further that this inclination was ${}^1/_2{}^\circ$, so that the maximum altitude of the Sun at summer solstice, for example, could vary by as much as 1°. In his *Commentary to the Phaenomena of Aratus and Eudoxus* [Manitius 1894, 88], Hipparchus discusses this question and remarks that Attalus and other contemporary mathematicians affirmed the existence of a solar motion in latitude. Hipparchus asserts that such a motion is impossible, since the discrepancy between computed and observed eclipse-magnitudes was seldom found to be greater than 2 digits or 0;5°. Despite Hipparchus' argument, the notion that the Sun exhibited a periodic deviation from the mean ecliptic remained current at Ptolemy's time, as witnessed by Theon of Smyrna [Dupuis 1892, 211, 223, 279, 289, 313]. Indeed, in spite of Ptolemy's denial, Martianus Capella maintained it as late as the 5th century AD. See Dreyer 1906, 94–95, for a summary of the several variants of this theory and further references.

In contrast, the modern value for the obliquity at Ptolemy's time (+130) is

$$\epsilon = 23;40,46^\circ \quad \text{so}$$
$$2\epsilon = 47;21,32^\circ.$$ [2]

The error in (correction to) the value adopted by Ptolemy is, therefore, $-0;10^\circ$, while the error in the angle actually measured is $-0;21^\circ$. Since the latter error is four times greater than the precision claimed by Ptolemy ($\pm 0;5^\circ$), and since Ptolemy's results confirm almost exactly the earlier value obtained by Eratosthenes, it is natural to ask if Ptolemy could have obtained his result from careful, independent observations.[3] Is it plausible that a careful observer, following the procedures described by Ptolemy, could have consistently found Ptolemy's limits without adapting his observational procedures to yield the predetermined result?

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[2] The modern expression for the obliquity of the ecliptic [Nautical Almanac Office 1961, 81], epoch 1900.0, is $23;27,08.26^\circ - 46.845''T - 0.0059''T^2 + 0.00181''T^3$. In year +130, ($T = -17.7$), its value is $23;40,46^\circ$; in -140, it is $23;42,46^\circ$.

[3] Cf. Delambre 1871, i 86, ii 75. When Delambre wrote his *Histoire de l'astronomie ancienne*, the rate of change of the obliquity (and, thus, the value of the obliquity at Ptolemy's time) was still uncertain. Delambre notes that Ptolemy uses Eratosthenes' value in his tables of declination rather than the mean of the limits which Ptolemy claimed to have found; and this is also remarked by Manitius [1912–1913, i 44n*b*]. This fact is, of course, irrelevant to the question of whether Ptolemy re-determined the obliquity, since the difference, $0;0,5^\circ$ is far too small to warrant changing an apparently satisfactory value.

A more significant question was raised by Berger [1880, 131] who pointed out that the text is ambiguous concerning the value of the obliquity used by Eratosthenes and Hipparchus. Berger suggests that Eratosthenes and Hipparchus used 24° for the obliquity and that the ratio, 11:83, was Ptolemy's invention. If Berger is correct, then the question of how Ptolemy confirmed the value of his predecessors of course vanishes. Berger's argument, however, seems weak for two reasons. One is that Theon of Alexandria [Rome 1936–1943, ii 52, 528–529] states that Eratosthenes discovered the value $^{11}/_{83}$ of a circle for the double obliquity. Theon may have had no further information than that given in the *Almagest*, and thus his testimony is not conclusive. Nevertheless, his statement seems to deserve some weight. Secondly, if the value, $23;51,20^\circ$ ($= {}^{11}/_{83} \cdot 180^\circ$), did not originate with Eratosthenes, it is difficult to understand why Ptolemy did not merely take the mean between his observed limits for the double obliquity, $47;42,30^\circ$, corresponding to an obliquity of $23;51,15^\circ$.

For further discussion, see also Tannery 1893, 119–120; Toomer, 63n75; Goldstein 1983.

Ptolemy describes two instruments for making these observations, a meridional armillary and a plinth or quadrant.[4] It appears, however, that he used only the plinth and not the meridional armillary. This is suggested by two facts. First, Ptolemy begins his description of the construction and use of the plinth [Heiberg 1898–1903, i 66.5] with the statement, ἔτι δὲ εὐχρηστότερον ἐποιούμεθα... ('even more conveniently, however, we made...'). In contrast, he begins his description of the meridional armillary [Heiberg 1898–1903, i 64.12] by saying in the future tense, ποιήσομεν γὰρ κύκλον χάλκεον... ('we shall make a bronze ring...'). Second, both in the statement quoted above describing his results and in his description of the plinth, Ptolemy explicitly mentions 'marks' indicating the midpoint of the Sun's shadow. In describing the meridional armillary, however, he says only that the zenith-distance can be read directly from the scale. Accordingly, I shall consider here only the problems which Ptolemy might have encountered in making such observations by means of a plinth.[5]

First, we should note that the precision claimed by Ptolemy ($\pm 0;5^\circ$) is consistent with what can be achieved with a plinth of moderate size. Ptolemy gives no indication of the size of either of the two instruments he describes; he simply states that the scales on each instrument should be divided into integer degrees and their subdivisions. Proclus [Manitius 1909, 43 ff.] does not discuss the plinth but does describe the construction of the meridional armillary, which he says should be 'not less than half a cubit in diameter'.[6] Proclus adds, however, that the scale on such an instrument should be subdivided to $0;1^\circ$. This would have been impossible on such a small instrument,[7] and we are left to wonder how much Proclus actually knew about such instruments. Pappus [Rome 1931–1943, i 6] describes a 'meteorscope' similar in construction to Ptolemy's armillary astrolabe, whose diameter, Pappus says, was equal to one cubit. Finally,

[4] See Dicks 1954, 78–79 and Price 1957, 587–589, for a discussion and description of these two instruments.

[5] Tannery [1893, 119–120] observed that Ptolemy's determination was actually made on the plinth, which Tannery describes as more convenient but less accurate than the *scaphe* that he supposes Eratosthenes to have used.

[6] According to Hultsch [1889, 200–203], the Egyptian cubit was 525 mm. (= 20.6 in.) and the Roman cubit, 443.6 mm. (= 17.2 in.). The range in size of the instruments mentioned by Proclus, Pappus, and Theon is, therefore, from 9 in. to 41 in.

[7] See Dicks 1954, 77–85 and Price 1957, 582–619, for a discussion of the magnitude of possible subdivisions on instruments of different sizes in antiquity. Both authors give $0;5^\circ$ as the subdivision recommended by Proclus. The text, however, gives $0;1^\circ$: cf. Manitius 1909, 44–45; Halma 1813–1816, 79.

Theon [Rome 1931–1943, ii 819–820] notes that an equatorial ring of his day had a diameter of two cubits.

These later descriptions afford us no certain information about the size of Ptolemy's plinth, but they do suggest the order of magnitude of graduated instruments in antiquity. If we assume that subdivisions much smaller than a millimeter were impractical, then a scale graduated to 0;15° would require a radius of roughly half a cubit (9–10 in.), while subdivisions of 0;5° would require a radius of $1^1/_2$ cubits (27–30 in.). Though it is possible that Ptolemy's plinth was graduated to 0;5°, it seems more likely that it was graduated to 0;10°, or possibly to 0;20°, from which readings might be estimated to halves or quarters of a division.[8] In either case an error of 0;20° should lie well outside the limits of instrumental precision. Thus, considerations of precision alone suggest that Ptolemy should have been able to improve upon Eratosthenes' value for the obliquity and to have obtained a value of ϵ accurate to within ±0;5°.

A second question is whether using a plinth to determine the obliquity would tend to produce values systematically greater than those found from accurate observations. Since the shadow on the plinth loses definition as the Sun crosses the meridian, I shall first consider what should be observed exactly at noon and, then, how the shadow moves in the interval just before noon, when readings could have been made more easily. Assuming for the latitude of Alexandria $\phi = 31;12°$,[9] we find the following apparent (corrected for refraction) noon zenith-distances of the Sun at summer and winter solstice for the year +130:

$$z_s = 7;31^\circ$$
$$z_w = 54;51^\circ.$$

In contrast, using Ptolemy's value for the latitude of Alexandria [*Alm.* v 12], $\phi' = 30;58°$ and his value for the obliquity ($\epsilon = 23;51,20°$), we obtain the following noon zenith-distances (z'):

$$z'_s = 7;\,6,40^\circ \qquad \approx 7;\,5$$
$$z'_w = 54;49,20^\circ \qquad \approx 54;50.$$

[8] See Vogt 1925, 40–42, for a discussion of the possibility that Ptolemy's armillary astrolabe may have been graduated at 0;20° and read to 0;10°.

[9] P. V. Neugebauer [1929, ii 101] gives 31.2°. Dreyer [1906, 176] states that the latitude of the Museum at Alexandria was 31;11,7°, but gives no source for this statement. Since it is not known where in Alexandria Ptolemy made his observations, it is possible that the value 31;12° may be high by one or two minutes of arc. See Lalande 1766, 496; Chazelles 1761, 172.

Ptolemy does not say explicitly that he derived his value for the latitude of Alexandria from such observations; he merely comments that it is easy to determine the latitude of any place from such observations. (Indeed, the identity of his value for the latitude of Alexandria with that implicit in the crude ratio of 5:3 between the length of a gnomon and its equinoctial shadow at noon,[10] suggests an alternative source for this parameter.) Nevertheless, assuming a precision of 0;5°, the above values are the only possibilities consistent with Ptolemy's value for the latitude of Alexandria, and so are most probably the zenith-distances he actually observed. If so, Ptolemy's determination of the Sun's zenith-distance at winter solstice was essentially accurate, and the error in his value for the obliquity arose solely from the error in his measurement of the Sun's zenith-distance at summer solstice.

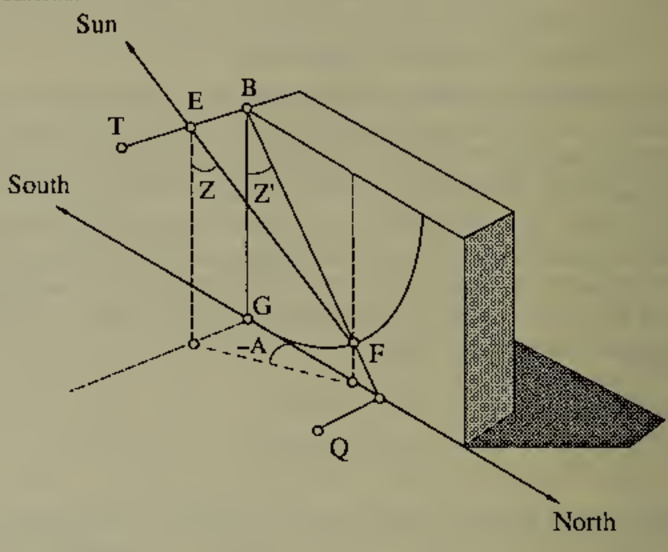

Figure 1.2. Ptolemy's Plinth as Seen from the Northeast

Consider next the movement of the Sun's shadow on the plinth as the Sun approaches the meridian. In Figure 1.2, BT represents a small cylinder parallel to the horizon and perpendicular to the plane of the meridian, whose shadow, BFQ, intersects the scale of the plinth at F. The face of the plinth is in the plane of the meridian and the line BG perpendicular to the horizon. The Sun's actual zenith-distance is denoted by z and its azimuth by $-A$. Finally z' $(= \angle GBF)$ is the angle which would be read

[10] The ratio 5:3 between the gnomon and its equinoctial shadow at Alexandria was attributed to Hipparchus by Strabo. See Dicks 1960, 95, 174.

off the plinth when the Sun was at zenith-distance z, and azimuth $-A$.[11] In what follows, I shall refer to z' as the apparent zenith-distance (on the plinth). At noon, when the Sun crosses the meridian, $z'(0) = z(0)$.

The problem I wish to investigate is:

> Given the Sun's declination (δ), its hour-angle (t), and the latitude of the place of observation (ϕ), what is the difference between the noon zenith-distance of the Sun and the apparent zenith-distance measured on the plinth at t, i.e., $z(0) - z'(t)$?

First, consider Figure 1.2 and observe that

$$\tan z' = \tan z \cdot \cos A, \tag{1}$$

where [see Smart 1962, 35]

$$\sin z \cdot \cos A = -\cos\phi \cdot \sin\delta + \sin\phi \cdot \cos\delta \cdot \cos t, \text{ and} \tag{2}$$

$$\cos z = \sin\phi \cdot \sin\delta + \cos\phi \cdot \cos\delta \cdot \cos t. \tag{3}$$

If we now let

$$m \cdot \sin M = \sin\delta, \tag{4}$$

$$m \cdot \cos M = \cos\delta \cdot \cos t, \tag{5}$$

and substitute for $\sin\delta$ and $\cos\delta \cdot \cos t$ in (2) and (3), we obtain

$$\begin{aligned} \tan z \cdot \cos A &= \tan(\phi - M) \\ &= \tan z', \end{aligned} \tag{6}$$

whence

$$z'(t) = \phi - M(t). \tag{7}$$

From (4) and (5),

$$M(t) = \arctan\left(\frac{\tan\delta}{\cos t}\right). \tag{8}$$

Since the noon zenith-distance is

$$z(0) = \phi - \delta, \tag{9}$$

[11] Here azimuth is counted from the southern meridian, and is considered positive to the west and negative to the east.

our desired quantity is

$$
\begin{aligned}
z(0) - z'(t) &= M(t) - \delta \\
&= \arctan\left(\frac{\tan\delta}{\cos t}\right) - \delta.
\end{aligned}
\tag{10}
$$

Interestingly, $z(0) - z'(t)$ is independent of ϕ; that is, the error in the Sun's apparent zenith-distance observed on a plinth some time before noon is the same for all places of observation. More importantly, (10) shows that for

$$
\begin{array}{ll}
0^\circ < \delta < 90^\circ & z'(0) > z'(t) \\
0^\circ = \delta & z'(0) = z'(t) \\
0^\circ > \delta > -90^\circ & z'(0) < z'(t).
\end{array}
\tag{11}
$$

Thus, when the Sun is north of the equator $z'(t)$, the apparent zenith-distance, reaches a *maximum* at noon ($t = 0^\circ$), whereas when the Sun is south of the equator, $z'(t)$ is a *minimum* at noon. The behavior of the shadow on the plinth when the Sun has a positive declination is just the reverse of what we might intuitively expect, since the Sun's actual zenith-distance has, of course, always a minimum at noon.

The error in the value of the obliquity which arises from accurate measurements made at summer and winter solstice some time (t) before noon, is determined as follows. For

$$\delta = \pm\epsilon,$$

we have from (10)

$$
\begin{aligned}
\epsilon_{obs} &= \frac{1}{2}[z'(t)_w - z'(t)_s] \\
&= \arctan\left(\frac{\tan\epsilon}{\cos t}\right).
\end{aligned}
\tag{12}
$$

The error in the obliquity from such a determination is, therefore,

$$\epsilon - \epsilon_{obs} = \epsilon - \arctan\left(\frac{\tan\epsilon}{\cos t}\right). \tag{13}$$

Hence, the effect of making either observation somewhat before noon is to make the measured obliquity greater than the true obliquity. Since this is the direction of the error in Ptolemy's value for the obliquity, it is possible, then, that Ptolemy's error of $-0;10^\circ$, arose in this way.

Table 1.1 shows the error, $\epsilon - \epsilon_{obs}$, which would result from accurate observations at winter and summer solstice in Alexandria T minutes before noon. This error is numerically the same as the error in a single observed zenith-distance at either solstice measured at that time. (The sign of the error at summer solstice is positive.) An error in the obliquity of $-0;10°$ would arise from two observations made nearly 30 minutes before noon, or from a single observation made roughly 40 minutes before noon. At summer solstice in Alexandria 40 minutes before noon, for the shadow to reach the scale of the plinth, the cylinder which casts the shadow would have to be greater than $0.25r$, where r is the radius of the scale of the plinth, i.e., greater than 5 inches if $r = 1$ cubit. This requirement does not seem unreasonable, and we can safely assume that readings could have been made at this time.

Minutes before Noon	$\epsilon - \epsilon_{obs}$ (0;1°)	Summer z_s	Summer $-A_s$	Winter z_w	Winter $-A_w$	Distance to Meridian
0	0.0	7.52°	0.00°	54.88°	0.00°	0.00°
−20	−4.8	8.73	31.73	55.09	5.59	4.58
−30	−10.9	10.04	43.31	55.35	8.36	6.87
−40	−19.5	11.62	52.13	55.71	11.10	9.15
−60	−44.3	15.27	64.17	56.73	16.47	13.71

Table 1.1

The error in Ptolemy's value for the obliquity can thus be accounted for by assuming that he made his observations roughly half an hour before noon. If, on the other hand, the error arose primarily from the observations at summer solstice, as seems implied by Ptolemy's value for the latitude of Alexandria, then the time before noon required to produce this error is around 40 minutes. This seems a rather long time, although the Sun is already very near the meridian then. Moreover, this assumption fails to explain Ptolemy's apparently accurate determination of the Sun's zenith-distance at winter solstice.

An alternative, and to my mind preferable, explanation is that Ptolemy made his determination some time before noon and estimated the progress of the shadow in the interval to noon. If in so doing, he extrapolated the wrong way at summer solstice (which would be a natural mistake), the error in the determination at summer solstice would be twice the estimated correction. Thus, if Ptolemy made accurate observations roughly half an hour before noon and assumed that the shadow moved 0;10° further in the direction of decreasing zenith-distance during the interval to noon, his results would be in error by just the amounts which we find.

This explanation is neither altogether satisfactory nor conclusive. For his part, Ptolemy mentions the difficulty of observing the shadow at noon [*Alm.* i 12: Toomer, 63] and says that he placed something at the edge of the scale to make the shadow visible. Theoretically, this procedure would obviate the difficulty and would allow him to observe the shadow just as the Sun crossed the meridian. In any event, the preceding explanation of the possible origin of Ptolemy's error requires us to assume that Ptolemy's actual procedure was slightly different from what he describes, for no other plausible source of systematic error comes to mind which would tend to produce consistently high values for the obliquity.[12]

A different explanation, which Delambre [see 3n3, above] and other critics of Ptolemy have favored, is that Ptolemy's entire description of his determination is an elaborate misrepresentation, and that his observed limits for the double obliquity are either imaginary or the result of careless efforts to confirm Eratosthenes' value for the obliquity. This, however, seems even less satisfactory than the explanation offered above for several reasons.

First, it ignores the fact that even after Hipparchus, some astronomers upheld the theory that the Sun possessed a perceptible motion in latitude [see 2n1, above], so that for theoretical reasons Ptolemy would have been concerned to establish the constancy of the Sun's extreme altitude from his own observations. Second, Delambre's explanation requires that we assume a highly contrived and unlikely distortion by Ptolemy. For, if Ptolemy did not determine the obliquity from his own observations and, instead, merely accepted the value of Eratosthenes, it is difficult to understand why he should have bothered to describe two instruments for determining it, to

[12] The most obvious other sources of systematic error are graduation-error or an error in centering the cylinder which casts the shadow. The first should least affect observations at summer solstice, when the Sun's zenith-distance is small, whereas it appears that this observation was the one most seriously in error. Concerning the second, it can easily be shown that the apparent error in the Sun's zenith-distance at summer solstice would require an error of 1 inch in the lateral positioning of the cylinder on a plinth of radius 1 cubit. Such an error seems far too large to have been possible.

Another possible source of error is that the plinth was not accurately aligned in the plane of the meridian. To produce the observed error in the obliquity, the azimuth of the plinth would have to be $+12^\circ$ at both solstices. For the error to arise from the determination at summer solstice alone, the azimuth of the plinth at summer solstice would have to be roughly $+17^\circ$.

indicate which of these he actually used, and to state the limits he found for the observed arc.

On balance, therefore, it seems that we should withhold judgment on whether Ptolemy actually determined the obliquity as he said he did, since it is quite possible that the systematic errors discussed above affected his determination. Furthermore, the evidence we have suggests that one of the two observed limits for the Sun's zenith-distance was indeed quite accurate, while the other may have been distorted by the peculiar behavior of the Sun's shadow on the plinth in summer.

2

Observations of Solar Position and Ptolemy's Solar Model

Ptolemy mentions a total of 28 observations of solstices and equinoxes. Four of these are his own, and the rest are taken from two works by Hipparchus, *On the Changes of the Solstitial and Equinoctial Points* and *On the Length of the Year* [*Alm.* iii 1: cf. Table 2.1 for dates and times]. The latter observations concern two summer solstices, one observed by 'the school of Meton and Euctemon' (−431) and one by Aristarchus (−279), and a spring equinox (−145) observed at Alexandria. The remainder is comprised of a summer solstice observed by Hipparchus in −134, six fall and three spring equinoxes which Hipparchus designated 'very accurately observed', and eleven spring equinoxes which are described as agreeing with the other three in accordance with the $^1/_4$-day surplus. The gap between −140 and −134 in what is otherwise a complete series of spring equinoxes suggests that these eleven equinoxes were in fact observed, and I have therefore included them in the discussion.[1]

[1] According to Ptolemy [*Alm.* iii 1: Toomer, 139], Hipparchus discusses the solstices observed by Aristarchus (−279) and himself (−134) in his work, *On the Length of the Year*, which is probably also Ptolemy's source for the report of the solstice of −431. Ptolemy gives only the year in which Aristarchus' and Hipparchus' solstices were observed. Thus, we have no direct evidence that Hipparchus found the solstice of −134 to have occurred $94^1/_2$ days after the spring equinox of that year. This date, however, is the only one consistent with the time of the solstice of Meton and Euctemon and Hipparchus' value for the length of the year. Since Ptolemy notes that Hipparchus computed the interval between the solstice of Meton and Euctemon (−431) and that of Aristarchus (−279) as well as that between the solstice of −279 and −134, it is probable that all three were consistent with his value for the length of the year. I assume, therefore, that

Ptolemy's four observations include one spring equinox (+140), two fall equinoxes (+132, +139), and a determination of the summer solstice in +140. He tells us [*Alm.* iii 7] that the earlier fall equinox was 'one of the most accurately determined', and that it was 'among the first of the equinox observed by us'. He also says [*Alm.* iii 1: Toomer, 138] that he observed 'very securely' the other fall equinox, which he compares with the one Hipparchus observed in −146 to verify the length of the year. In contrast, Ptolemy says only that he found the following spring equinox to have occurred at the stated time. Finally, he reports that he 'determined securely' and 'as accurately as possible' that the summer solstice of +140 occurred about 2 hours after midnight.

Ptolemy does not state explicitly what instruments or measurements either he or Hipparchus used to determine the equinoxes and solstices, but only indicates the observations in which he and Hipparchus had greatest confidence. There is some evidence, however, that the two men used different methods to find the times of the equinoxes.

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the times (using a midnight epoch) of the two solstices were: −279 Jun 26 18^h (Aristarchus) and −134 Jun 26 12^h (Hipparchus). For a discussion of Meton's solstice-observation, see Bowen and Goldstein 1988.

The equinoxes listed as accurately observed are:

Spring Equinox	Fall Equinox
	−161 Sep 27 18^h
	−158 Sep 27 6
	−157 Sep 27 12
	−146 Sep 27 0
−145 Mar 24 6^h	−145 Sep 27 6
	−142 Sep 26 18
−134 Mar 24 0	
−127 Mar 23 18	

The spring equinoxes are all consistent with each other and with the $^1/_4$-day surplus. The fall equinoxes are less consistent, and are observed progressively earlier than would accord with the $^1/_4$-day surplus. Thus, the equinoxes of −158, −146, and −142 were each observed 6^h earlier than would be expected from the preceding equinox, while those of −157 and −145 agree with the preceding equinoxes.

In discussing the $^1/_4$-day discrepancies in the fall equinoxes reported by Hipparchus,[2] Ptolemy remarks that an error of this amount would arise 'if the placing or division of the instruments deviated from exactness by only one 3600$^{\text{th}}$ part of the circle [of declination]', (i.e., by 0;6°). This implies that Hipparchus used a graduated instrument similar to the meridional armillary described in book i 12. With such an instrument the Sun's declination could be determined from its meridian-altitude and the latitude of the place of observation (or read directly from the scale if the equator were marked on the instrument). A meridional armillary with a diameter of 1 cubit ($\approx$ 18 in.) could have been graduated to $^1/_5$° (0;12° = 0.85 mm.),[3] or twice the amount Ptolemy mentions. Subdivisions of $^1/_5$° would also mean that near the equinoxes the Sun's declination would change just two divisions a day. This would permit the times of the equinoxes to be estimated to the nearest $^1/_4$ day from successive observations either before or after the equinox, while any greater precision would require a considerably larger instrument.

Hipparchus' report [*Alm.* iii 1] of the spring equinox of −145 also suggests that he measured the Sun's declination directly to determine the equinoxes, rather than using an equatorial ring. For he says he found that the equinox occurred at dawn, but that the ring at Alexandria was illuminated equally from both sides at about the fifth hour (of the day), so that the 'same equinox, differently observed, was found to differ by nearly five hours'.[4]

[2] See 12n1 above, for a list of discordant equinoxes. Ptolemy [*Alm.* iii 1: Toomer, 135] says that Hipparchus found from eclipses that the magnitude of suspected inequality in the length of the year was not greater than $^3/_4$ day. This is the amount by which the fall equinox of −142 differs from that of −161 assuming a tropical year of 365$^1/_4$ days, and is also equal to the maximum error due to refraction for equinoxes observed on an equatorial ring.

[3] See chapter 1 for a discussion of the possible dimensions and graduations of ancient instruments. Rome [1937–1938, 218] notes that Theon 'admits' graduations of 0;5° on a meridional armillary, which Rome points out would correspond to 3 divisions *per* millimeter on a scale 1 cubit in diameter.

[4] Considerable confusion has surrounded Hipparchus' report of this equinox (−145 Mar) and its relation to the rest. Delambre [1817, i xxiii] interprets the passage [*Alm.* iii 1: Toomer, 134] to mean that the ring at Alexandria showed the equinox first at dawn and again at the fifth hour, but he concludes that Hipparchus observed the equinox at Rhodes at dawn, and that this report was secondhand. Tannery [1893, 149] refers to the 'double determination' and concludes that this equinox was part of a series which included the three fall equinoxes of −161, −158, and −157. Tannery thinks that these equinoxes were all observed at Alexandria, by someone other than Hipparchus. Fotheringham [1918, 408] disputes this conclusion, and also shows that there is no evidence that Hipparchus

Furthermore, Hipparchus' statement [Toomer, 133] that the variation in the year-length could be seen from equinoxes observed on the ring in the Square Stoa at Alexandria also implies that he did not use such an instrument for the observations he reports.

It seems likely, then, that Hipparchus determined the times of the equinoxes from direct observations of solar declinations: that he observed them we know from Ptolemy [*Alm.* vii 3] and his own commentary on the *Phaenomena* of Aratus and Eudoxus. Moreover, seven of Hipparchus' eighteen reported determinations of the declinations of stars [*Alm.* vii 3] are quoted in $^1/_5{}^\circ$, and the others could readily have been made on an instrument so graduated.[5] This further suggests that Hipparchus may have possessed an instrument with graduations of 0;12°, which, as we have seen, would have been most convenient for finding the times of the equinoxes.

The other instrument for this purpose mentioned in the *Almagest* is a ring set in the plane of the equator. Hipparchus' report of an equinox observed on such a ring at Alexandria (−145) and his statement in *On the Displacement of the Solstitial and Equinoctial Points* [*Alm.* iii 1: Toomer, 132–133] that such a ring, made of bronze, was placed in the 'place called the Square Stoa' at Alexandria, have already been cited.[6]

Ptolemy [*Alm.* iii 1: Toomer, 134] also refers to at least two metal rings, in the same passage in which he discusses how errors in the alignments of

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made his observations in Alexandria. Rome [1937–1938, 230] points out that the text does not imply that this equinox appeared twice on the ring at Alexandria. He also notes [1931–1943, 817] that Theon understands this passage to mean that Hipparchus observed the equinox at Rhodes at dawn, and that someone else observed it at Alexandria at 11 hours. Thus, there is no evidence that Hipparchus did not observe the equinoxes which he reports at Rhodes.

[5] Vogt [1925, 19] states that *In Arat.* includes more than 40 designations of declinations not mentioned in the *Almagest*. Of the declinations determined by Hipparchus and cited in the *Alm.* vii 3 [Toomer, 331], four are given in integer degrees, one in half degrees, two in thirds of a degree, two in quarters of a degree, seven in fifths of a degree, and two in sixths of a degree. Thus, if we include the declinations given in integers, nearly two thirds of the total can be accounted for by assuming a scale subdivided to 0;12°. The remaining fractions can be explained if we assume that they were read as 'a little more than' or 'a little less than' a certain division. Although the distribution of fractions other than fifths is not quite symmetrical, it is difficult to imagine another subdivision in which it would be natural to estimate positions between the divisions to fifths of a degree.

[6] Cf. Rome1937, 233 f., for a detailed discussion of equatorial rings. See also Price 1957, 587–589 and Dicks 1954, 79. Rome [1937, 226] notes that there is no reason to identify this ring with one of those Ptolemy mentions as being in the Palaestra.

instruments could have led to errors in the times of the equinoxes. After noting that an error in declination of only 0;6° will produce an error of a $^1/_4$ day, he continues:

> The error could be even greater in the case of an instrument which, instead of being set up for the specific occasion and positioned accurately at the time of the actual observation, has been fixed once [and] for all on a base intended to preserve it in the same position for a long period, [if] the instrument is affected by a [gradual] displacement which is unnoticed because of the length of time over which it takes place. One can see this in the case of the bronze rings in our Palaestra, which are supposed to be fixed in the plane of the equator. When we observe with them the distortion in their positioning is apparent, especially that of the larger and older of the two, to such an extent that sometimes the direction of illumination of the concave surface in them shifts from one side to the other twice on the same equinoctial day.

Unfortunately the interpretation of this passage is not entirely secure in some details. Yet, it is clear that Ptolemy observed some equinoxes on at least two different equatorial rings, that he found discrepancies and irregularities in the results obtained, and specifically that he discovered that an equinox sometimes appeared twice on the same instrument. Furthermore, Ptolemy implies that by carefully adjusting and checking his own instrument he overcame these difficulties, although he does not say how.[7]

Ptolemy states his observations of equinoxes to the nearest hour. This would require an implausibly large instrument if the equinoxes were determined from noon-altitudes or declinations of the Sun. On an equatorial ring, however, such apparent precision could be obtained by observing the time when the edge of the ring in shadow first became illuminated, or vice versa. The equinox would occur, of course, when the two edges of the ring were equally lit, as Hipparchus indicates in connection with the equinox observed at Alexandria in −145. Hipparchus and Ptolemy both mention the 'change of light' observed in these rings, which would occur roughly 3 hours before or after the actual equinox (if there were no refraction), and

[7] Rome [1937, 224, 231; 1931–1943, 817–818] argues that no astronomer would believe that the equinoxes appeared twice on the same ring, despite the transient effect due to refraction, since the shadow would cross the ring in the proper direction only once. Thus, he prefers the interpretation that the equinox was merely observed at two different times on two different rings. This interpretation seems somewhat forced, and also unnecessary, since Ptolemy mentions the phenomenon only to indicate the poor alignment of the rings.

which would have been apparent within an hour.[8] A simple extrapolation would then yield the time of the equinox. Therefore, although Ptolemy does not explicitly say so, we may conclude that if he did indeed observe the times of the equinoxes which he reports, his instrument must have been an equatorial ring.

In sum, Hipparchus probably determined the times of equinoxes from measurements of the Sun's declination, while Ptolemy's observations, if real, were probably made on an equatorial ring. Furthermore, Hipparchus' meridional armillary seems likely to have been graduated to $^1/_5$°, which would have enabled him to estimate the times of equinoxes to the nearest $^1/_4$ day. In contrast, the equatorial ring should, in theory, have enabled Ptolemy to determine the hour at which an equinox occurred, a precision which is consistent with Ptolemy's reported observations. As I shall show subsequently, however, the gain in precision afforded by an equatorial ring would have been more than offset by other difficulties encountered in its use.

The errors of the observations

In Table 2.1, columns I and II show the times of the equinoxes and solstices Ptolemy reports and the Sun's modern longitudes for these times.[9] Three different sets of errors are also presented. Column III shows the errors deduced from my elements [cf. appendix 1] expressed as corrections to Ptolemy's stated times. Column IV gives these errors reduced to Schoch's

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[8] Rome [1937, 233] shows it would be difficult to estimate the time of equal illumination to within 4^h on a ring 2 cubits in diameter. He ignores refraction, however.

[9] Computed from Tuckerman [1962–1964] and corrected by

$$\Delta(L) = + 0.86'' + 1.23''T - 0.51''T^2 \text{ (epoch, 1900.0)}$$

in accordance with the elements derived in appendix 1. If $\Delta(L)$ is the correction to the Sun's computed longitude at a given moment in Universal Time, and $\Delta(t)$ is the mean correction to the time computed (Tuckerman) at which the Sun would have a given longitude, $\Delta(t)$ is equal to $-\Delta(L)$ divided by the Sun's mean motion, 147.8″ *per* hour. These corrections, for the dates covered by the

Date and Place				I Local Apparent Time	II Solar Longitude (Computed)	III Error in Time Observed	IV Error (Schoch)	V Error (Newcomb)
−431	Jun	27	Athens	6^h	88.83°	$+29.5^h$	$+27.4^h$	$+32.7^h$
−279	Jun	26	[Alexandria]	[18]	89.52	+12.1	+10.3	+15.0
−161	Sep	27	[Rhodes]	18	180.62	−15.0	−16.6	−12.3
−158	Sep	27	[Rhodes]	6	180.40	−9.6	−11.2	−6.9
−157	Sep	27	[Rhodes]	12	180.41	−9.8	−11.4	−7.1
−146	Sep	27	[Rhodes]	0	180.23	−3.6	−7.2	−3.1
−145	Mar	24	Rhodes	6	359.61	+9.5	+7.9	12.0
−145	Mar	24	Alexandria	11	359.81	+4.6	+3.0	+7.1
−145	Sep	27	Rhodes	6	180.24	−5.8	−7.4	−3.3
−144	Mar	23	Rhodes	12	359.62	9.3	+7.7	+11.8
−143	Mar	23	Rhodes	18	359.63	+9.1	+7.5	+11.6
−142	Mar	24	Rhodes	0	359.64	+8.9	+7.3	+11.4
−142	Sep	26	Rhodes	18	180.01	−0.2	−1.8	+2.3
−141	Mar	24	Rhodes	6	359.65	+8.9	+7.1	+11.2
−140	Mar	23	Rhodes	12	359.66	+8.5	+6.9	+11.0
−134	Mar	24	Rhodes	0	359.70	+7.4	+5.8	+9.8
−134	Jun	26	Rhodes	12	90.17	−4.0	−5.6	−1.6
−133	Mar	24	Rhodes	6	359.71	+7.2	+5.6	+9.6
−132	Mar	23	Rhodes	12	359.72	+7.0	+5.4	+9.4
−131	Mar	23	Rhodes	18	359.73	+6.8	+5.2	+9.2
−130	Mar	24	Rhodes	0	359.73	+6.6	+5.0	+9.0
−129	Mar	24	Rhodes	6	359.74	+6.4	+4.8	+8.8
−128	Mar	23	Rhodes	12	359.75	+6.2	+4.6	+8.6
−127	Mar	23	Rhodes	18	359.76	+6.0	+4.4	+8.4
+132	Sep	25	Alexandria	14	181.36	−32.7	−33.9	−30.9
+139	Sep	26	Alexandria	7	181.37	−33.0	−34.2	−31.2
+140	Mar	22	Alexandria	13	0.83	−20.4	−21.6	−18.6
+140	Jun	25	Alexandria	2	91.42	−35.4	−36.6	−33.6

Table 2.1. Errors in Solar Observations

elements for the Sun,[10] which I have included since these elements form the basis for Tuckerman's tables [1962–1964]. Finally, column V shows the errors which result from omitting the Sun's secular acceleration from the computations. These errors are virtually identical with those found by using any of the older solar tables,[11] and I have included them merely to illustrate how the Sun's acceleration affects the distribution and magnitudes of the errors in Hipparchus' observations. The following discussion refers to the errors in column III unless stated otherwise.

It is evident that Ptolemy's equinox-observations are significantly and systematically in error; whereas the times Hipparchus reports fall on either

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equinoxes, are:

Date	$\Delta(L)$	$\Delta(t)$
−431	$-305'' = -0.085^\circ$	$+2.1^{\mathrm{h}}$
−279	$-268 \;= -0.074$	+1.8
−150	$-238 \;= -0.066$	+1.6
−130	$-234 \;= -0.065$	+1.6
+130	$-181 \;= -0.050$	+1.2
+140	$-179 \;= -0.050$	+1.2

The errors shown in column III of Table 2.1 are determined from $L'' - L$ divided by the Sun's true velocity; thus, they represent the corrections to Ptolemy's stated times. The solar velocities have been taken to be 0.0405° *per* hour at spring equinox, 0.0399° *per* hour at summer solstice, and 0.0415° *per* hour at fall equinox. The longitudes of the equinoxes in column II and the errors in column III have been checked against Fotheringham's [1918] with the appropriate corrections [cf. appendix 2]; small adjustments (less than 0.2^{h}) have been made in some of the errors in the times (column II) to compensate for the errors in rounding in Tuckerman. Thus, the errors in the times should be accurate to $\pm 0.1^{\mathrm{h}}$.

[10] Determined from column 2 by applying $\Delta(t)$ shown in 17n9 above.

[11] These errors are determined by Fotheringham [1918, 410], and for the solstices from my calculations using Newcomb's solar tables [Newcomb 1898]. The differences between Newcomb's longitude for the Sun and that shown in column II (C_{II}) may be found from

$$\Delta'(L) = L(C_{\mathrm{II}}) \; - L \text{ (Newcomb)} = 1.06'' + 2.66''T^2 \text{ (1900)}.$$

Cf. the comparisons by Rome [1937, 215–216] which are based on Schram's Tables [1908]. These tables, like Newcomb's, do not contain a correction for the Sun's secular acceleration.

side of the computed times, and exhibit relatively small errors.[12] Also, Hipparchus' observations display a systematic error in declination, while Ptolemy's do not.

Year	Type	I Solar δ (Declination)	II Residual Error (Col. I +0;7.0°)	III Solar δ (Schoch)	IV Residual Error (Col. III +0;6.0°)
−161	FE	−15.0	−8.0	−16.6	−10.6
−158	FE	−9.6	−2.6	−11.2	−5.2
−157	FE	−9.8	−2.8	−11.4	−5.4
−146	FE	−5.6	+1.4	−7.2	−1.2
−145	SE	−9.5	−2.5	−7.9	−1.9
−145	FE	−5.8	+1.2	−7.4	−1.4
−144	SE	−9.3	−2.3	−7.7	−1.7
−143	FE	−9.1	−2.1	−7.5	−1.5
−142	FE	−8.9	−1.9	−7.3	−1.3
−142	FE	−0.2	+6.8	−1.8	+5.2
−141	SE	−8.7	−1.7	−7.1	−1.1
−140	SE	−8.5	−1.5	−6.9	−0.9
−134	SE	−7.4	−0.4	−5.8	+0.2
−133	SE	−7.2	−0.2	−5.6	+0.4
−132	SE	−7.0	−0.0	−5.4	+0.6
−131	SE	−6.8	+0.2	−5.2	+0.8
−130	SE	−6.6	+0.4	−5.0	+1.0
−129	SE	−6.4	+0.6	−4.8	+1.2
−128	SE	−6.2	+0.8	−4.6	+1.4
−127	SE	−6.0	+1.0	−4.4	+1.6
+132	FE	−32.7		−33.9	
+139	FE	−33.0		−34.2	
+140	SE	+20.4		+21.6	

Table 2.2. Solar Declinations and Residual Errors at the Times of Equinoxes Reported by Hipparchus and Ptolemy in Units of 0;1°

[12] Note that the relatively small errors of the solstices ($+29.5^h$ in −431, $+12.1^h$ in −279, and -4.0^h in −134) are generally less than the errors in Ptolemy's equinoxes. Note also that the error in the equinox observed on the ring at Alexandria (−145) is equivalent to an error of less than 0;5° in declination. Thus, the alignment of this ring at the time must have been quite accurate.

This can be seen in Table 2.2, where column I shows the Sun's declinations at the times reported for the equinoxes.[13] All of Hipparchus' observations require a negative correction to the observed declination (i.e., δ). The average error for all twenty observations is $-0;7.7^\circ$, or $-0;7.0^\circ$, excluding the three earliest observations (-161, -158, -157). Column II shows the error remaining when a systematic error of $-0;7^\circ$ is removed. Except for the fall equinoxes of -161 and -142, these residual errors are all less than $0;3^\circ$.[14] Thus, if we ignore the systematic error in declination, Hipparchus' equinox-observations are accurate to the nearest $^1/_4$ day with only two exceptions.

For comparison, columns III and IV in Table 2.2 show the errors in declination derived from Schoch's elements and the residuals after correcting for a mean systematic error of $-0;6^\circ$. The differences between the two sets of residuals, especially for all but the three earliest observations, are very slight and of questionable significance for determining the Sun's acceleration.[15] Indeed, the only significant difference occurs in the equinox-observations of -158 and -157, where the residual errors exceed $0;3^\circ$ according to Schoch's elements, in contrast to those shown in column II.

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[13] Near the equinoxes the Sun's declination changes at a rate of very nearly $\pm 0;1^\circ$ *per* hour. Therefore, the magnitudes of the errors in the times of the equinoxes are also equal to those of the errors in the observed declinations of the Sun at the stated times. The sign of the correction to the observed declination is always the same as the sign of the error in the time at fall equinox, while at spring equinox the error in the observed declination has the opposite sign of the error in the time. Thus, for example, the fall equinox of -146 occurred roughly 6^h *earlier* than reported by Hipparchus, so that at the observed time the Sun's declination was nearly $-0;6^\circ$. Similarly, the following spring equinox occurred about 9^h *after* it was observed, so that at the observed time the Sun's declination was $\approx -0;9^\circ$.

[14] As may be seen from Table 2.2 column II, any value for the systematic error in declination between $-0;6.9^\circ$ and $-0;8.5^\circ$ will leave all the residuals less than $0;3^\circ$ except for the observations of -161 and -142.

[15] Fotheringham's initial least-squares determination [1918, 482] of the systematic error in declination ($\Delta\delta$) and of the Sun's acceleration (S_s) from the errors in Hipparchus' equinoxes determined from Newcomb's tables was

$$S_s = +1.0'' \pm 0.18''$$
$$\Delta\delta = -0;7.6^\circ \pm 0;0.46^\circ,$$

a result virtually identical with the elements used here.

On the assumption that Hipparchus 'would not be the man we assume him to have been if his equator at the latter dates had not been considerably better than his mean equator for the whole range of dates', Fotheringham [1918, 415]

Except for the equinoxes of −161 and −142, (and perhaps also −158 and −157), then, Hipparchus' reported observations form a consistent series which exhibit a systematic error in declination of $\approx -0;7°$, but which otherwise appear accurate to the nearest fraction of a day that Hipparchus could have observed.[16] In contrast, Ptolemy's three equinox-observations show a systematic error in time but not in declination, since the errors in declination of his spring and fall equinoxes differ by nearly a degree.

Hipparchus' observations and solar parameters

Before turning to the questions raised by Ptolemy's errors, let us first consider how the systematic error in Hipparchus' observations may have arisen. While the error of $-0;7°$ might have been purely accidental, it is remarkable that this is precisely the error which would lead to an interval between spring and fall equinoxes of 187 days.[17] Thus, if Hipparchus thought that the length of this interval was 187 days before he made his observations, he could have adjusted the equator of his meridional armillary to yield this result.

Such a procedure would reduce the uncertainty inherent in any attempt to determine fundamental alignments by direct measurements, a problem Ptolemy [*Alm.* iii 1: Toomer, 134] notes when discussing the alignment of equatorial rings. More importantly, this procedure would also ensure that the average error in the times of accurately observed individual equinoxes

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subsequently obtained, after eliminating the three earliest observations,

$$S_s = +1.95'' \pm 0.27''$$
$$\Delta\delta = -0;6.4° \pm 0;0.8°.$$

Within these limits, any value of the secular acceleration deduced from these observations will clearly depend on the assumptions made about weights and the methods of combining the observations.

[16] This is somewhat surprising in view of the apparent discordance between the fall equinoxes of −158, −157, and −146, and the long run of spring equinoxes consistent with a year of $365\,^1\!/_4$ days. In 18 years the time of the equinox moves forward nearly 4 hours.

[17] In −145 the actual interval between spring and fall equinox was 186 days 8.8 hours, or 15.2 hours less than that found by Hipparchus. Since a systematic error of −0.7° in declination would make the observed times of the spring equinoxes early by an average of 7 hours and the times of the fall equinoxes late by the same amount, the observed interval between the two types of equinoxes would be almost exactly 187 days.

would be only half as great as the error in the length of the interval between spring and fall equinoxes. Hence, if Hipparchus assumed that the interval from spring to fall equinox was 187 days to within $^1/_4$ day, he could have expected the times of individual equinoxes which reproduced this interval to be accurate, on the average, to within half this amount. Such a procedure would have been especially advantageous if, as Ptolemy suggests [*Alm.* iii 1], Hipparchus was particularly concerned with whether the year-length was constant, for determining this would require that the times of successive equinoxes of the same type be accurately observed.[18]

Thus, one explanation for both the systematic declination error and the relative accuracy of the times of Hipparchus' equinoxes is that they were determined by measuring the Sun's declination on an instrument whose equator was adjusted to yield a predetermined interval between spring and fall equinoxes. This would have been a perfectly rational observational procedure, which would have served to control the errors in the times of individual equinoxes. Still, it requires us to assume that at least one of Hipparchus' solar parameters was not determined from his own observations.

This seems probable, since the 187-day value for the interval from fall to spring equinox is attested in a solar scheme described by Geminus [Manitius 1898, 211] and attributed to Callippus (ca. −340), which gives the number of days which the Sun spends in each zodiacal sign.[19] As Aaboe

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[18] Ptolemy [*Alm.* iii 1] addresses the first part of his discussion of the length of the year to the question of whether it varies. He tells us [Toomer, 132] that 'the inequality [in the length of the year] revealed by successive observations disturbed Hipparchus', and he indicates that part of Hipparchus' work, *On the Changes of the Solstitial and Equinoctial Points*, discusses this problem. Stating further that Hipparchus tried to resolve this question by means of eclipses, Ptolemy cites two eclipses (−145 Apr 21 and −134 Mar 21) from which Hipparchus found the longitude of Spica to be Virgo $23^1/_2°$ and Virgo $24^3/_4°$. Theon [Rome 1931–1943, 826–830] gives further details of the eclipse of −134 and Hipparchus' procedure. These eclipses are cited as examples of the maximum observed deviation, and Hipparchus is said to have concluded [*Alm.* iii 1: Toomer, 135] that the inequality, if it existed, was no greater than $+^3/_4$ day [see 14n2, above]. Ptolemy concludes that no such inequality exists, and attributes the observed irregularities to observational and computational errors. He also notes [*Alm.* iii 1: Toomer, 136] that Hipparchus' solar model included only one inequality.

Hipparchus' concern about the existence of a second solar inequality may have been founded upon the irregularity of the appearances of the equinoxes on equatorial rings due to refraction, since he notes [*Alm.* iii 1: Toomer, 133] that the inequality may be observed on the ring in the Square Stoa at Alexandria.

[19] This scheme is included in the calendar attributed to Geminus. The dates of the Sun's entry into each of the signs are explicitly ascribed to Callippus. For

and Price [1964, 13] have pointed out, this rough value might easily have been determined by means of the *skaphe* sundials known from about −300. Indeed, the value is fairly close to the average which one would find using an accurate dial of this sort, since in the majority of instances the shadow at one equinox or another would be significantly affected by refraction in such a way as to make the apparent interval longer than it actually is [cf. Table 2.5]. Whatever the case, the assumption that 187 days separated the two equinoxes clearly antedates Hipparchus, and there is no reason to attribute this parameter to him.

In summary, the general accuracy of Hipparchus' equinox-observations appears to result from observations made with a meridional armillary from which the Sun's declination could be estimated to perhaps $^1/_{10}$°, and which may have been adjusted to yield an interval between fall and spring equinoxes of 187 days. Such a procedure would explain not only the individual systematic errors, but also the symmetry of the errors at the fall and spring equinox. In addition, it would have given Hipparchus a sound method for detecting any significant second inequality in the Sun's motion.

Ptolemy's observations

Ptolemy's observations raise different problems than those of Hipparchus. Whereas Hipparchus found the times of the equinoxes rather accurately, and the interval from spring to fall equinox less accurately, Ptolemy finds the same result as Hipparchus for the interval from spring to fall equinox, while the times Ptolemy gives for the equinoxes (and solstice) are badly in error. Furthermore, as already noted, this error could not have arisen solely from a systematic error in the declination of his equatorial ring, since such an error would have made the spring equinoxes appear too early by as much as it made the fall equinoxes too late. We may ask, therefore, whether the equinox times Ptolemy reports could have resulted from actual observations, or whether these reports can only be understood as calculations based on Hipparchus' solar model using one of his equinoxes as epoch.

The apparent advantage of using an equatorial ring rather than a meridional armillary is that the ring is a null-reading device which marks the moments of the equinoxes with much greater precision than direct measurements on any scale of modest size would yield. This apparent advan-

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a discussion of Callippus' authorship of this scheme, see Manitius 1898, 281n34; Boeckh 1863, 27–28, 46. Aaboe and Price [1964, 10–11] note that the interval of 187 days from spring to fall equinox appears in this scheme cited by Geminus.

tage, however, is more than offset by two serious difficulties which Ptolemy mentions and which any user of such a ring could hardly have avoided.

The first difficulty is that an equatorial ring must be accurately oriented not only with respect to the altitude of its north-south diameter, but also with respect to the level of its east-west diameter. Furthermore, unlike the meridional armillary, an equatorial ring does not allow the observer to check its alignment directly by means of a plumb-line. Ptolemy's statement [*Alm.* iii 1: Toomer, 134] that such instruments require careful adjustment ('positioning') at the time of the actual observations suggests that he was aware of the difficulties of aligning an equatorial ring by direct measurement, without indicating how he accomplished it.

The second and more serious shortcoming of the equatorial ring is that its results are very sensitive to the effect of refraction. On a meridional instrument this effect is both small and constant, but on an equatorial ring the effect of refraction on its apparent lighting is both significant and highly variable, since the effect depends on the time of day at which the actual equinox occurs.[20]

To understand how refraction affects observations made with an equatorial ring, consider first the apparent declination of the equator as a function of hour-angle. In Figure 2.1, NS represents the eastern horizon seem from due west, CD the true equator, $C'D'$ the apparent equator (that is, the locus of points at which a body located at each point of the true equator would be seen due to refraction), SDD' the meridian, $\bar{\phi}$ the co-latitude of the place of observation, P some point on the true equator with altitude $h(t)$ at hour-angle t, and P' the point on the apparent equator at which a body located at P would be seen due to refraction. Finally, $r(h)$ is the refraction and $\delta'(t)$ the declination of P'.

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[20] In Mediterranean latitudes the spring equinox should appear on a meridional armillary about half an hour before it occurs due to refraction, while the fall equinox should appear half an hour after it occurred. See Table 2.3.

Manitius [1898, 427n21] remarks that refraction would make a spring equinox appear on such a ring before it occurred. He also explains the double equinoxes mentioned by Ptolemy and the two times reported for the spring equinox of -145 [*Alm.* iii 1: Toomer, 135–136] as due to the variation of refraction during the day. Thus, he implies that both the latter observations were made on the same ring. Rome [1937, 231–232] discusses the effect of refraction and the resulting appearance of double equinoxes in more detail, but some of his statements are misleading and incorrect. Thus, speaking of spring equinoxes he says that 'in order to register one false equinox and one true one, the Sun must rise at least 12 hours before it passes the vernal point.' This is incorrect, as may be seen from Figure 2.2. Neither Rome nor Manitius describes the general effect of refraction at both equinoxes in detail.

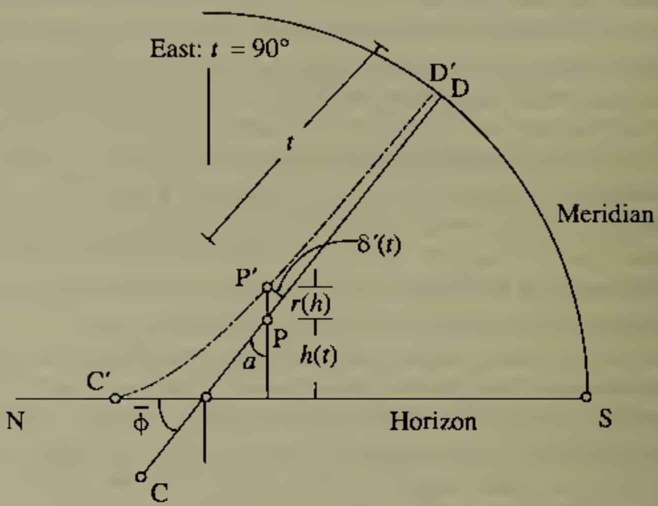

Figure 2.1

Since $r(h)$ is small, we may put

$$\delta'(t) = r(h) \cdot \sin a, \tag{1}$$

where a is the angle between the altitude-circle through P and the equator as shown. Furthermore, since

$$\sin a = \frac{\cos \bar{\phi}}{\cos h(t)}, \tag{2}$$

we have

$$\delta'(t) = \frac{r(h) \cos \bar{\phi}}{\cos h(t)}. \tag{3}$$

Finally, $h(t)$ is determined from the formula

$$\sin h(t) = \sin \bar{\phi} \cos t. \tag{4}$$

To find the error in the apparent time of an equinox observed on the ring, we first observe that, near the equinoxes, the Sun's true declination changes at very nearly $\pm 0;1°$ *per* hour.[21] Thus, if the equinox occurs at some time

[21] Near fall equinox the Sun's declination changes at a rate of $-0;0.98°$ *per* hour, while at spring equinox the rate of change is $+0;1.03°$ *per* hour.

T' (midnight epoch)		Altitude $h(T')$	Refraction $r(h)$	$\delta'(T') = \Delta T$ (0;1° or hours)
5;57.4^h	18; 2.6^h	−0;33.9°	0;34,54°	17.95
5;58	18; 2	−0;25.7	0;33,12	17.1
6; 0	18; 0	0; 0.0	0;28,52	14.9
6; 4	17;56	0;51.5	0;22,44	11.7
6; 8	17;52	1;43.0	0;18, 6	9.3
6;12	17;48	2;34.5	0;14,48	7.6
6;16	17;44	3;25.9	0;12,32	6.5
6;20	17;40	4;17.3	0;10,40	5.5
6;30	17;30	6;25.4	0; 7,40	3.96
6;40	17;20	8;34	0; 5,58	3.11
6;50	17;10	10;42	0; 4,52	2.54
7; 0	17; 0	12;49	0; 4, 6	2.16
8; 0	16; 0	25;24	0; 2, 1	1.14
9; 0	15; 0	37;32	0; 1,16	0.81
10; 0	14; 0	47;52	0; 0,52	0.67
11; 0	13; 0	54;55	0; 0,41	0.61
12; 0	12; 0	59; 0	0; 0,35	0.583

Table 2.3. Apparent Declination, $\delta'(T')$, of the Sun on the Equator T' Hours after Midnight

T, but appears on the ring at the time T', the Sun's true declination at T' will be equal to $\mp(T - T') \cdot 0;1^{\circ/h}$ if T and T' are expressed in hours. (Here and below, the upper sign applies to spring equinox and the lower sign to fall equinox.)

At T' the Sun's negative declination is just offset by its apparent elevation due to refraction. It follows that

$$\Delta T \cong T - T' = \pm\delta'(T'), \tag{5}$$

where ΔT is the error in (correction to) the time of the apparent equinox and $\delta'(T')$ is the apparent declination at time T' due to refraction as given in (3). Strictly, $\delta'(T')$ should be determined from (3) using the apparent altitude h', instead of the true altitude h; but since h' is not as easily computed, I have used the approximation given by (5). This approximation will affect the times of apparent equinoxes near dawn by a few minutes, but it will not otherwise affect the results discussed below.

Table 2.3 shows values of h, $r(h)$, and $\delta'(T')$, computed for $\bar{\phi} = 59;0°$ for different times during the day. Since $\pm\delta'(T')$ is the error in the time of an equinox which *appeared* at T', it is also the error in the time of an equinox which *occurred* at $T' \pm \delta'(T')$. Thus, the error in the observed time of an equinox, as a function of the time at which it occurred, can easily be computed for

$$T = T' + \delta'(T'), \tag{6}$$

and found by interpolation for other T.

Figure 2.2 shows the error in the time at which an equinox would be observed[22] as a function of the time at which it actually occurred. Since the error in the time of a spring equinox is the reflection, about noon, of the error in the time of a fall equinox, two time scales have been used. The upper one, which begins at midnight and reads from left to right, is for fall equinoxes, while the lower one, which also begins at midnight but which reads from right to left, is for spring equinoxes. The sign of the error (correction to the observed times) is positive for spring equinoxes and negative for fall equinoxes.

In Figure 2.3, the times (T') at which equinoxes of both types would be observed are plotted against the time (T) at which they occurred. As in Figure 2.2, the time scales for spring and fall equinoxes are the reverse of each other.

These graphs show that multiple appearances of an equinox on a well-aligned equatorial ring are common rather than exceptional. Thus, at fall equinoxes occurring between midnight and noon, the shadow crosses the ring twice, first in the correct direction and, then, near Sunset in the opposite direction. At fall equinoxes occurring between 12^h (noon) and 15^h (3 p.m.), the shadow crosses the ring three times: once shortly after the equinox occurs, once again before Sunset, and a third time shortly after Sunrise on the following day. Here again the shadow crosses the ring in the wrong direction on its second appearance, but the third crossing, at dawn, is in the proper direction.

After 15^h at fall equinox, refraction causes the apparent declination of the Sun to increase more rapidly than the Sun's true declination decreases. Fall equinoxes occurring after 15^h, therefore, do not appear on the ring that day, but rather they appear the following morning, near Sunrise, when the shadow crosses the ring in the correct direction. Similarly, at spring equinox, 'double equinoxes' appear when the equinox occurs between 12^h

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22 In speaking of the 'observation of an equinox' on an equatorial ring, I mean merely that the shadow is observed to cross the ring, regardless of which direction it moves.

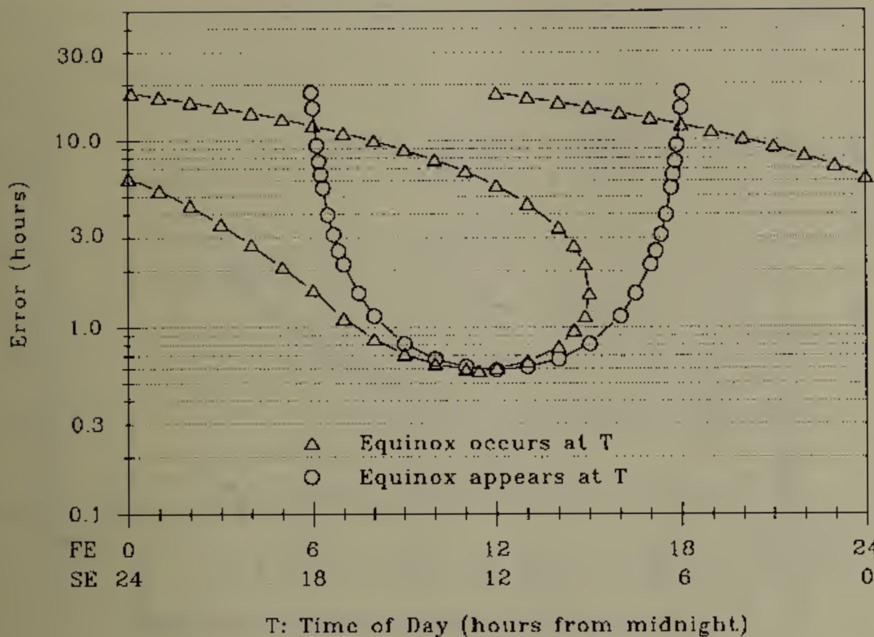

Figure 2.2. Error in an Equinox Observed on an Equatorial Ring

(noon) and the following midnight; whereas 'triple equinoxes' appear when the equinox occurs between 9^h and 12^h, and single equinoxes appear when the equinox occurs between midnight (0^h) and 9^h.

That the Sun's shadow may cross the ring two and even three times at a single equinox is only one of the problems encountered in using an equatorial ring to determine the equinoxes. Another difficulty is that even if the second and third appearances of equinoxes were ignored, equinoxes of the same type would still appear at very irregular intervals [cf. Bruin 1976]. Furthermore, although one should see (and would expect to see) only two successive equinoxes of the same type followed by two years in which these equinoxes occurred at night, both equinoxes would appear on an equatorial ring every year with the shadow moving in the correct direction.

For example, a fall equinox occurring between 7^h and 14^h would be observed only slightly ($\approx {}^3/_4{}^h$) after its occurrence, while those occurring after 15^h and before 7^h would all appear near Sunrise. Thus, a fall equinox observed in the middle of the morning would be followed by one which would not be observed until Sunrise of the day after that which the $^1/_4$-day surplus would lead us to expect. This equinox, furthermore, would be followed by

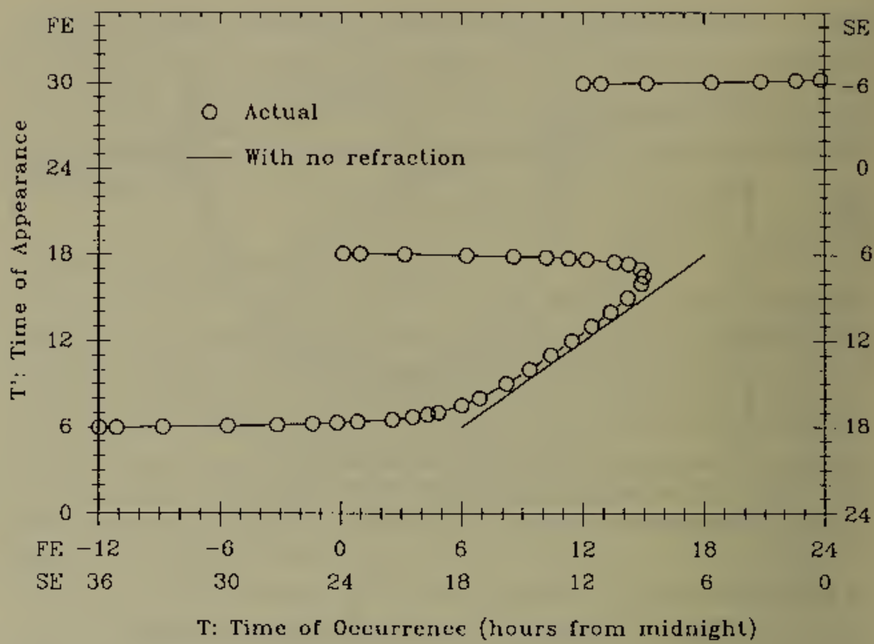

Figure 2.3. Apparent Time (T') of an Equinox Occurring at T

two more which would appear at almost exactly the same time of day at intervals of 365 days. The same problem would be encountered at spring equinoxes, with the difference that an equinox observed in mid-afternoon just before it actually occurred would be preceded by one observed near Sunset on the day before it should have appeared, and so on.

To illustrate the behavior of the shadow more clearly, Table 2.4 gives the times of the true equinoxes during the period for which Ptolemaic observations are preserved, together with the times at which the Sun's shadow would appear centered on an accurately aligned ring.[23] The times in parentheses denote the cases in which the shadow would move in a direction opposite to that characteristic of the type of equinox in question. The times of the apparent 'correct' equinoxes—i.e., those at which the shadow first crosses the ring in the proper direction at fall equinox and last crosses it

[23] The times of the true equinoxes are computed from Schoch's elements and serve merely to illustrate irregularities due to refraction. To reduce the times at which the equinox occurred to the elements derived in appendix 1, add 1.2^h. The times of the apparent equinoxes can then be obtained from Figure 2.3.

Year	Spring Equinoxes					Fall Equinoxes			
	Month	True Time	Apparent Time			Month	True Time	Apparent Time	
		Day Hour	Day Hour	Day Hour	Day Hour		Day Hour	Day Hour	Day Hour
+125	Mar	22 0; 5	21 17;43			Sep	24 11;32	24 12; 8	(24 17;42)
+126	Mar	22 5;55	21 17;58			Sep	24 17;22	25 6; 2	
+127	Mar	22 11;44	22 11; 5	(22 6;21)	21 18; 2	Sep	24 23;11	25 6; 4	
+128	Mar	21 17;33	21 16;15	(21 6; 3)		Sep	24 23;11	25 6; 4	
+129	Mar	21 23;31	22 17;40	(21 5;58)		Sep	24 10;48	24 11;26	(24 17;45)
+130	Mar	22 5;10	21 15;57			Sep	24 16;37	25 6; 1	
+131	Mar	22 11; 0	22 10;22	(22 6;29)	21 18; 2	Sep	24 22;27	25 6;13	
+132	Mar	21 16;49	21 15;47	(21 6; 4)		Sep	24 4;16	24 6;49	(24 17;59)
+133	Mar	21 22;38	22 17;37	(21 5;59		Sep	24 10; 5	24 10;40	(24 17;48)
+134	Mar	22 4;27	21 7;25			Sep	24 15;54	25 6; 1	
+135	Mar	22 10;16	22 9;32	(22 6;34)	21 18; 1	Sep	24 21;43	25 6;10	
+136	Mar	21 16; 6	21 15;15	(21 6; 6)		Sep	24 3;33	24 6;41	(24 17;59)
+137	Mar	21 21;55	22 17;32	(21 5;59)		Sep	24 9;22	24 10; 3	(24 17;50)
+138	Mar	22 3;44	21 7;15			Sep	24 15;11	21 17;54	
+139	Mar	22 9;33	22 8;41	(22 8; 9)	21 18; 1	Sep	24 21; 0	25 6; 9	
+140	Mar	21 15;22	21 14;37	(21 6; 7)		Sep	24 2;49	24 6;33	(24 18; 0)

Table 2.4. True and Apparent Times of Spring and Fall Equinoxes

at spring equinox—have been used to compute the intervals between spring and fall equinox and the lengths of the 'year' between successive equinoxes of the same type. Table 2.5 shows these intervals, and also that the average apparent time from spring to fall equinox is very nearly 187 days.

These two tables illustrate the grave deficiencies of even a perfectly aligned equatorial ring due to refraction. The erratic behavior of the Sun's shadow also explains why Hipparchus noted that the inequality in the length of the year could be seen on the ring at Alexandria. The maximum difference between the apparent lengths of two successive years is very nearly 18^h, or $^3/_4$ day [see Table 2.5, column B], which agrees with Hipparchus' limit for the inequality of the year-length. Thus, although Ptolemy says that this limit was derived from observations of lunar eclipses, Hipparchus could have found confirmation of this estimate in reports of equinoxes observed on the ring at Alexandria.[24]

Ptolemy's discussion of the sources of errors in such observations and his unequivocal rejection of a second solar inequality show clearly that he was satisfied that observational errors caused these irregularities. In particular, he seems to attribute the appearances of double equinoxes to errors in the alignment of the rings on which they were observed. Furthermore, he remarks that such errors were likely to occur if the instruments were not set up and adjusted for each actual (set of) observations, implying that he succeeded in eliminating these errors in some fashion.

It is difficult to see how Ptolemy could have done this, since no single alignment of his equatorial ring would eliminate the irregularities due to refraction. A deviation in his ring's altitude from the altitude of the equator at Alexandria would not affect the behavior of the shadow when the Sun was near the horizon, and so would not affect the appearances of multiple equinoxes. Similarly, an error in the level of the east-west diameter of the ring would not prevent double equinoxes from appearing when the Sun was near the horizon, while accentuating the irregularity of the appearances of equinoxes occurring at different times during the day. Thus, correcting any error in the alignment of such an instrument would not alleviate the difficulties described above.

Nor could Ptolemy have aligned his ring so that equinoxes of both types would appear at the times expected from Hipparchus' observations and the

[24] According to Ptolemy [*Alm.* iii 1: Toomer, 135], Hipparchus was unwilling to accept errors of this sort as decisive evidence of the existence of an inequality in the length of the year and had more confidence in measurements made during lunar eclipses. Since Hipparchus [*Alm.* iii 1: Toomer, 136] did not include a second inequality in his solar model, however, it seems that he, as well as Ptolemy, eventually concluded that these irregularities were due to observational errors.

Year	A Apparent Time SE to FE[a]		B Apparent Year Length ($365^d + \Delta^h$) from SE Δ	Δ^2	from FE Δ	Δ^2
+125	186^d	18.4^h	$0;15^h$	$+16;52^h$	$18;10^h$	$-17;52^h$
+126	187	12.1	17; 7	−11;57	0;12	+0;40
+127	186	19.1	5;10	−3;45	0;52	+3;26
+128	186	14.9	1;25	−1; 8	4;20	14;15
+129	186	17.8	0;17	+16; 8	18;35	−18;23
+130	187	12.1	16;25	−11; 0	0;12	+0;24
+131	186	19.9	5;25	−3;35	0;36	+3;18
+132	186	5.0	1;50	−1;32	3;54	+15;24
+133	186	17.1	0;18	+15;19	19;18	−19; 9
+134	187	12.1	15;37	−9;54	0; 9	+0;22
+135	186	20.6	5;43	−3;26	0;31	+2;51
+136	186	15.4	2;17	−1;59	3;22	+16;36
+137	186	16.5	0;18	+14;29	19;58	−19;50
+138	187	12.1	14;47	−8;51	0; 8	+0;16
+139	186	21.5	5;56		0;24	
+140	186	15.9				

[a] Mean: $186^d\ 22.3^h$ True: $186^d\ 11.45^h$

Table 2.5

length of the tropical year. This may be seen by considering the alignment errors necessary to produce the observed times of the three equinoxes which Ptolemy reports, and which agree almost exactly with Hipparchus' observations of −146 and −145 [*Alm.* iii 1: Toomer, 134–135].

As shown in Figure 2.4, any (small) deviation from the plane of the equator in a ring's alignment may be considered the result of independent rotations about the ring's east-west and north-south diameters. Let m denote a rotation about the east-west axis and n a rotation about the north-south axis. Let both angles be positive in the direction shown in Figure 2.4, and let t' equal the hour-angle of the Sun when an equinox

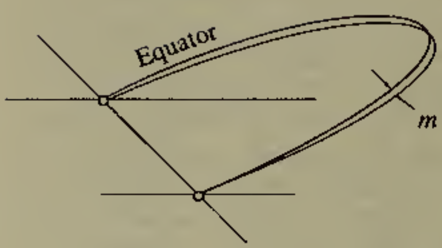

Figure 2.4a

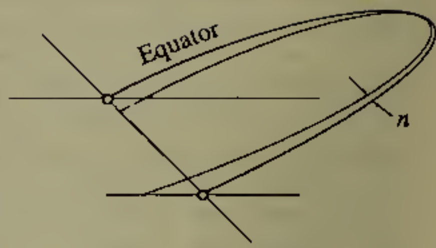

Figure 2.4b

appears on the ring.[25] Then, if $\delta_m(t')$ is the declination of P' due solely to m, and $\delta_n(t')$ is the declination of P' due solely to n, we have

$$\tan \delta_m(t') = \tan m \cdot \cos t' \tag{7}$$

and

$$\tan \delta_n(t') = \tan n \cdot \sin t'. \tag{8}$$

If both m and n are small, (7) and (8) may be replaced by

$$\delta_m(t') = m \cos t' \tag{9}$$

and

$$\delta_n(t') = n \sin t'. \tag{10}$$

Thus, the declination of P' due to the combined rotations is

$$\delta(t') = m \cos t' + n \sin t'. \tag{11}$$

For an equinox to appear on the ring, the Sun's apparent declination (affected by refraction) must just equal the declination of P'. Thus, if δ is the Sun's true declination and δ' is the apparent increase due to refraction, an equinox will appear to occur when

$$\delta + \delta' = m \cos t' + n \sin t'. \tag{12}$$

[25] t' is positive when the Sun is west of the meridian and negative when the Sun is east of the meridian. The time at which an equinox appears expressed in hours after midnight is $T' = 12^{\mathrm{h}} + t'/15$.

Conversely, given T', $\delta(T')$, and $\delta'(T')$, we can determine m and n from the times (T') at which any two equinoxes are said to have been observed.

Applying (12) to the three equinoxes reported by Ptolemy taken in pairs, we find:[26] from the fall equinoxes of +132 and +139,

$$m = -0;48^\circ \text{ and } n = +0;19^\circ;$$

from the fall equinox of +132 and the spring equinox of +140,

$$m = +1;13^\circ \text{ and } n = -2;40^\circ;$$

and from the fall equinox of +139 and the spring equinox of +140,

$$m = -0;19^\circ \text{ and } n = +0;27^\circ.$$

Clearly, all three equinoxes could not have been (accurately) observed on a ring with the same alignment, and in general Ptolemy could not have so observed the times of both the spring and fall equinoxes which he would have expected from Hipparchus' observations and length of the tropical year. On the other hand, Ptolemy could have aligned his ring so that equinoxes of one type or the other would appear at the expected times, although these would have been followed by appearances of equinoxes of the same type which he would have expected to occur at night.

This may be seen from Table 2.6, which shows that pairs of equinoxes of both types would be expected in the morning and shortly after noon. Since at these times the effect of refraction is relatively small (1–2^h) and nearly the same [cf. Figure 2.2], a ring set up so that one such pair appeared would also produce appearances of the following pairs of the same type.

It is evident that Ptolemy's equinox-observations should not be understood as independent observations affected by an inadvertent systematic error, or even as consistent observations designed to verify Hipparchus' solar parameters. In view, however, of Ptolemy's explicit statements concerning the two fall equinoxes which he reports, particularly that of +132, it is also difficult to conclude that he did not observe the equinoxes at all. Furthermore, such a conclusion fails to explain Ptolemy's evident familiarity with the difficulties encountered in such observations. Nevertheless, it seems that at best Ptolemy could have set up his equatorial ring to show only one of the equinoxes expected from Hipparchus' solar model, and

[26] Given T', t' is computed from the relationship shown in 34n25, above. $\delta'(T')$ can be found from Table 2.3, and δ is obtained from modern theory. See Table 2.2.

Spring Equinoxes Date	Time	Fall Equinoxes Date	Time
127 Mar 23	18;15^{h}	127 Sep 26	8;10^{h}
128 Mar 22	14;10	128 Sep 25	14; 5
131 Mar 23	7;55	131 Sep 26	7;50
132 Mar 22	13;50	132 Sep 25	13;45[a]
135 Mar 23	7;35	135 Sep 26	7;30
136 Mar 22	13;30	136 Sep 25	13;25
139 Mar 23	7;15	139 Sep 26	7;10[b]
140 Mar 22	13;10[c]	140 Sep 25	13; 5

[a] Ptolemy: 14^{h} [b] Ptolemy: 7^{h} [c] Ptolemy: 13^{h}

Table 2.6. Observable Equinoxes Computed from Hipparchus' Fall Equinox of −146 and Spring Equinox of −145, Assuming a Tropical Year of 365;14,48 Days

then found that the following pair of equinoxes of the same type occurred at roughly the times predicted for them.

Such a procedure would hardly have provided any significant evidence to confirm Hipparchus' solar model, and Ptolemy would still have had to ignore the appearances of double equinoxes, of equinoxes which should have occurred at night, and of discordant equinoxes characteristic of the other season from that for which the ring was set up. Furthermore, such a procedure is hardly consistent with Ptolemy's assertion [*Alm.* iii 1: Toomer, 139] that he confirmed Hipparchus' solar parameters by his own observations.

Although the conclusion that Ptolemy's equinox-observations can scarcely have been more than the results of computations is unsatisfying, I can find no other explanation of the errors in his reported times and their agreement with Hipparchus' observations and year-length. On the other hand, if Ptolemy set out to determine the times of the equinoxes using an equatorial ring, he could not have avoided the difficulties and irregularities described above. So he might easily have concluded that he could make no secure improvement on Hipparchus' solar parameters. Furthermore, since not only his observations of planetary oppositions, but all observations made with his armillary astrolabe, require knowing the longitude of some celestial body for use as a reference point, Ptolemy could not have observed the longitudes of any other celestial body without a solar model.[27] Thus, he

[27] Ptolemy reports three observations of planets he made with an armillary astrolabe which antedate his earliest reported equinox-observation (+132). The

might well have adopted Hipparchus' solar parameters in order to proceed with other observations.

Finally, it is quite possible that Ptolemy was aware of the errors in his equinox-'observations', but chose to accept a poor equinox and tropical year-length to avoid undermining his (correct) conclusion that the year-length was constant and not subject to a second solar inequality. Hipparchus apparently left this an open question, while reporting at least two older observations which supported his determination of the tropical year-length. Accordingly, if Ptolemy had accurately observed the equinoxes and consequently found a nearly correct year-length from comparisons with Hipparchus' equinox-observations, he would either have had to show that the year-length derived by Hipparchus' was in error or accept a variation in the length of the year. It is probable that Ptolemy lacked a sufficient number of early equinox-observations to demonstrate such an error; and it is quite possible that no such observations existed, since Hipparchus apparently mentions only solstice-observations prior to his own time. Lacking reliable early observations of tropical phenomena to settle the question, Ptolemy may well have chosen to sacrifice the accuracy of his equinox for theoretical clarity.

Ptolemy's solar tables

Whatever the explanation of Ptolemy's reported observations of the Sun, it is clear that he needed both mean and true solar positions and, thus, solar tables, long before he observed the equinoxes and solstice in +139 and +140. These are the observations which he cites to justify accepting Hipparchus' values for the lengths of the year and the seasons. Since both Ptolemy and Hipparchus used the same values for the mean motion, eccentricity, and apogee of the Sun, it is natural to ask if Ptolemy's solar tables were identical with Hipparchus'. The little evidence there is suggests that they were not.

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earliest of these is an observation of an opposition of Saturn on +127 Mar 26 [*Alm.* xi 5: Toomer, 525]. The others are an opposition of Mars in +130 Dec 15 [*Alm.* x 7: Toomer, 484] and of Mercury's greatest elongation as an evening star in +132 Feb 2 [*Alm.* ix 7: Toomer, 449].

In such observations it is necessary to know the longitude of some reference body in order to align the longitude ring of the armillary astrolabe in the plane of the ecliptic. Ptolemy would thus have needed to know at least the longitude of a few reference stars to make these observations. To determine these longitudes or even to check provisional longitudes derived from Hipparchus' observations, Ptolemy would have needed solar tables.

Date	−127 Aug 5	−126 May 2	−126 Jul 7
Time (since epoch)	619^d 314^y 17;50^h	620^d 219^y 18;20^h	620^d 286^y 4^h
Mean Solar Anomaly (Tables)	64;59°	331; 7°	36;34°
Solar Longitude (°)			
Hipparchus	Leo 8 $\frac{1}{2}$ $\frac{1}{12}$^{a}	Tau 7 $\frac{1}{2}$ $\frac{1}{12}$^{b}	Can 11 − $\frac{1}{10}$^{c}
Ptolemy	Leo 8;20	Tau 7;45	Can 10;40
Recomputed	Leo 8;22	Tau 7;44	Can 10;42
Tables: Hipparchus	−0;13°	−0; 1°	−0;12°

^{a} = 8;35° ^{b}= 7;45° ^{c}= 10;54°

Table 2.7. Comparison of Hipparchus' Computed Solar Longitudes with Values Computed by Ptolemy and Values Recomputed from Ptolemy's Tables

Ptolemy quotes three observations of the elongation of the Moon which Hipparchus made at Rhodes [*Alm.* v 3, 5: Toomer, 224, 227, 230]. In each Hipparchus reports the computed solar longitudes which he used to find the Moon's elongation (possibly with an instrument similar to Ptolemy's armillary astrolabe, since the elongations are given directly in longitude).

Table 2.7 shows these longitudes compared with those Ptolemy computed for the same times, and also with longitudes accurately computed from Ptolemy's tables for the times of Hipparchus' observations. These comparisons show discrepancies of $\frac{1}{5}$° in two cases and close agreement for the second of the three observations. Ptolemy [*Alm.* iv 11: Toomer, 211–216] also cites four computations of the Sun's progress in longitude during the intervals between eclipses,[28] which Hipparchus used to obtain erroneous values for the Moon's eccentricity. As Ptolemy remarks, each of these intervals of longitude differs significantly those he himself computed for the same eclipses. Since the actual solar longitudes are not given, it is impossible to deduce the values of the solar equation that would account

[28] The eclipses are:

(1) −382 Dec 23	(2) −381 Jun 18	(3) −381 Dec 12
(4) −200 Sep 22	(5) −199 Mar 19	(6) −199 Sep 12

The possibility that only the solar longitude for the middle eclipse of each triad is in error is excluded, since such an error would produce equal errors with opposite signs in each pair of successive intervals. The actual errors, however, are as shown for the observed discrepancy. Nevertheless, it is clear that some of those values must have differed from Ptolemy's.

All these discrepancies can be explained, of course, by assuming that Hipparchus was a poor computer. A more plausible explanation, however, is that Hipparchus' correction for the solar inequality differed from Ptolemy's. If so, his tabular mean solar longitude should also have differed.[29]

Whatever the case, it appears unlikely that Ptolemy's solar tables are merely copies or extensions of Hipparchus' tables, even though they are based upon identical parameters and reproduce almost precisely Hipparchus' fall equinoxes of −145 and −146 [cf. Table 2.8]. In view of the fact that Ptolemy could hardly have waited until +140 or even +132 to construct his solar tables, it is quite possible that he first rigorously recomputed the solar inequality from Hipparchus' parameters, and then determined the epoch of the mean motion to agree with Hipparchus' fall equinox of either −145 or −146. This would have given him a set of provisional tables to work with in order, for example, to determine the opposition of Saturn in +127.

In summary, it seems impossible that the errors in Ptolemy's equinox-observations arose either from a systematic error in independent observations or from procedures designed to confirm Hipparchus' parameters. It does appear, however, that Ptolemy's solar tables are not identical with Hipparchus' despite the identity of the underlying parameters, and that Ptolemy must have at least recomputed the values for the solar inequality and, hence, the epoch of the Sun's mean motion.

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below.

Solar Progress in Longitude between Eclipses			
Eclipses	Hipparchus	Ptolemy	Δ
(1) and (2)	172;52,30°	173;28°	+0;35,30°
(2) and (3)	175; 7,30	174;44	+0;36,30
(4) and (5)	180;20	180;11	−0; 9
(5) and (6)	168;33	168;55	+0;22

For extended discussion of these data and Hipparchus' solar model, see Jones 1991.

[29] Hipparchus' determination of the Sun's mean longitude at any equinox must have depended on his value for the Sun's inequality at that time. Thus, the observed discrepancies cannot be viewed as solely due to an error in Hipparchus' equation. Unfortunately, I can find no plausible scheme which would account for the discrepancies which appear.

Type	Date	Time[a]	Era Nabonassar			Computed Solar Longitude[b]	ΔL (Comp. − Obs.)
HIPPARCHUS							
FE	−161 Sep 27	18ʰ	584ʸ	359ᵈ	6ʰ	180;41,47°	+0;41,47°
FE	−158 Sep 27	6	588	359	18	180;12,25	+0;12,25
FE	−157 Sep 27	12	589	360	0	180;12,37	+0;12,37
FE	−146 Sep 27	0	600	362	12	179;59,47	−0; 0,12
SE	−145 Mar 24	6	601	175	18	0; 1, 8	+0; 1, 8
FE	−145 Sep 27	6	601	362	18	179;59,59,42	−0; 0, 0,18
FE	−142 Sep 26	18	604	363	6	179;45,34	−0;14,26
SE	−134 Mar 24	0	612	178	12	0; 3,16	+0; 3,16
SE	−127 Mar 23	18	619	180	6	0; 4,37	+0; 4,37
PTOLEMY							
FE	+132 Sep 25	14ʰ	879ʸ	66ᵈ	2ʰ	180; 0,24°	+0; 0,24°
FE	+139 Sep 26	7	886	67	19	179;59,18	−0; 0,42
SE	+140 Mar 22	13	886	246	1	0; 0,39	+0; 0,39
SE	+140 Jun 25	2	886	340	14	90; 2,42	+0; 2,42

[a] Observed local apparent time, midnight epoch. [b] From Ptolemy, *Alm.* iii

Table 2.8. Spring and Fall Equinoxes Observed by Hipparchus and Ptolemy. Comparison with Ptolemy's Tables

In view of his need for an adequate solar table for other observations, and the difficulties he must have encountered in whatever observations he made on the equatorial ring, it is not surprising that Ptolemy did not attempt to improve on Hipparchus' solar model but only on the tables derived from it. Indeed, considering the irregularities he must have found with his equatorial ring, it is perhaps more surprising that he did not accept the false conclusion of a second inequality in the Sun's motion. Instead, his remarks in *Alm.* iii 1 [Toomer, 136] suggest that he excluded this possibility because a second solar inequality would destroy the agreement of his lunar model at syzygy with the observed times of eclipses.

The errors of Ptolemy's solar model

Since Ptolemy's solar model forms the basis—directly or indirectly—for the reduction of all his longitude-observations of other celestial bodies, it is convenient to determine here the corrections to Ptolemy's tabular longitudes which bring them into agreement with modern theory. This error has two components. One is a secular error in Ptolemy's mean solar longitude resulting from the error in his mean motion and epoch. The other is a nearly periodic error due to the inaccuracy of his eccentricity and apogee, which changes slowly with the motion of the Sun's apsidal line. In the following discussion, symbols with primes (′) denote Ptolemy's elements while symbols without primes denote modern elements.

The error in Ptolemy's mean solar longitude. The mean longitude of the Sun according to Ptolemy may be expressed as

$$\bar{L}' = 330;45^\circ + (100^r + 0;19,42,8^\circ)T_1, \tag{13}$$

where T_1 is the number of Julian centuries from Ptolemy's epoch, −746 Feb 26, noon [*Alm.* iii 7: Toomer, 183]. From the elements derived in appendix 1, we find for the the same epoch,

$$\bar{L} = 328;13,58^\circ + (100^r + 0;44,20.6^\circ)T_1 + 0;0,2.1^\circ T_1^2. \tag{14}$$

Thus, the error in Ptolemy's mean longitude of the Sun is

$$\bar{L} - \bar{L}' = -2;31.0^\circ + 0;24.63^\circ T_1 + 0;0,2.1^\circ T_1^2. \tag{15}$$

If T is the number of Julian centuries from 0 AD, January 0, the error is[30]

$$\bar{L} - \bar{L}' = +0;30.7^\circ + 0;25.15^\circ T + 0;0,2.1^\circ T^2. \tag{16}$$

[30] From Schoch's elements [P. V. Neugebauer 1929, i 35], the error in Ptolemy's mean longitude for 0.0 AD, is

$$\bar{L}_{\text{Schoch}} - \bar{L}' = +\ 0;34.1^\circ + 0;24.80^\circ T + 0;0,2.6^\circ T^2.$$

From Newcomb's elements [1898, 1], which do not include the Sun's secular acceleration, the error is

$$\bar{L}_{\text{Newcomb}} - \bar{L}' = +\ 0;25.5^\circ + 0;25.74^\circ T + 0;0,1.1^\circ T^2.$$

The latter error is nearly identical with the error found by Ideler [1806, 107].

In ∓140 the errors in Ptolemy's mean solar longitude according to Schoch and Newcomb are:

	Schoch	Newcomb
−140	−0;1°	−0;10°
+140	+1;9°	+0;58°.

Thus, for any reasonable assumption about the magnitude of the Sun's acceleration, Ptolemy's mean solar longitude is very nearly accurate in −140 and roughly 1° in error in his own time.

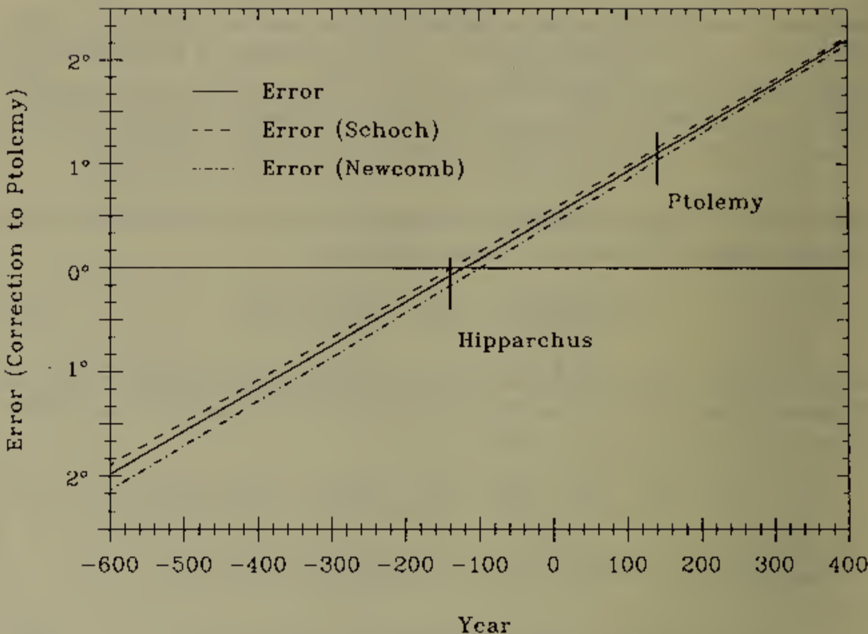

Figure 2.5. Error in Ptolemy's Mean Solar Longitude

This error is plotted in Figure 2.5. It is zero in −122, and +1;5° at +137.5, the epoch of Ptolemy's star catalogue. By 1500 the error is nearly +7° (+6;56°). For Schoch's (Tuckerman's) elements the error is zero in −138 and 1;8° at Ptolemy's epoch.

The error in Ptolemy's solar inequality. Figure 2.6 shows Ptolemy's solar model. The Sun at S moves on a circle $A'SP'$ with uniform motion about its center C, which coincides with the center of the zodiac. The observer at O sees the Sun at $L' = \bar{L}' + g'$, where g' is the angle OSC. (As shown here, g' is negative.) A' and P' denote the Sun's apogee and perigee, $\bar{a}'$ $(= \bar{L}' - A')$ the Sun's mean anomaly, and a' $(= L' - A')$ its true anomaly. In Ptolemy's model the longitude of the apogee is 65;30° and the eccentricity, e' $(= OC)$, is 0;2,30, where $R = CS = 1$.

For uniform eccentric motion, the equation (g') may be expressed as

$$g' = a' - \bar{a}' = -e' \sin \bar{a}' + \frac{1}{2} e'^2 \sin 2\bar{a}' - \frac{1}{3} e'^3 \sin 3\bar{a}' + \dots, \qquad (17)$$

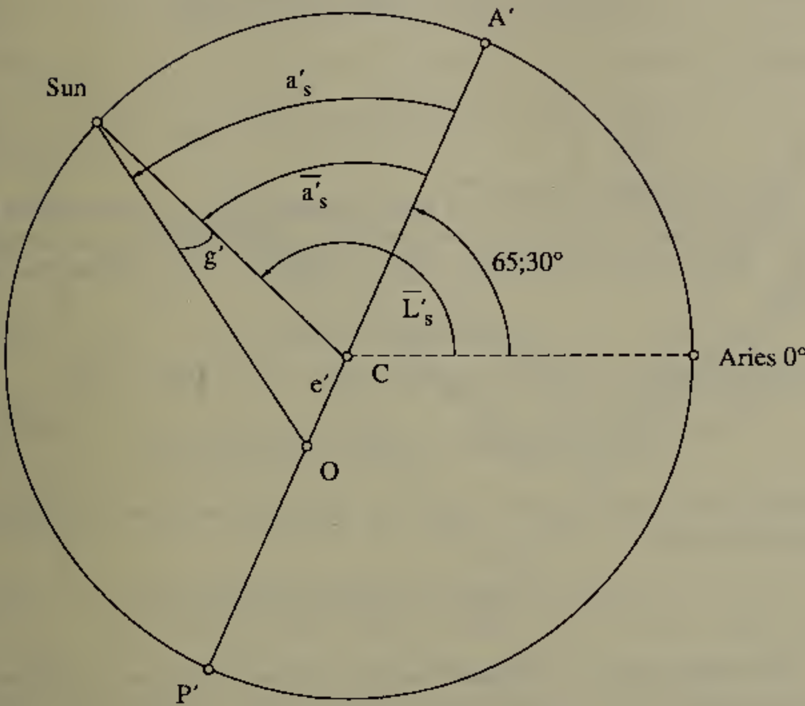

Figure 2.6: Ptolemy's Solar Model

where the powers of the eccentricities are expressed in radians or their equivalents in degrees [see appendix 3]. In undisturbed elliptic motion, the corresponding equation is[31]

$$g = a - \bar{a} = -(2e - \frac{1}{4}e^3)\sin\bar{a} + (\frac{5}{4}e^2 - \frac{11}{24}e^4)\sin 2\bar{a} \\ - (\frac{13}{12}e^3)\sin 3\bar{a} + \dots \qquad (18)$$

[31] For a development of this expression for the inequality in undisturbed elliptic motion, see Brown 1896, 30 ff.

Since for our purposes accuracy of 0;1° suffices, for the Sun we may ignore powers of e and e' greater than 2. Thus, (17) and (18) become

$$g' = a' - \bar{a}' = -e' \sin \bar{a}' + \frac{1}{2} e'^2 \sin 2\bar{a}' \tag{19}$$

and

$$g = a - \bar{a} = -2e \sin \bar{a} + \frac{5}{4} e^2 \sin 2\bar{a}. \tag{20}$$

To compare (19) and (20) we must first determine the relationship between the Sun's actual mean anomaly ($\bar{a}$) and Ptolemy's mean anomaly ($\bar{a}'$). From Newcomb's *Tables of the Sun* [1898, 1], we find for the year 0 AD

$$A = 68;43,12° + 1;42,7° T, \tag{21}$$

and, thus, that the error in Ptolemy's apogee ($A' = 65;30°$) is[32]

$$A - A' = 3;13.2° + 1;42.1° T \text{ (epoch, 0 AD)}. \tag{22}$$

Finally, from (22) and (16) above, we obtain for the error in Ptolemy's mean anomaly

$$\bar{a} - \bar{a}' = -2;42.5° - 1;17.0° T \text{ (epoch, 0 AD)}. \tag{23}$$

From (19), (20), and (23), we can now determine the error in Ptolemy's solar equation (g') as a function of his mean solar anomaly ($\bar{a}'$). Letting $\bar{a} - \bar{a}' = B(T)$, we obtain

$$\begin{aligned} g - g' &= -\, 2e \sin(\bar{a}' + B) + e' \sin \bar{a}' + \frac{5}{4} e^2 \sin(2\bar{a}' + 2B) - \frac{e'^2}{2} \sin 2\bar{a}', \\ &= -\,(2e \cos B - e') \sin a' + (\frac{5}{4} e^2 \cos 2B - \frac{e'^2}{2}) \sin 2\bar{a}' \\ &\quad - 2e \sin B \cos \bar{a}' + \frac{5}{4} e^2 \sin 2B \cos 2\bar{a}'. \end{aligned} \tag{24}$$

---

[32] For the years ± 140, the errors in Ptolemy's apogee are:

+140	$A = 71;\ 5°$	$A - A' = +5;35°$
−140	$A = 66;19°$	$A - A' = +0;49°$.

Ptolemy's apogee is correct for −188.

Note that the error in Ptolemy's mean anomaly differs from that in his apogee due to the error in his mean longitude. The date at which Ptolemy's mean anomaly is accurate, which is the date at which the errors in his inequality are due solely to the error in his eccentricity, is −210 [cf. Aaboe and Price 1964, 14].

Introducing numerical values for e, e', and $B(T)$,[33] we find for the time of Ptolemy's own observations (ca. +135)

$$g - g' = +0;23.4^\circ \sin \bar{a}' - 0;1.2^\circ \sin 2\bar{a}' + 0;9.2^\circ \cos \bar{a}' \ldots \qquad (25)$$

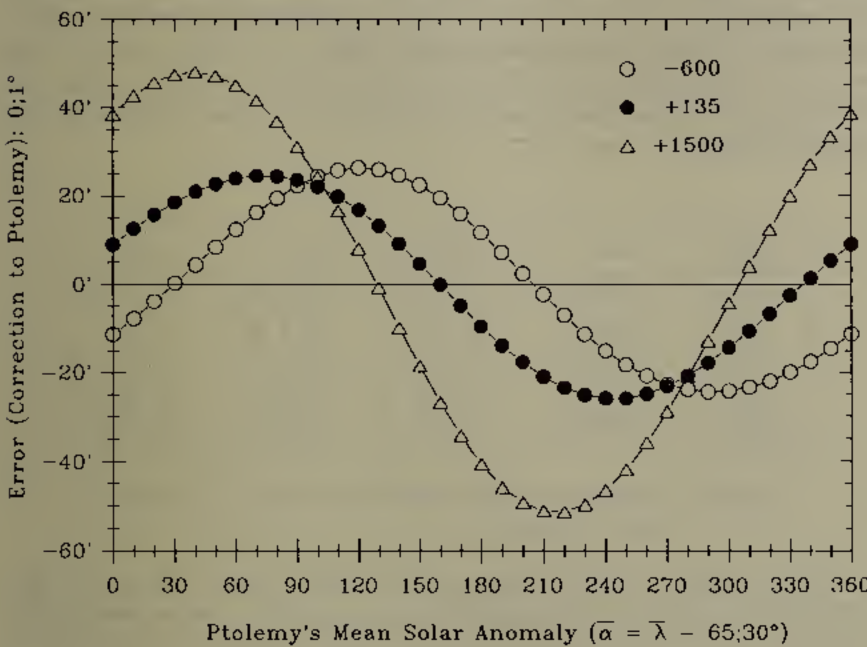

Figure 2.7. Error in Ptolemy's Solar Equation

This error is shown in Figure 2.7. Its amplitude is roughly +0;25° (at $\bar{a}' = 70^\circ$ and 245°and it is zero at $\bar{a}' = 160^\circ$ and 337° ($\bar{L}' = 225^\circ$ and 32°). Such an error could produce discrepancies of nearly an hour between computed and observed times of lunar eclipses. This error is substantially

[33] For +135, these are:

$2e = +2;\ 0^\circ$	acc. $= 1;59,58^\circ$	[Newcomb 1898, 1]
$e^2 = +0;\ 1,3$		
$e' = +2;23$	acc. $= 2;22,45$	for $e' = 0;\ 2,29,30^{P}$
	acc. $= 2;23,13$	for $e' = 0;\ 2,30$
$e'^2 = +0;\ 5,57$		
$B = -4;27.$		

offset at syzygy, however, by the Moon's annual equation,[34] so that the apparent error in Ptolemy's computed times of eclipses due to his solar model would be considerably smaller.

Since B changes with time, the coefficient of each term in (25) also changes. Table 2.9 shows the coefficients of each term for different dates. The amplitude of the error remains nearly constant throughout the period for which Ptolemy reports observations (−720 to +141). The phase of the error, however, shifts about 50° in the interval from −600 to +135, and at the time of Hipparchus the error is very nearly in phase with the Moon's annual equation.

Date	$\sin \bar{a}'$	$\sin 2\bar{a}'$	$\cos \bar{a}'$	$\cos 2\bar{a}'$
−600	+0;22.5°	−0; 1.3°	−0;11.5°	+0; 0.2°
−210	+0;23.0	−0; 1.2	0; 0	0; 0
−140	+0;23.0	−0; 1.2	+0; 1.1	0; 0
+135	+0;23.4	−0; 1.3	+0; 9.3	−0; 0.2
+1500	+0;30.0	−0; 1.5	+0;39.6	−0; 1.1

Table 2.9. Coefficients of the Terms in the Error in Ptolemy's Solar Equation

For later dates, and particularly in the medieval period, the error is significantly out of phase with the Moon's annual equation and is considerably larger than at Ptolemy's time. Thus, the error in the times of eclipses computed from Ptolemy's tables in +1000 could amount to nearly $1\,^1\!/_2{}^{\text{h}}$ from the error in his solar inequality alone.[35]

In summary, for the period of Ptolemy's own observations (+125 to +140) the correction to his tabular longitudes of the Sun is (±0;3°)

$$L - L' = +1;5^\circ + (g - g')_{+135}, \tag{26}$$

while for the time of Hipparchus (ca. −140), the error is

$$L - L' = -0;4^\circ + (g - g')_{-140}. \tag{27}$$

Either directly or indirectly, Ptolemy determines the longitudes of all other celestial bodies with reference to the Sun. Consequently, we would

[34] The correction for the Moon's annual equation at syzygy is $+0;14,3^\circ \sin \bar{a}$. Thus, it will be very nearly in phase with the error in Ptolemy's solar inequality, when $B(T) = 0$ in roughly −210.

[35] Cf. al-Battānī [Nallino 1903–1907, i 56–57].

expect to find at least the secular component of the error in his solar model in all of his observed longitudes and, thus, in all of his other models. We should also expect his star positions to reflect this same error, since he tells us [*Alm*. vii 2: Toomer, 328] that he found the longitudes of the bright stars along the ecliptic by measurements with reference to the Moon and, hence, indirectly to the Sun.

Lunar Observations in the *Almagest*: Errors in the Observations and Derived Data

The *Almagest* reports thirty-seven dated observations of the Moon in detail. Twenty-six of them (19 eclipses and 7 occultations) involve only determining the time at which some phase of an event occurred (together with the magnitude in the case of partial eclipses). All the others, except for Ptolemy's determination [*Alm.* v 13] of the Moon's parallax,[1] entail measuring the distance from the Moon to another body at a specific time. Ptolemy [*Alm.* v 12: Toomer 247] also describes his determination of the Moon's greatest northern latitude without mentioning the date of the observations,[2] and he refers [*Alm.* iii 1: Toomer, 135] to two lunar eclipses

[1] The observation was made on +135 Nov 1. Ptolemy finds the Moon's parallax on the meridian at Alexandria to be 1;7° and the Moon's distance from the center of the Earth to be 39;45 Earth radii. Since the Moon was actually near its mean distance, Ptolemy should have found its parallax to be ≈ 0;45°, so that his observation was in error by roughly 0;20°. It is interesting that from this observation Ptolemy deduces a mean distance of the Moon at syzygy, 59 Earth radii, which agrees very well with the modern value, 60.3 Earth radii.

[2] A date for this observation can be inferred from Ptolemy's statement [*Alm.* v 12] that the Moon was simultaneously near the summer solstice (90°) and also near the northern limit of its orbit. To satisfy these conditions, the Moon's ascending node must have been near Aries 0°.

For Ptolemy's time the condition is satisfied in +126 and in +145, the best date for the observation being +126 Aug 3. On this day the Moon culminated about 2 hours before noon with a longitude of 88°, while the position of its ascending node was Aries 0;1°. The observation could have been made a month or so on either side of this date, but the longitude of the Moon at culmination would have been observed by Hipparchus without reporting any details of the observations except the longitudes of Spica which Hipparchus derived from them.[3]

I have limited the following discussion to the dated observations of lunar eclipses, occultations, and elongations, since these observations are completely described, and since Ptolemy's determination of the Moon's parallax is a unique observation which does not fit into any of the other groups. I consider first the eclipses and occultations, which several astronomers have already compared with modern theory and which enable us to evaluate the accuracy of two groups of ancient time-determinations. Then, I discuss the errors in the observations of elongations which involve measurements of both time and arc. These errors have not been investigated previously.

In comparing the lunar observations with modern computations, my objective is to determine, first, the accuracy of the observations which Ptolemy had available to him and, then, the errors in the data Ptolemy uses to determine or demonstrate specific features of his models. The two problems are not quite identical, partly because Ptolemy introduces additional errors in reducing the observations, and partly because the reports themselves are often sufficiently ambiguous to allow several interpretations; indeed, a few of them contain inconsistencies which raise considerable doubt as to their proper interpretation. In general, these difficulties arise only in connection with the observations made by Ptolemy's predecessors and, in particular, the eclipses and occultations where (a) either the time of a specific phase may be uncertain, or (b) the phase associated with a stated time may be uncertain.

The first type of uncertainty can arise either from the vagueness of the time-reference (e.g., 'after rising' in the case of the eclipse of −719 Sep 1 [*Alm.* iv 6: Toomer, 192]) or from an over-determined and inconsistent time-designation (e.g., the occultation of Spica observed by Timocharis in −282 Nov 9 [*Alm.* vii 3: Toomer, 336], where the designation '9½ hours [after Sunset]' and 'just as the Moon was rising' differ by more than an hour). There is also some ambiguity in the meaning of the phrases ὥρας ἀρχομένης and ὥρας ληγούσης, which are frequently applied to times designated in seasonal hours. The question is whether such times should be understood as 'near the beginning' or 'towards the end' of the given hour, or 'at' the beginning or end of the hour. Fotheringham [1915a, 280] has discussed the alternatives and interprets the adjectives, ἀρχομένης and ληγούσης, as referring to the first and last thirds of the stated hour. He then

less satisfactory. This is the earliest date of an observation made by Ptolemy [cf. *Alm.* iv 9: Toomer, 206n54].

[3] See Rome 1937–1938, 6; 1931–1943, iii 828, for discussion of these eclipses.

uses the midpoints of these intervals as the observed time-data. Ptolemy, on the other hand, understands such designations to mean the beginning or end of the hour, and this interpretation was followed by Newcomb [1878, 35].

Another source of uncertainty in the reported times is the occasional ambiguity of the units in which time-intervals are given and of the times of day to which these intervals are referred. Three systems for measuring time are used in describing the observations in the *Almagest*. The most convenient of these, and the one Ptolemy always uses to describe the times of his own observations, states the number of equinoctial hours ($1^h = {}^1/_{24}$ day) between noon or midnight and the time of the observation. Times given in this system thus require no seasonal correction except for the equation of time.

A variation of this system (historically its predecessor) is encountered in Babylonian astronomical Diaries.[4] Here the time of an event (e.g., Moonrise or the beginning of an eclipse) is given in terms of an interval measured in equinoctial units with respect to Sunset or Sunrise. The Babylonian units of time were the UŠ, equal to ${}^1/_{6;0}$ days, or 4 minutes, and the DANNA (KAS.BU) or *bēru*, equal to 30 UŠ or 2 equinoctial hours.[5] Times given in this system thus require a seasonal correction, equal to the variation in the time from Sunrise or Sunset to noon (or midnight), as well as the equation of time, in order to reduce them to a uniform system. One observation in the *Almagest* [iv 9: Toomer, 208] which explicitly gives the time in this system is the Babylonian eclipse of −501 Nov 19/20, which 'took place when $6\,{}^1/_3$ equinoctial hours of the night had passed'. Here the unit of measurement is Ptolemy's hour, ${}^1/_{24}$ of a day, so that the original Babylonian report has been partially modified at least.

The third system of time-measurement, local civil time, was used for reporting nearly all the pre-Ptolemaic observations. In this system the unit of time is the seasonal hour (s.h.), defined as ${}^1/_{12}$ of the interval from Sunrise to Sunset (or Sunset to Sunrise) of the day on which an event occurs.[6] In this system, the time of an event is the interval between the

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[4] Cf. Sachs 1948, 285, for a definition and description of the Babylonian astronomical Diaries. Kugler [cf., e.g., 1907–1924, i 76–77] calls these *Beobachtungstafeln*. Diaries from −651 to +165 have been published in Sachs and Hunger 1988–1989.

[5] For discussions of these units, cf. Kugler 1907–1924, i 25, 272; ii 58–60, 68–71; O. Neugebauer 1955, i 39.

[6] Fotheringham [1932a, 338] and van der Waerden [1951, 20] have discussed evidence of Babylonian computations which seem to use a 'quarter-watch', equivalent to $1^{s.h.}$, as a unit of time. Both conclude that seasonal hours were used in Babylonian astronomy. This is a hazardous inference from very uncertain evidence.

event and Sunset or Sunrise (expressed in seasonal hours). Times in this system, therefore, require a twofold correction in addition to the equation of time in order to reduce them to uniform time, one in the variation in the length of the seasonal hour and the other for the time of Sunset or Sunrise. Ptolemy understands the times of all observations made by others than himself as given in this system, except for the eclipse of +125: Apr 5[7] and the Babylonian eclipses of −522 Jul 16 and −501 Nov 19/20. He generally describes such times in ordinal hours and fractions thereof, and he always explicitly designates these units as seasonal hours in discussing the reductions of these observations.

It is curious that most of the times of the Babylonian eclipses were reduced to this cumbersome system. The reduction serves no astronomical purpose, and it would have been far easier to convert the units given in the Babylonian reports or records directly into equinoctial hours. Since not all of the eclipse-times were reduced to this system, we may ask whether Ptolemy was mistaken in assuming that the times of some of the observations were given in seasonal hours. For this reason I have occasionally included calculations of the errors of the observed times, based on the assumption that the times given refer to equinoctial hours after Sunset, although Ptolemy understands seasonal hours.

Still another uncertainty, which affects several eclipse-observations, arises from Ptolemy's occasional assumption that the report he is citing refers to the midpoint of an eclipse when it says that an eclipse 'took place' at the given time. Instances of this include four of the Babylonian eclipses (−719 Mar 8, −522 Jul 16, −501 Nov 19/20, and −490 Apr 25) and the eclipse of +125 Apr 5. Nevill [1906, 2] and, following him, Cowell [906, 523] and Fotheringham [1920a, 578–579] have interpreted all times but that of −719 Mar 8 as specifying the beginning of the eclipse. Since there is at least one case, −381 Dec 12, in which Ptolemy takes the same vague description to refer to the beginning of the eclipse, and since the Babylonian diaries generally state the time of the beginning, but not the midpoint, of eclipses, I shall consider the possibility that Ptolemy may have been mistaken about the phase in these instances.

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[7] The earliest observation in the *Almagest* which Ptolemy explicitly claims to have made himself is his observation of an opposition of Saturn on +127 Mar 26. Since his observation of the Moon's extreme latitude probably antedates this [see 49n3 above], and since the time reported for this eclipse is given in equinoctial hours relative to midnight, it is quite possible that he himself observed the eclipse of +125 Apr 5 [*Alm.* iv 9: Toomer, 206]. He says only, however, that the eclipse was observed in Alexandria.

Finally, I should note that throughout this discussion the term 'error' (alternatively, Δ) denotes simply the difference between a datum calculated from modern theory and that reported by Ptolemy, always in the sense of a correction to Ptolemy's datum. Thus, the term embraces all sources of error in a given datum such as errors of measurement, recording, reduction, transmission, interpretation, and so on, in addition to any error in the modern theory on which the calculation is based.

ECLIPSE-OBSERVATIONS

In this section I consider Ptolemy's reported eclipse-observations individually in chronological order. Table 3.1 contains the following computed data used to determine the errors of these observations:

col. 1 Julian civil date when the midpoint of the eclipse occurred.

col. 2 Local apparent time (midnight epoch) of eclipse-midpoint for:
Babylon ($2;58^h$) E: 32;30° N
Alexandria ($2;0^h$) E: 31;12° N
Rhodes ($1;53^h$) E: 36;24° N.
For eclipses nos. 1 through 15, the times are taken from P. V. Neugebauer [1934, 13]; the times of eclipses nos. 16 through 19 are from Newcomb [1878, 42].

col. 3 Half-duration of the eclipse, for eclipses 1 through 15 from P. V. Neugebauer [1934]; for eclipses 16 through 19 from Cowell [1906, 526]. Cowell's computed durations are nearly identical with Newcomb's when the latter are corrected for the error in Hansen's argument of latitude.

col. 4 Correction to the mean tabular elongation at the time given in col. 2 which is needed to reduce the tabular elongation to my elements. For eclipses nos. 1–15 and 16–19, these are from appendix 2, Table A2.1 and Table A2.3, respectively.

col. 5 Velocity of the Moon's elongation at the time of the eclipse in seconds of arc *per* minute of time, as computed from Newcomb [1878, 41].

col. 6 Correction to the tabular time in col. 2 obtained by dividing col. 4 by col. 5. (The sign of the correction is positive, since the negative correction to the elongation at tabular eclipse-midpoint means the Moon must still travel ΔD to reach eclipse-midpoint.)

col. 7 Corrected local apparent time (midnight epoch) of eclipse-midpoint (col. 2 + col. 6).

No.	1 Date and Place	2 $T'(M)$	3 Dur.	4 ΔD	5 $\frac{\Delta D}{\Delta t}$	6 ΔT	7 $T(M)$	8 $T(B)$	9 $T(E)$	10 $\approx L_s$	11 $\frac{1}{2}N$	12 Mag.
1	−720 Mar 19 Babylon	21.5^h	1.9^h	$-555''$	$29.1''/^m$	$+0;19^h$	$21;49^h$	$19;55^h$		$351.5°$	$6;\ 9^h$	10.2^d
2	−719 Mar 8 Babylon	23.6	0.7	−555	27.1	+0;20	23;56	23;14		340.7	6;20	1.5
3	−719 Sep 1 Babylon	20.0	1.2	−555	35.4	+0;15	20;15	19; 3		159.9	5;30	6.1
4	−620 Sep 22 Babylon	5.25	0.85	−510	27.2	+0;19	5;34	4;43		24.4	5;35	2.1
5	−522 Jul 16 Babylon	23.65	1.35	−465	27.6	+0;17	23;56	22;35		106.6	4;58	6.1
6	−501 Nov 20 Babylon	0.1	0.8	−460	27.0	+0;17	0;23	23;35		231.9	6;51	2.1
7	−490 Apr 25 Babylon	22.75	0.65	−455	31.4	+0;14	22;59	22;20		28.5	5;32	1.7
8	−382 Dec 23 Babylon	0.0	0.9	−410	34.8	+0;12	8;12	7;18		267.0	7; 6	3.0
9	−381 Jun 18 Babylon	21.15	1.35	−410	27.5	+0;15	21;24	20; 3	$22;45^h$	80.5	4;57	5.9
10	−381 Dec 12 Babylon	23.05	1.75	−410	35.5	+0;12	23;15	21;30		256.2	7; 5	18.2
11	−200 Sep 22 Alexandria	18.9	1.5	−340	28.7	+0;12	19;12	17;42	20;42	176.0	5;56	8.5
12	−199 Mar 20 Alexandria	0.8	1.8	−340	32.0	+0;11	0;59	23;11		355.4	6; 5	16.0
13	−199 Sep 12 Alexandria	2.35	1.85	−340	32.3	+0;11	2;32	0;41		165.0	5;45	19.3
14	−173 May 1 Alexandria	1.8	1.3	−330	35.4	+0; 9	1;57	0;39	3;15	35.7	5;26	7.4
15	−140 Jan 27 Rhodes	21.65	0.85	−320	35.6	+0; 9	21;48	20;57		304.5	6;49	2.8
16	+125 Apr 5 Alexandria	20.75	0.77	−260	32.2	+0; 8	20;53	20; 7		14.3	5;52	1.8
17	+133 May 6 Alexandria	22.93	1.77	−257	28.2	+0; 9	23; 5	21;19		44.2		12.9
18	+134 Oct 20 Alexandria	22.93	1.57	−256	29.0	+0; 9	23; 5	21;31		206.3		10.1
19	+136 Mar 6 Alexandria	3.37	1.35	−254	34.6	+0; 7	3;29	2; 8		344.6		5.5

Table 3.1. Data for Comparing Eclipses

col. 8 Corrected local apparent time of the beginning of the eclipse, i.e., col. 7 − col. 3.

col. 9 Corrected local apparent time of the end of the eclipse, for those for which Ptolemy gives the time of the end, i.e., col. 7+ col. 3.

col. 10 Approximate longitude of the Sun from Newcomb [1878, 41].

col. 11 Half the length of the night in equinoctial hours, computed accurately from Ptolemy, *Alm.* ii 8 for the latitudes assumed above in col. 2. For the apparent half-length of the night, subtract 2^m from these values to correct for refraction.[8]

col. 12 Magnitude of the eclipses: nos. 1 through 15 are from P. V. Neugebauer [1934]; nos. 16, 18, 19 are from Fotheringham [1909c, 668]; and no. 17 is from Oppolzer [1962].

The precision of the times from P. V. Neugebauer is $\pm 0;3^h$, excluding the uncertainty of the secular accelerations. If we assume an uncertainty of $\pm 0.3''T^2$ in the secular acceleration of the mean elongation used in the comparison, the corresponding errors in the eclipse-times would be $\approx \mp 0;6^h$ at −600 and $\mp 0;4^h$ at −100. Further, the uncertainty in the argument of latitude can increase the error in the time of an initial or terminal phase by $\pm 0;2^h$. Finally, the error in the computed time of Sunrise or Sunset is estimated to be $\pm 0;2^h$. The probable error of a computed time, therefore, will be roughly $\pm 0;6.5^h$.

Eclipse 1. −720 Mar 19 *Alm.* iv 6: Toomer, 191

1 Mardokempados: 29/30 Thoth[9]

The eclipse began, [the report] says, well over an hour after Moonrise, and was total.

[8] Fotheringham [1915a, 381] and Schoch [1926, 32] understand 'Sunrise' and 'Sunset' to mean the appearance or disappearance of the Sun's upper rim, in which case the half-length of the night should be further reduced by $0;1^h$. I cannot determine on what basis they make this assumption, and I have assumed rising or setting to refer to the center of the body in question.

[9] To be read as 'year 1 of the reign of Mardokempados, on the night between Thoth 29 and Thoth 30'. Ptolemy uses a continuous Egyptian calendar, the epoch of which is Thoth 1, Nabonassar 1 (= −746 Feb 26, JDN 1488638). The number of days between an event and this epoch can be obtained by first finding the number of intervening Egyptian years (each of 365 days) from Ptolemy's List of Reigns [Ginzel 1906-1914, i 139] and then adding the number of days between

Lunar Eclipse-Data	Computed[a]	Ptolemy	Δ
Sunset (Babylon)	$17;53^h$	$18;\ 0^h$	$-0;\ 7^h$
Moonrise (Babylon)[b]	17;40		
Beginning (Babylon)	19;55	19;30	+0;25
Midpoint at Babylon	21;49	21;30	+0;19
Alexandria	20;51	20;40	+0;11
Magnitude	18.2^d	$(21.6)^d$	$(-3.4)^d$

[a] All computed times are given in the local apparent time (midnight epoch) of the place indicated. The times of risings and settings refer to the center of the body indicated and are corrected for refraction.

[b] Newcomb, following Zech [1851, 13], puts Moonrise at $17;53^h$ mean time. This is consistent with $17;40^h$ local apparent time when refraction is taken into account.

Eclipse No. 1: −720 Mar 19

Ptolemy assumes the night at Babylon was 12^h long and that the eclipse began $1\frac{1}{2}^h$ after Sunset $1;45^h$ after Moonrise. He also assumes the eclipse was central and computes the duration as 4^h.[10]

The time given for this eclipse has caused substantial difficulties for modern investigators. Nevertheless, there is general agreement that Ptolemy's

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Thoth 1 and the date of the event. The order of the months is:

I	Thoth	VII	Phamenoth
II	Phaophi	VIII	Pharmuthi
III	Athyr	IX	Pachon
IV	Choiak	X	Payni
V	Tybi	XI	Epiphi
VI	Mechir	XII	Mesore.

Each has 30 days and Mesore is followed by 5 epagomenal days. In computing the number of days since Thoth 1, it must be remembered that Thoth 1 counts as day zero. Furthermore although Ptolemy's calendar assumes a midnight epoch, he uses noon on Thoth 1, Nabonassar 1 as the epoch of his mean motion tables. This has the advantage that all observations made at night can be reduced on a uniform basis without considering whether they were made before or after midnight.

[10] Indeed, according to Manitius [1912–1913, i 433n28], the duration computed from Ptolemy's tables, assuming a central eclipse, is $3;59,45^h$. See also Toomer, 191n30.

estimate of the elapsed time since Moonrise, 1 1/2 hours, is the maximum time consistent with the description well over one hour, although Ptolemy applies this interval to Sunset rather than Moonrise. Newcomb [1878, 35–36] assumes that the report indicates an interval of between 1 1/4 and 1 1/2 hours after Moonrise, and finds the difference between the observed and computed time to be:

Lunar Datum	Computed	Observed	Δ
Time since Moonrise	$2;15^{h}$	$1;22^{h}$	$+0;53^{h}$

On the other hand, if we follow Ptolemy's interpretation applied to accurate Sunrise, the error (Δ) is $+0;32^{h}$.

Kugler [1907–1924, ii 68] has suggested that the time accepted by Ptolemy can be explained by assuming that the original report said only that the eclipse occurred in the first watch and that its total phase ended before the end of the first watch; this would require the eclipse to have begun less than $1;40^{h}$ after Sunset. Although Kugler correctly remarks that the unit, $1^{h} = 1/24$ day, is not a Babylonian unit [see 50n6, above], his explanation still does not account for the description Ptolemy quotes. An alternative explanation is that whoever transmitted this report mistakenly translated 1 KAS.BU (DANNA = double hour) into 'well over one hour'. If so, the time, 1 double hour after Moonrise, would agree very closely with the computed time. If Ptolemy's report more or less accurately represents the Babylon account, however, the error is nearly an hour.

Eclipse 2. −719 Mar 8 *Alm.* iv 6: Toomer 192

2 Mardokempados: 18/19 Thoth

> The [maximum] obscuration, [the report] says, was 3 digits from the south exactly at midnight.

Kugler [1907–1924, ii 69] assumes that the original report may have indicated that the greatest phase occurred when the Moon was on the meridian, and notes examples from other texts to demonstrate this possibility. Since the eclipse was of short duration ($1;24^{h}$), determining the time by reference to the meridian would make the observation's probable error much less than we might otherwise expect from the somewhat vague description.[11] In any

[11] Newcomb [1878, 36] assumes a probable error of $\pm 40^{m}$, equal to half the duration of the eclipse. As a result, he gave the eclipse very little weight in his subsequent analysis.

case, the stated time agrees very well with the computed time if we assume that the observed time referred to mid-eclipse.

Lunar Eclipse-Data	Computed	Ptolemy	Δ
Midpoint at Babylon	$23;56^h$	$24;\ 0^h$	$-0;\ 4^h$
Alexandria	22;58	23;10	$-0;12$
Magnitude	1.5^d	3.0^d	-1.5^d

Eclipse No. 2: −719 Mar

Eclipse 3. −719 Sep 1 *Alm.* iv 6: Toomer, 192

2 Mardokempados: 15/16 Phamenoth

> The eclipse began, [the report] says, after Moonrise, and the [maximum] obscuration was more than half from the north.

Ptolemy concludes that the eclipse began at least half but less than one (equinoctial) hour after Moonrise, implying that a smaller or greater interval would have been specifically mentioned. He then adopts half an (equinoctial) hour after Sunset ($\approx 0;40^h$ after Moonrise) as his beginning time. Newcomb [1878, 36] assumes $0;25^h$ after Moonrise as most probable. Ptolemy computes the duration to be 3 hours, equivalent to an assumed magnitude of 8^d by his tables. Ptolemy's assumption that the eclipse began half an hour after Sunset is in excellent agreement with the computed time.

Lunar Eclipse-Data	Computed	Ptolemy	Δ
Moonrise (Babylon)	$18;24^h$		
Sunset (Babylon)	18;23	18;30	
Beginning (Babylon)	19; 3	19; 0	$+0;\ 3^h$
Time since Moonrise	0;39	0;30[a]	+0; 9[a]
Sunset	0;31	0;30	+0; 1
Midpoint at Babylon	20;15	20;30	$-0;15$
Alexandria	19;17	19;40	$-0;25$
Magnitude	6.1^d	$[8.0^d]$	$[-1.9^d]$

[a] With Newcomb's estimate of $0;25^h$, the error is $+0;14^h$.

Eclipse No. 3: −719 Sep 1

Eclipse 4. −620 Apr 22 *Alm.* v 14: Toomer, 253

5 Nabopolassar: 27/28 Athyr

> at the end of the eleventh hour in Babylon, the Moon began to be eclipsed; the maximum obscuration was one quarter of its diameter from the south.

This report is notably more precise than those preceding it. Ptolemy takes $5^{s.h.}$ after midnight as the time of beginning and $6^{s.h.}$ after midnight (i.e., $5;50^h$) as the time of mid-eclipse [cf. Manitius 1912–1913, 307na; Toomer, 253n56].

Lunar Eclipse-Data	Computed	Ptolemy	Δ
Sunset (Babylon)	$18;27^h$	$18;10^h$	
Beginning at Babylon	4;43	$(4;52)^a$	$(-0;\ 9)^h$
$5^{s.h.}$ after midnight		$4;38^b$	+0; 5
$4;50^{s.h.}$ after midnight		$4;27^c$	+0;14
Midpoint at Babylon	5;34	$5;50^d$	−0;16
Alexandria	4;36	5; 0	−0;24
Magnitude	2.1^d	3.0^d	-0.9^d

[a] As Ptolemy's reduction implies. [b] Computed.

[c] According to Fotheringham's assumption.

[d] Given $6^{s.h.}$ accurately computed, mid-eclipse (Babylon) is at $5;33^h$ and the error is $+0;1^h$.

Eclipse No. 4: −620 Apr 22

Eclipse 5. −522 Jul 16 *Alm.* v 14: Toomer, 253

7 Cambyses: 17/18 Phamenoth

> one hour before midnight in Babylon, the Moon was eclipsed from the north half of its diameter.

This is the only eclipse Ptolemy reports which is also mentioned in an extant cuneiform text [Strm. Kambys. 400 rev.]. This text, which was published by Kugler [1907–1924, i 71], differs from the general form of Babylonian astronomical Diaries [cf. Sachs 1948, 271 ff.] and, as Kugler remarks [1900, 65], seems to contain both computed and observed data concerning the Moon and planets. Kugler translates the description of the eclipse as follows:

> Year 7, month IV, night of the 14th, 1⅔ double hours after the beginning of the night a lunar eclipse; the whole course is visible; it was eclipsed from the north more than one half.

According to Professor A. Sachs (private communication), a correct reading of Kugler's transcription is:

> Year 7, month IV, night of the 14th, 1⅔ double hours in the night a 'total' lunar eclipse took place [with only] a little remaining [uneclipsed]. The north wind blew.

Lunar Eclipse-Data	Computed	Ptolemy	Babylonian	Δ
Sunset (Babylon)	19; 4^h			
Beginning at Babylon	22;35		22;24^h	+0;11^h
1^h before midnight		23; 0^h		−0;25
$1^{s.h.}$ before midnight		23;11		−0;36
Midpoint at Babylon	23;56			
1^h before midnight		23; 0		+0;56
$1^{s.h.}$ before midnight		23;11		+0;45
Midpoint at Alexandria	22;58	22;10		+0;48
Magnitude	6.1^d	6.0^d	($\approx 11.0^d$)	$+0.1^d$

Eclipse No. 5: −522 Jul 16

Ptolemy assumes that the time which he quotes refers to mid-eclipse, and in subsequent calculations he takes 'hour' to mean an equinoctial hour. Fotheringham [1932a, 338] and van der Waerden [1951, 25] draw attention to the discrepancy between the time stated by Ptolemy and that given in the Babylonian text, and both offer the explanation that the time was converted to seasonal hours in Babylon in accordance with a crude scheme for the length of daylight (or night) based on the ratio 2:1 for the lengths of the longest and shortest day. By this explanation the time, the unit of time, and the phase described by Ptolemy are all incorrect. However, the discrepancy between the observed magnitude in the Babylonian text and that given by Ptolemy (which agrees very well with the computed magnitude) makes it difficult to draw any secure conclusions from this text alone.

The phase assumed by Ptolemy and the magnitude reported in the Babylonian text are clearly incorrect, while the Babylonian and computed times for the beginning of the eclipse are in good agreement. Such close agreement may well be fortuitous, since the same text describes another eclipse (−521 Jan 10) as follows (translated by A. Sachs):

> Month X, night of the 14th, 2½ double hours of the night remaining to dawn, a total lunar eclipse took place. During it the south and the north wind blew.

From P. V. Neugebauer [1934] we find, with the corrections from appendix 2 (below):

Lunar Eclipse-Data	Computed	Babylonian	Δ
Sunrise (Babylon)	7; 1^h		
Beginning (Babylon)	3; 2	2; 1^h	+1; 1^h
Magnitude	22.1^d	Total	

All in all the Babylonian text raises more problems than it solves. We may conclude only that Ptolemy's description of the magnitude and the Babylonian time of beginning agree with modern theory, and that the time Ptolemy uses in his computation is badly in error.

Eclipse 6. −501 Nov 19/20 *Alm.* iv 9: Toomer, 208

20 Darius I: 28/29 Epiphi

> The eclipse, which Hipparchus also used, occurred... when 6⅓ equinoctial hours of the night had passed. At this [time] the Moon was obscured from the south one quarter of its diameter.

As in eclipse no. 5, Ptolemy assumes that the time refers to mid-eclipse. Here too the errors strongly favor the assumption of a mistaken phase.

Lunar Eclipse-Data	Computed	Ptolemy	Δ
Sunset (Babylon)	$17;11^h$	($17;15^h$)	(−0; 7^h)
Beginning (Babylon)	23;35	23;31[a]	+0; 4
Midpoint at Babylon	24;23	23;35[b]	+0;48[b]
Alexandria	23;25	22;45	+0;40
Magnitude	2.1^d	3.0^d	-0.9^d

[a] Assuming a mistake in Ptolemy's interpretation of the phase, and $6;20^h$ after actual.

[b] The time given is Ptolemy's datum: $6;20^h$ from actual sunset equals $23;31^h$. The error is thus $0;52^h$.

Eclipse No. 6: −501 Nov 19/20

Eclipse 7. −490 Apr 25 *Alm.* iv 9: Toomer, 206

31 Darius I: 3/4 Tybi

> at the middle of the sixth hour. It is reported that at this eclipse the Moon was obscured 2 digits from the south.

Ptolemy again assumes that mid-eclipse is meant, and takes half an hour before midnight as his datum. The only assumption that would reconcile this report with the computed times is the unlikely one that the times refer to the end of the eclipse, in which case the error would be $+0;8^h$. In describing this eclipse (but not eclipse nos. 5 or 6) Ptolemy seems to indicate that the report explicitly gives the time of maximum phase, so we should probably accept this phase.

Lunar Eclipse-Data	Computed	Ptolemy	Δ
Sunset (Babylon)	$18;30^h$		
Length of seasonal hour	0;55		
Beginning (Babylon)	22;20		
Midpoint at Babylon	22;59	$23;30^h$ [a]	$-0;31^h$ [a]
Alexandria	22; 1	22;40	$-0;30^h$
Magnitude	1.7^d	2.0^d	-0.3^d

[a] $23;32^h$ using $5;30^{s.h.}$ accurately computed; the error is $-0;33^h$.

Eclipse No. 7: −490 Apr 25

Eclipse 8. −382 Dec 23 *Alm.* iv 11: Toomer, 211–212

Archonship of Phanostratos: Month of Poseidon

> a small section of the disk of the Moon was eclipsed from the [north-east], when half an hour of night was remaining. He (Hipparchus) adds that it was still eclipsed when it set.

This is the first of three eclipses which Ptolemy notes that Hipparchus selected from those 'brought over from Babylon and [which] were observed there'. The fact that these eclipses are dated according to the Athenian calendar [cf. Toomer, 211n63], and the further difficulty that this eclipse would have been difficult if not impossible to observe in Babylon, led Oppolzer [1881, 32] to assume that all three were observed in Athens and the times erroneously reduced to Babylon. Van der Waerden [1958] discusses this question and shows that there is no evidence to support Oppolzer's improbable assumption. Nevill [1906, 2] assumes that only the first was observed in Athens, which makes even less sense. It seems reasonable to conclude that the eclipses were known to astronomers in Athens, which suggests that at least these three eclipses were known in Greece before Alexander the Great.

Ptolemy assumes that the eclipse began $5\,{}^1\!/_2^{\text{s.h.}}$ after midnight and that the duration could have been no greater than $1;30^{\text{h}}$.

Several early investigators[12] considered this eclipse to be the most critical of those reported by Ptolemy for determining the secular acceleration of the Moon, since a substantially larger acceleration is required to make it at all visible at Babylon.[13]

Newcomb [1878,43], in discussing a correction to Hansen's acceleration similar to that deduced above, comments that

> The question whether eclipse no. (8) was really seen is a very serious one.... the serious point is not simply that no. (8) gives a negative result, for this might arise from accidental errors of observation, but that a positive correction to the time will render the eclipse absolutely invisible at Babylon. In fact, the account says that there was a small eclipse (not simply that the eclipse was beginning) half an hour before Sunrise. At this time however, the twilight would have been so bright, and the altitude of the Moon so low, that the eclipse could not be seen for a number of minutes after its commencement....
>
> We have therefore this dilemma: either there is a mistake about the eclipse of −382, December 23, having really been observed at Babylon, or the seventeen good observations of phases cited by Ptolemy are systematically in error by nearly half an hour. I cannot hesitate to accept the former as the most probable alternative. The occurrence of the eclipse being expected, it is quite possible that observers may have thought they saw the Moon eclipsed in the increasing daylight when there was really no eclipse; or, under

[12] E.g., Dunthorne [1749, 169] and Lalande [1757, 429]. The year of this eclipse according to Dunthorne is misprinted as 313 BC. Lalande fails to notice this error. Bernoulli [1773, 183] and Lagrange [1773, 50] give the correct date. Dunthorne's error is merely a misprint, as may be seen from his computations.

[13] P. V. Neugebauer [1934] also makes it questionable whether this eclipse was visible at Babylon. For it to have begun half an hour before sunrise at Babylon would require a correction to Schoch's acceleration of the Moon's mean elongation (1900) of $\simeq +3''T^2$.

> the unfavorable circumstances they may have been deceived by a dark region of the lunar disk being near the Moon's limb.... On the whole, I think that this eclipse should be rejected, since, if we regard it as a real observation, the results from the other eclipses must be regarded as all wrong.

Lunar Eclipse-Data	Computed	Ptolemy	Δ
Sunrise (Babylon)	$7;\ 4^{h}$ [a]	$7;12^{h}$	$-0;\ 8^{h}$
Beginning (Babylon)	7;18	6;36	+0;42
Beginning assuming			
$0;30^{s.h.}$ from computed Sunrise		6;28	+0;50
$0;30^{h}$ from computed Sunrise		6;34	+0;44
Midpoint at Babylon	8;12	6;30	+0;52
Alexandria	7;14	6;30	+0;44
Magnitude	3.0^{d}	$\approx 2.0^{d}$	

[a] Apparent Moonset at $7;5^{h}$.

Eclipse No. 8: −382 Dec 23

Newcomb's argument fairly states the difficulties with this eclipse. It is also possible that all three anomalous observations were in fact computations by the Babylonians, perhaps misinterpreted in transmission.[14]

Eclipse 9. −381 Jul 8 *Alm.* iv 11: Toomer, 212

Archonship of Phanostratos: Month of Skirophorion

> [the Moon] was eclipsed from the [northeast] when the first hour was well advanced.... And since the duration of the whole eclipse was reported as three hours,...

Ptolemy assumes that the eclipse began half a seasonal hour ($0;24^{h}$) after Sunset. There is some uncertainty among translators as to the meaning of προεληλυθυίας. Newcomb [1878, 38], apparently relying upon Halma's translation [1813–1816, i 277], *déjà passée*, gives 'the first hour having

[14] See Aaboe and Sachs [1969, 19–20] for examples of calculated times of solar eclipses for −474 to −456, none of which were observed. These calculations apparently antedate (and in any case do not reflect) the procedures of the fully developed Babylonian lunar theory, and are subject to errors of several hours. Nevertheless, they suggest the opportunities for misinterpretation of 'observational' reports.

passed'. Toomer, Manitius [1912–1913, 248], and Ptolemy, however, take the report to mean that the first hour had not passed. I shall assume $0;50^{s.h.}$ as the time indicated by the report. Since the actual time from Sunrise to beginning was $\approx 0;58^h$ or $1;12^{s.h.}$, better agreement would result from assuming either that the time should have been understood to mean equinoctial hours or that Halma's interpretation is correct.

Lunar Eclipse-Data	Computed	Ptolemy	Δ
Sunset (Babylon)	19; 5^h	(19;12^h)	(−0; 7^h)
Beginning (Babylon)	22; 3	19;36	+0;27
Beginning assuming $0;50^{s.h.}$ from computed Sunset		19;46	+0;17
Duration	2;42	3; 0	−0;18
Midpoint (Alexandria)	20;26	20;15	+0;11
Magnitude	5.9^d		

Eclipse No. 9: −381 Jun 18

Eclipse 10. −381 Dec 12 *Alm.* iv 11; Toomer, 213

Archonship of Euandros, Month of Poseidon

> [the Moon] was totally eclipsed, beginning from the [northeast], after four hours had past.

It should be noted that Ptolemy here speaks of cardinal rather than ordinal hours, which he generally uses when he mentions seasonal hours. Manitius again translates the time as 'late in the fourth hour', as in the case of eclipse no. 9, but (as noted by Toomer, 213n68), παρεληλυθυιῶν seems to indicate that 4 hours had gone by. Ptolemy understands $3\frac{1}{2}^{s.h.}$ after Sunset as the time of beginning, which is consistent with his 4;12 equinoctial hours (accurately, $4;7^h$). Thus, we are invited to assume that in this case the time is given in equinoctial hours. Alternatively, if we assume that $4^{s.h.}$ were meant, the time should be closer to $4;42^h$ after Sunset. Ptolemy estimates the duration to have been 4 hours.

The errors offer no basis for choosing between the alternative systems for reckoning time. The extreme alternatives, i.e., '4 equinoctial hours having passed' ($\Delta = 0;33^h$) and 'the fourth seasonal hour having past' ($\Delta = -0;9^h$), both give results within the plausible limits of error. At best, all we can say of eclipses nos. 9 and 10 is that the underlying observation (or computation) could have been quite accurate, but the ambiguity of the reported time imparts an uncertainty of roughly $\pm 20^m$ to the report.

Lunar Eclipse-Data	Computed	Ptolemy	Δ
Sunset (Babylon)	$16;57^h$	$16;48^h$	$-0; 9^h$
Beginning (Babylon)	21;30	21; 0	+0;30
Beginning assuming			
$4;\ 0^h$ after computed Sunset		20;57	+0;33
$3;30^{s.h.}$ after computed Sunset (Ptolemy)		21; 4	+0;26
$4;\ 0^{s.h.}$ after computed Sunset		21;39	−0; 9
Duration	3;30	4; 0	−0;30
Midpoint (Alexandria)	22;17	22;10	+0; 7
Magnitude	18.2^d	Total	

Eclipse No. 10: −381 Dec 12

Eclipse 11. −200 Sep 22 *Alm.* iv 11: Toomer, 214

54 Callipic Period II: 16 Mesore

the Moon began to be obscured half an hour before it rose and its full light was restored in the middle of the third hour.

Both the time reported for the observed phase (end) and the estimated (or computed?) duration agree extremely well with the computation.

Lunar Eclipse-Data	Computed	Ptolemy	Δ
Sunset (Alexandria)	$18;\ 6^h$	$18;\ 0^h$	$(+0;\ 6^h)$
Moonrise	18; 2		
Beginning (Alexandria)	17;42	$17;30^a$	+0;12
from computed Moonrise		$17;32^a$	+0;10
Midpoint (Alexandria)	19;12	19; 0	+0;12
End (Alexandria)	20;42	20;36	+0; 6
Duration	3; 0	3; 4	−0; 4
Magnitude	8.5^d		

[a] Estimated value.

Eclipse No. 11: −200 Sep 22

Eclipse 12. −199 Mar 19/20 *Alm.* iv 11: Toomer, 214

54 Callipic Period II: 9 Mechir

[the eclipse] began when 5⅓ hours of the night had passed, and was total.

Ptolemy assumes the duration to be 4 equinoctial hours,and his subsequent computation confirms that here προελθουσῶν must mean 'had passed'. Again, the reported and computed times agree very closely.

Lunar Eclipse-Data	Computed	Ptolemy	Δ
Sunset (Alexandria)	$17;57^h$	$18;\ 0^h$	
Beginning (Alexandria)			
Mar 19	23; 1	23;20	$-0;\ 9^h$
from computed Sunrise, Mar 20		23;17	−0; 6
Midpoint (Alexandria) Mar 20	0;59	1;20	−0;21
Magnitude	16.0^d	Total	

Eclipse No. 12: −199 Mar 19/20

Eclipse 13. −199 Sep 12 *Alm.* iv 11: Toomer, 215

55 Callipic Period II: 5 Mesore

[the eclipse] began when 6⅔ hours of the night had passed, and was total.

Ptolemy accepts $3;20^{s.h.}$ as the duration, a figure which he ascribes to Hipparchus. The principal error seems to have been in the reported duration.

Lunar Eclipse-Data	Computed	Ptolemy	Δ
Sunset (Alexandria)	$18;17^h$	$(18;15^h)$	
Beginning (Alexandria)	0;41	0;40	$+0;\ 1^h$
Beginning $6;40^{s.h.}$ after computed Sunset		0;38	+0; 3
Midpoint (Alexandria)	2;32	2;15	+0;17
Duration	3;42	3;12	+0;30
Magnitude	19.3^d	Total	

Eclipse No. 13: −199 Sep 12

In his reductions of nos. 8-13, Ptolemy compares his time-intervals between successive pairs of eclipses with those used by Hipparchus. Ptolemy's and Hipparchus' values for these intervals are taken without his correction for the equation of time. If the latter is included, all four intervals differ, and the amount of the difference is increased in each case. (Thus, it appears that Hipparchus did not apply a correction for the equation of time in his reduction of these eclipses.) See Table 3.2 for the intervals found by Ptolemy and Hipparchus.

I can find no consistent explanation of these differences. From the agreement between Ptolemy's and Hipparchus' values for the interval from eclipses nos. 8 to 9, it seems that Hipparchus also assumed that no. 9 began $0;30^{s.h.}$ after sunset. Ptolemy does not state what duration Hipparchus assumed for eclipse no. 10. Thus, the discrepancy in the interval from no. 9 to no. 10 could be due to Hipparchus' having assumed $3;30^h$ for the duration of no. 10 instead of $4;0^h$ as Ptolemy assumes.

Unfortunately, no such assumption will mitigate the discrepancies found for the last two pairs. Not only are the times of these eclipses reported with greater precision than times for the three earlier eclipses, but the duration is stated for each except no. 12. Thus, if the discrepancy in the interval between nos. 11 and 12 is attributed to different estimates of the duration of no. 12, the discrepancy in the time of eclipse no. 13 becomes $0;55^h$. On the whole, it seems most likely that, as Ptolemy suggests, the discrepancies are due to errors in Hipparchus' reduction of the observations.

Whatever the case, these intervals offer no secure information beyond that given in the reports themselves and in Ptolemy's reduction of them, for they evidently depend on computations by Hipparchus.

Between Eclipse Numbers	Days	Ptolemy with Equation of Time	Ptolemy without Equation of Time	Hipparchus	Ptolemy − Hipparchus without Equation of Time
8 and 9	177	$13;35^h$	$13;45^h$	$13;45^h$	$0;\ 0^h$
9 10	177	2; 0	1;55	1;40	+0;15
11 12	178	6;50	6;20	6; 0	+0;20
12 13	176	0;24	0;5[5]	1;20	−0;25

Table 3.2

Eclipse 14. −173 May 1 *Alm.* vi 5: Toomer, 283

7 Philometor: 27/28 Phamenoth

> from the beginning of the eighth hour to the end of the tenth in Alexandria, there was an eclipse of the Moon which reached a maximum obscuration of 7 digits from the north.

Ptolemy computes the midpoint of the eclipse as 2;20 equinoctial hours after midnight ($1^{s.h.} = 0;54^{h}$). In the following comparison, I alternately assume, (A) that the times refer to the beginning and end of the stated hours (accurately computed), and (B) that the times refer to the middle of the first and last third of these hours. Assumptions (A) and (B) lead to durations respectively greater and less than the computed duration. Since the mean error of the two phases is the same in either case, the agreement is not improved by assumption (B).

Lunar Eclipse-Data	Computed	Ptolemy	Δ
Sunset (Alexandria)	$18;36^{h}$	$(18;24^{h})$	$(+0;12^{h})$
Beginning (Alexandria)			
(A)	0;39	0;54	−0;15
(B)		1; 3	−2; 4
End (Alexandria)			
(A)	3;15	3;36	−0;21
(B)		3;27	−0;12
Duration (Alexandria)			
(A)	2;36	2;42	−0; 6
(B)		2;24	+0;12
Midpoint (Alexandria)	1;57	2;20	−0;23
Magnitude	7.4^{d}	7.0^{d}	$+0.4^{d}$

Eclipse No. 14: −173 May 1

Eclipse 15. −140 May 1 *Alm.* vi 5: Toomer, 284

37 Callipic Period III: 2/3 Tybi

> At the beginning of the fifth hour [of night] in Rhodes, the Moon began to be eclipsed; the maximum obscuration was 3 digits from the south.

Ptolemy assumes that the eclipse began $2^{s.h.}$ ($2;20^h$) before midnight (half the night at Rhodes = $7;0^h$), but in computing the eclipse-midpoint he takes the duration to have been only $1;0^h$.[15]

Several previous investigators have remarked upon the substantial error in the time reported for the beginning of this eclipse,[16] and Zech [1851, 19] has assumed that there is an error of one hour either in the time reported or in the phase ascribed to the observed time. Ptolemy too probably had difficulty with this eclipse, since he obtains exact agreement with his tables only by assuming that the duration was half as great as his tables give. On the assumption that the stated time should have referred to the eclipse-midpoint, the error becomes only $+0;1^h$. If we assume that the hour stated is incorrect, the resulting error is $+0;20^h$.

Lunar Eclipse-Data	Computed	Ptolemy	Δ
Sunset (Rhodes)	$17;13^h$	$(17;\ 0^h)$	$(+0;13^h)$
Beginning (Rhodes)	20;57	21;40	−0;43
Beginning $4;0^{s.h.}$ after computed Sunset		21;44	−0;47
Midpoint (Rhodes)	21;48	22;10	−0;22
Magnitude	2.8^d	3.0^d	-0.2^d

Eclipse No. 15: −140 May 1

Eclipse 16. +125 Apr 5 *Alm.* iv 9: Toomer, 206

9 Hadrian: 17/18 Pachon

> the second eclipse ... [was] observed in Alexandria ... 3 3/5 equinoctial hours before midnight. At this eclipse too the Moon was obscured 1/6 th of its diameter from the south.

Ptolemy assumes that the time refers to the midpoint of the eclipse. The comparison with the computed times gives somewhat better agreement, on the assumption that the time refers to its beginning; but the difference is insufficient to conclude that Ptolemy misinterpreted the report. There is no direct evidence that Ptolemy himself observed this eclipse, and Toomer [206n54] conjectures that it was observed by Theon who gave Ptolemy

[15] Manitius [1912, i 450n44] finds the half-duration to be $0;58^h$ from Ptolemy's tables. See also Toomer, 284n23.

[16] Cf. Fotheringham's summary in 1920, 379.

some planetary observations. In view of the frequency of errors in reducing observed times to eclipse-midpoint, it is possible that as much as half of this error arose from this source.

Lunar Eclipse-Data	Computed	Ptolemy	Δ
Beginning (Alexandria)	$20;\ 7^h$	$20;24^h$ [a]	$-0;17^h$
Midpoint (Alexandria)	20;53	20;24	+0;29
Magnitude	1.8^d	2.0^d	-0.2^d

[a] Assuming that the time stated is for the beginning of the phase.

Eclipse No. 16: +125 Apr 5

Eclipse 17. +133 May 6 *Alm.* iv 6: Toomer, 198

17 Hadrian: 20/21 Payni

from those very carefully observed by us in Alexandria.... We computed the exact time of mid-eclipse as $^3/_4$ of an equinoctial hour before midnight. It was total.

Lunar Eclipse-Data	Computed	Ptolemy	Δ
Beginning (Alexandria)	$21;19^h$		
Midpoint (Alexandria)	$20;\ 5$	$23;15^h$	$-0;10^h$
Magnitude	$\approx 12.9^d$	Total	

Eclipse No. 17: +133 May 6

Eclipse 18. +134 Oct 20 *Alm.* iv 6: Toomer, 198

19 Hadrian: 2/3 Choiak

We computed that mid-eclipse occurred 1 equinoctial hour before midnight. [The Moon] was eclipsed $^5/_6$ of its diameter from the north.[17]

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[17] Newcomb [1878, 40] misreading Halma's somewhat obscure translation [1813–1816, i 255] gives the magnitude 'one third of its diameter'. Cowell [1906, 527] repeated Newcomb's error; Fotheringham [1909, 666] noticed and corrected it.

Lunar Eclipse-Data	Computed	Ptolemy	Δ
Beginning (Alexandria)	$21;31^h$		
Midpoint (Alexandria)	$23;\ 5$	$23;\ 0^h$	$+0;\ 5^h$
Magnitude	10.1^d	10.0^d	$+0.1^d$

Eclipse No. 18: +134 Oct 20

Eclipse 19. +136 Mar 6 *Alm.* iv 6: Toomer, 198

20 Hadrian: 19/20 Pharmuthi

We computed that mid-eclipse occurred 4 equinoctial hours after midnight. [The Moon] was eclipsed half its diameter from the north.

Lunar Eclipse-Data	Computed	Ptolemy	Δ
Beginning (Alexandria)	$2;\ 8^h$		
Midpoint (Alexandria)	$3;29$	$4;\ 0^h$	$-0;31^h$
Magnitude	5.5^d	6.0^d	-0.5^d

Eclipse No. 19: +136 Mar 6

The error of half an hour in Ptolemy's time is the largest found among the Alexandrian eclipses.

Errors in the eclipse-observations and data

Errors in observed eclipse-times and phases. The ambiguity of some of the times reported for the earlier eclipses, the probability that some of the times and phases are mis-stated, and the opportunities for misinterpreting some of Ptolemy's reports of these eclipses make any estimate of the general accuracy of these observations somewhat uncertain. Nevertheless, since the times of fourteen of the twenty-one recorded phases are reasonably secure, an estimate of their general accuracy is possible.

In comparing the errors of different groups of observations, I have assumed that times reported in seasonal hours were appropriately reduced from the original data of the observations, and thus that these were not merely mistaken for equinoctial hours. This is clearly the case for the early Alexandrian observations, for otherwise the errors in eclipses nos. 12, 13, and 14 are significantly increased. For the Babylonian observations the evidence is less conclusive. The errors of eclipses nos. 1, 3, and 9 are not significantly different under either assumption, while the errors of nos. 4, 7, and 10 are increased by assuming that equinoctial hours were meant. Thus, it seems best to assume that the observed data were converted to seasonal hours at some point in their transmission to Ptolemy.

In the few cases (eclipses nos. 4, 14, and 15) where a phase is said to have been observed at the beginning or end of an hour, I have assumed that the exact beginning or end of the hour was meant. Fotheringham's assumption that such times refer to the middle of the first or last third of the hour results in generally larger errors, and unnecessarily add another uncertainty.

To estimate the general accuracy of the observations it is convenient to divide them into two groups: (A) those for which the reported times are unambiguously stated and where an error in the phase or in the reported hour seems precluded, and (B) the remaining observations. I have included eclipse no. 5 in the first group on the evidence of the Babylonian report.

Table 3.3 shows the errors of both groups. Column 1 contains the errors of the unambiguous observations in group A. For the uncertain eclipse-observations in group B, column 2 gives the errors from what seems the most plausible interpretation of the report, and column 3 records the errors derived from Ptolemy's interpretations of the reported times. Finally, column 4 shows the errors which arise from less likely but still possible interpretations of the reports.

Using the average of the errors of the two phases reported for eclipses nos. 11 and 14, the times reported for the more certain observations (A) show a mean error of $-0;3.4^{h} \pm 0;3.5^{h}$ (epoch $= -285$), which may be considered negligible. This error could be eliminated by a further small reduction in the secular acceleration of the Moon's mean elongation,[18] but it also virtually disappears ($-0;0.9^{h}$) if the data from column 2 are included in the average. Furthermore, the positive and negative errors in the twelve relatively secure times (A) are evenly distributed (6–6), and the apparent systematic error arises almost entirely from eclipses nos. 7, 14, and 19. On the average, therefore, the secure Ptolemaic eclipse-times agree well with the accelerations which I have adopted; and only eclipse no. 8 is clearly incompatible with these accelerations.

Excluding no. 8, the errors in the remaining uncertain observations nos. 1, 3, 6, 19, 10, and 15 can all be brought to within $\pm 0;20^{h}$ by different assumptions of varying plausibility. In nos. 1 and 3, the time is specified only by noting that the eclipse began after some event (the passage of an

[18] The correction would be $-0.23''T^2$. Cf. Newcomb 1912, 205–206.

No.	Date	Place	A	B		
			1	2	3	4
1	−720 Mar 19	Babylon			$+0;45^{h}$ [a]	$+0;15^{h}$ [b]
2	−719 Mar 8	Babylon	$-0;\ 4^{h}$			
3	−719 Sep 1	Babylon		$+0;\ 9^{h}$	−0; 9	
4	−620 Apr 22	Babylon	+0; 5			
5	−522 Jul 16	Babylon	+0;11[c]		+0;45	
6	−501 Nov 19/20	Babylon		+0; 4[d]	+0;52	
7	−490 Apr 25	Babylon	−0;33			+0; 8[d]
8	−382 Dec 23	Babylon			+0;50	
9	−381 Jun 18	Babylon		+0;17	+0;34	
10	−381 Dec 12	Babylon		−0; 9	+0;26	
11a	−200 Sep 22	Alexandria	+0;10			
11b	−200 Sep 22	Alexandria	+0; 6			
12	−199 Mar 20	Alexandria	−0; 6			
13	−199 Sep 12	Alexandria	+0; 3			
14a	−173 May 1	Alexandria	−0;15			
14b	−173 May 1	Alexandria	−0;21			
15	−140 Jan 27	Rhodes			−0;48	{ +0; 1[d] +0;20[e]
16	+125 Apr 5	Alexandria	+0;29			
17	+133 May 6	Alexandria	−0;10			
18	+134 Oct 20	Alexandria	−0; 5			
19	+136 Mar 6	Alexandria	−0;31			

A. Observations Secure in Reported Time and Phase
  1 Errors in Times

B. Uncertain Observations
  2 Errors in Most Plausible Times
  3 Errors in Ptolemy's Interpretation of Times
  4 Errors in Possible Alternative Interpretations of Report

[a] $0;32^{h}$ from Sunset: $0;45^{h}$ from Moonrise, assuming $1\,^1\!/_2{}^{h}$.

[b] Assuming report meant 1 double hour.

[c] From Babylonian report. [d] Assuming error in phase.

[e] Assuming error in stated hour.

Table 3.3. Errors in Observed Eclipse-Times

hour or Moonrise). Ptolemy and most modern investigators have assumed half an hour to be the upper limit to the time which could have passed without being specified. In eclipse no. 3, the estimate of half an hour after Sunset agrees very well with the computed time of the phase. In eclipse no. 1, however, more than an hour appears to have elapsed after the passing of the first hour, so that here we must either loosen this assumption or postulate a significant error in the reported time. As noted, this error could have arisen from a misinterpretation of the Babylonian unit corresponding to two equinoctial hours. The time of eclipse no. 6 seems almost certainly to be referred to the wrong phase, an assumption strengthened by the evident error in the reported phase of no. 5. Eclipses nos. 8–10 have been discussed at length. Within the limit of the uncertain designation of the times, the most probable errors for nos. 9 and 10 seem to be $+0;17^h$ and $-0;9^h$ respectively. Finally, an error in either the phase or the time of no. 15 would bring the reported time into reasonably good agreement with the computed time; since both are explicitly stated, however, we must consider this eclipse an anomaly along with no. 8.

Omitting nos. 8 and 15, and taking the most probable interpretations of the remaining uncertain eclipses, we find the following probable errors (i.e., average deviations) for individual observations:

	Number of Observations	Probable Error
Babylonian Eclipses	9[a]	$\pm 0;12^h$
Early Alexandrian Eclipses	6	$\pm 0;\ 8$
Late Alexandrian Eclipses	4	$\pm 0;17$
Total	19	$\pm 0;10.6$

[a] The average deviation of the four secure Babylonian observations is $\pm 0;13^h$.

The late Alexandrian observations, at least three of which Ptolemy made himself, exhibit a slightly larger average error than the Babylonian observations and nearly twice the error of the early Alexandrian observations. Taking into account the small number of these observations, the even distribution of the signs of their errors, and the fact that two of Ptolemy's observations agree very closely with the computed times, this difference does not seem significant. In general, we may assume that the probable error of an observed time of an eclipse-phase was on the order of $\pm 0;11^h$.

Errors in Ptolemy's data. Numerous errors occur in Ptolemy's reductions of these eclipse-times and also in his use of the observations. These include apparent errors in Ptolemy's interpretation of some eclipse-reports, errors in converting from seasonal to equinoctial hours, and errors in his reductions to the meridian of Alexandria.

In Table 3.4, column 1 shows the errors of Ptolemy's interpretations of the observed times of these eclipses, and column 2 gives the errors in the times which he finally adopts for eclipse-midpoint on the meridian of Alexandria. For comparison, column 3 gives my estimates of the errors in the observations from Ptolemy's reports.

No.	Year	Correction to Ptolemy's Value for		
		Observed Time 1	Time of Eclipse-Middle[a] 2	Estimated Error 3
1	−720	$+0;25^h$	$+0;11^h$	$+0;32^h$
2	−719	−0; 4	−0;12	−0; 4
3	−719	+0; 3	−0;25	+0; 9
4	−620	−0; 9	−0;24	+0; 5
5	−522	+0;56	+0;48	+0;11
6	−501	+0;48	+0;40	+0; 4
7	−490	−0;31	−0;39	−0;33
8	−382	+0;42	+0;44	+0;50
9	−381	+0;27	+0; 9	+0;17
10	−381	+0;30	+0; 7	−0; 9
11a	−200	+0;12	+0;12	+0;10
11b	−200	+0;12		+0; 6
12	−199	−0; 9	−0;21	−0; 6
13	−199	+0; 1	+0;17	+0; 3
14a	−173	−0;17	−0;23	−0;15
14b	−173	−0;29		−0;21
15	−140	−0;43	−0;22	−0;48
16	+125	+0;29	+0;29	+0;29
17	+133	−0;10	−0;10	−0;10
18	+134	+0; 5	+0; 5	+0; 5
19	+136	−0;31	−0;31	−0;31

[a] At Alexandria.

Table 3.4. Errors in Ptolemy's Interpretations of Eclipse-Observations and Final Data

The mean systematic errors from columns 1 and 2 are:

	Number of Observations	Observed Times (Ptolemy)	Concluded Data
Babylonian Eclipses	10	$+0;18.7^{h}$	$+0;\ 5.9^{h}$
Early Alexandrian Eclipses	7	$-0;10.4$	$-0;\ 7.4$
Late Alexandrian Eclipses	4	$-0;\ 1.8$	$-0;\ 1.8$
Total	21	$+0;\ 5.1$	$+0;\ 0.3$

Interestingly, Ptolemy's error in the difference in longitude between Alexandria and Babylon[19] served to reduce the systematic error in his Babylonian data, and this error was further reduced by errors in his reductions of these observations.

The probable non-systematic errors in a single datum in each group are:

	Number of Observations	Observed Times (Ptolemy)	Concluded Data
Babylonian Eclipses	10	$\pm0;19^{h}$	$\pm0;21^{h}$
Early Alexandrian Eclipses	7	$\pm0;11$	$\pm0;14$
Late Alexandrian Eclipses	4	$\pm0;17$	$\pm0;17$
All Observations	21	$\pm0;18.6$	$\pm0;17.9$

As might be expected from the uncertainties and inconsistencies of the reports, the Babylonian observations have the largest errors by Ptolemy's interpretation. The errors of the early Alexandrian eclipses are again smallest, although greater than those found for the observations themselves. In general, we may conclude that the probable non-systematic error of an eclipse-time used by Ptolemy is roughly $+0;18^{h}$.

Comparison of eclipse-magnitudes. Table 3.5 shows the computed magnitudes of the partial eclipses in column 1, Ptolemy's reported magnitudes in column 2, and the error in Ptolemy's magnitudes in column 3. In general, the Babylonian reports overestimate the magnitudes. In these observations the mean systematic error is nearly $^{3}/_{4}$ digit, and the average deviation roughly $^{1}/_{2}$ digit. In contrast, the later observations show a negligible systematic error ($\approx -0.1^{d}$), and an average deviation of $^{1}/_{4}$ digit. The latter agrees well with what we would expect from accurate estimates of eclipse-magnitudes to the nearest digit.

[19] Ptolemy assumes a longitude difference between Alexandria and Babylon of $0;50.^{h}$ According to P. V. Neugebauer [1929, ii 133] the difference is $0;58.4^{h}$.

No.	Computed	Observed	Δ	Argument of Latitude[a]
2	1.5^d	3.0^d	-1.5^d	$9°$
3	6.1	> 6.0	NM	187
4	2.1	3.0	-0.9	170
5	6.1	6.0	0.1	352
6	2.1	3.0	-0.9	170
7	1.7	2.0	-0.3	168
8	3.0	SMALL		358
Average: $-0.7^d \pm 0.4^d$ (7 obs.)				
14	7.4	7.0	$+0.4$	187
15	2.8	3.0	-0.2	9
16	1.8	2.0	-0.2	169
18	10.1	10.0	$+0.1$	185
19	5.5	6.0	-0.5	350
Average: $-0.1^d \pm 0.25^d$ (5 obs.)				

[a] Approximate value: cf. Newcomb 1878, 41.

Table 3.5. Comparison of Eclipse-Magnitudes

OBSERVATIONS OF OCCULTATIONS

In *Alm.* vii 3, Ptolemy reports lunar occultations of the Pleiades, Spica, and β Scorpionis, which were observed by Timocharis in Alexandria, Agrippa in Bithynia, and Menelaus in Rome. For the observed times of the occultations Ptolemy computes the apparent positions of the Moon and, hence, the positions of the occulted stars. From these positions he shows that the latitude of each star remained constant, while the longitude increased at a rate of $1°$ *per* century or $36''$ *per* year.

Although Ptolemy does not use these observations to establish his lunar model,[20] I have included a discussion of them for several reasons. One is that they illustrate the quality of some of the older, non-Babylonian,

[20] In *Alm.* iv 1 [Toomer, 173] Ptolemy remarks that such observations should not be used to establish a lunar model, since they require a prior knowledge of the Moon's parallax. In general, this can only be determined when the variation of the Moon's distance from the Earth and, hence, the lunar model, is known.

observational material at Ptolemy's disposal,[21] thus providing another indication of the accuracy of time-measurements in antiquity. Furthermore, the errors which they exhibit and the ambiguity of some of the reports exemplify some of the problems which Ptolemy must have encountered in attempting to use such material.

Secondly, these observations illustrate a problem which also arises in connection with Ptolemy's lunar model—namely, that the values of precession which Ptolemy finds from these observations are both better and more consistent than we should expect from random observations of the same general accuracy. From different pairs of occultations, Ptolemy finds:

From Occultations of	Total Precession	Interval	Annual Precession
The Pleiades	3;45°	375^y	36.0″
Spica	3;55	391	36.05
Spica	3;45	379	35.6
β Sco	3;55	391	36.05

Not only do these agree among themselves, but the value of precession, 36″ *per* year, agrees almost exactly with the value Ptolemy should have found given the error in his mean motion of the Sun and, hence, of the Moon.[22] This value is 34.8″ *per* year or 0;58.0° *per* century, so that the systematic error in Ptolemy's determinations of each of the longitude-intervals shown above is only 0;8°. Furthermore, these intervals are mutually consistent to within ±0;2.5°, or to within the nearest ±5 minutes of time. Since the errors of both the observations and Ptolemy's lunar model are much larger than this, it is evident that these are not random observations. Thus, we may also ask how Ptolemy could have achieved such good agreement and whether he must not have had a considerably larger number of observations to choose from.

21 Except for the solstice observed by 'the school of Meton and Euctemon' in −431 [*Alm.* iii 1: Toomer, 143], for which no details of the observation are given, the occultations observed by Timocharis (−294 to −282) are the earliest Greek observations recorded in the *Almagest*.

22 Despite the fact that Kepler [1627, 120], Lalande [1766, 467], Laplace [1756, 421], Ideler [1806, 107], Dreyer [1918, 346], and Fotheringham [1915a, 378; 1918, 421] have observed that the error in Ptolemy's star positions and, hence, the error in his value of precession, is due to the error in his mean motion of the Sun, it is still common to find references to Ptolemy's 'erroneous value of precession' which imply that this arose from an independent error in his determinations of the positions of the stars. See, e.g., Newcomb 1878, 279 and Manitius 1912, ii 399n3.

Finally, these observations have significantly influenced modern determinations of the Moon's secular acceleration. All of the occultations were first discussed by Schjellerup [1881], who showed that several of the times and phases Ptolemy describes disagree significantly with those computed from Hansen's tables. Later, Fotheringham [1915a, 1923] reinvestigated the circumstances of each occultation to determine the Moon's sidereal acceleration. After weighting the observations according to his estimates of the likely sources of error in each, he concluded that the occultations were best represented by a lunar acceleration of $(10.3'' \pm 0.74'')T^2$.[23] Finally, Schoch [1926] derived his value for the Moon's acceleration $(11.09''T^2)$ from one of these observations, −282 Nov 8, and from this recomputed the errors in the times which Ptolemy reports.

Each of these previous studies points out large discrepancies between the observed and computed circumstances of several of the occultations, regardless of the value assumed for the Moon's acceleration. The discussion which follows, therefore, reiterates some of the findings of these previous investigations. It seemed desirable, however, to reduce the results of Fotheringham and Schoch to a consistent basis and to show that the elements derived in appendix 1 produce a satisfactory distribution of errors.

In the following discussion I draw on Fotheringham's investigation [1915a, 1923] for the positions of the stars at the times of the observations and for provisional positions of the Moon [see Fotheringham 1915a, 384–385; 1923, 370–371]. I then corrrect Fotheringham's computed apparent longitudes of the Moon to bring these into agreement with my elements,[24] and also to

[23] Fotheringham's initial determination [1915a, 395] was $10.8''T^2$. Subsequently [1923, 370], he discovered an error in his comparison of occultation no. 7, which yielded a revised acceleration of $10.3''T^2$.

[24] To reduce Fotheringham's computed longitudes to the adopted elements, I have applied the correction, $\Delta L = 5.8'' + 11.9''T + 1.6''T^2$ (epoch 1900). For each observation, the correction is:

Occultation Number	Correction to Mean Lunar Longitude
1	+0; 6.6°
2	+0; 6.6
3	+0; 6.5
4	+0; 6.5
5	+0; 5.3
6	+0; 5.3
7	+0; 5.3

compensate for Fotheringham's assumption that ὥρας ἀρχομένης and ὥρας ληγούσης refer to the middle of the first and last thirds of the designated hour.[25] The results shown, therefore, are the apparent positions of the Moon at the exact beginning or end of the hours reported by Ptolemy (except in the case of −282 Nov 8 where the time is reported more precisely). Since in computing the positions of the Moon Fotheringham includes only terms with coefficients greater than 0;3°, the resulting longitudes are uncertain by roughly this amount. Finally, for each observation I include a diagram showing the computed apparent position of the Moon and the direction of its motion at the indicated time and also the position of the Moon relative to the star(s) which Ptolemy assumes in reducing the observation.

Occultation 1. −294 Dec 21 Alexandria *Alm.* vii 3; Toomer, 337

36 Callipic Period I: 25 Poseidon

> Timocharis, who observed at Alexandria, says that ... at the [very] beginning of the tenth hour, the Moon appeared to occult [reach] the northernmost (β) of the stars in the forehead of Scorpius very precisely with its northerm rim.

[25] The corrections to Fotheringham's interpretations of the observed times and the corresponding corrections to his computed longitudes are:

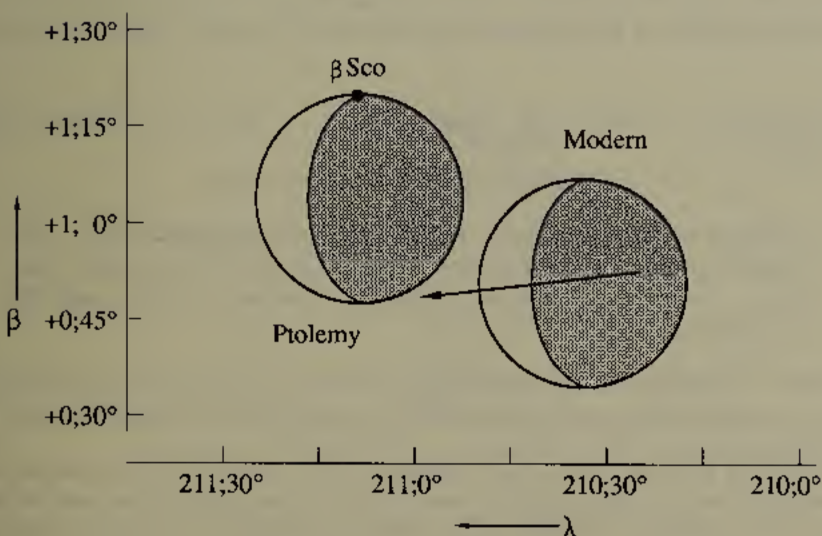
Occultation Number	Correction to Observed Times	Correction to Computed Longitude
1	0′	0; 0°
2	−11	−0; 4.7
3	+11	+0; 4.5
4	0	0; 0
5	−15	−0; 6.7
6a	0	0; 0
6b	+13	+0; 5.4
7	+13	0; 6.2

Except for no. 5, where a small additional correction is made for the longitude of Bithynia, these are corrections to the beginning or end of a seasonal hour, where Fotheringham has used the first or last third of the hour. The differences between the times given by Schjellerup [1881] and Fotheringham [1915a] are due mainly to this assumption by Fotheringham, and to the fact that Schjellerup's times are computed from true rather than apparent sunset.

Occultation-Data	Computed	Ptolemy
Apparent Time (Alexandria)	$3;30^h$	$3;24^h$
Longitude of β Sco	211; 9.2°	212; 0°
Apparent Lunar Longitude	210;32.6	212; 0
Difference in Longitude	0;46.6	0; 0
Latitude of β Sco	+1;17.9	+1;20
Apparent Lunar Latitude	+0;53.8	+1; 5
Moon's Semi-Diameter[a]	0;14.9	
Apparent Lunar Velocity in Longitude[b]	0.425[c]	
Correction to Observed Time	$+1;50^h$	

[a] Fotheringham 1915a, 383.

[b] Schjellerup 1881, 225. [c] 0;1° *per* min.



Occultation No. 1: −294 Dec 21

The report does not state that β Scorpionis was actually occulted, and the computation indicates that the upper rim of the Moon passed β Sco 0;10° to the south. The longitude of the Moon's center (or cusp) was $^3/_4$° less than that of the star at the stated time. An error in the reported hour seems likely.[26]

Occultation 2. −293 Mar 9 Alexandria *Alm.* vii 3: Toomer, 335

36 Callipic Period I: 15 Elaphebolion

> Timocharis, who observed at Alexandria, records that … at the beginning of the third hour, the Moon covered Spica with the middle of the [eastern] edge of its disk, … and that Spica in passing through, cut off exactly the northern third of its diameter.

Fotheringham allows 0;3° for the distance from the Moon's illuminated disk at which a star of the first magnitude would still be visible. I accept this value.

The occultation occurred just at the middle of the Moon's eastern rim, as reported, but Spica passed almost through the center of the Moon, rather than two digits to the north. As in occultation no. 1, the Moon had not yet reached the star at the reported time, the error being very nearly one hour.

Occultation 3. −282 Jan 29 Alexandria *Alm.* vii 3: Toomer, 334

47 Callipic Period I: 8 Anthesterion

> Timocharis, who observed at Alexandria, records the following. … towards the end of the third hour, the southern half of the Moon was seen to cover exactly either the rearmost [eastern] third or half of the Pleiades.

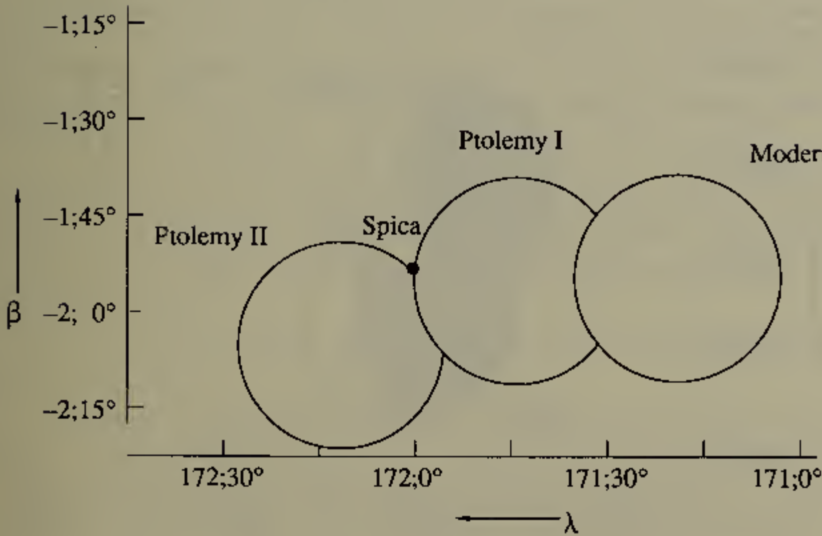
There is considerable uncertainty about Ptolemy's account of the Pleiades and complete disagreement between the identifications by Manitius [1912, i

[26] Ptolemy gives the time as ὥρας ι′ ἀρχούσης ἀκριβῶς and computes the position of the Moon for this time. Thus, any error in the reported time of the observation must have antedated Ptolemy. The simplest explanation is that the original report gave ὥρας ια′ or ὥρας ιβ′ indicating the beginning of the 11th or 12th seasonal hour, and that the α or β was lost through a scribal error during the four centuries between Timocharis and Ptolemy. At the beginning of the 12th seasonal hour, the apparent center of the Moon was 0;11° beyond β Sco, equivalent to an error of ±26 minutes.

Occultation-Data	Computed	Ptolemy
Apparent Time (Alexandria)	19;52^{h}	20; 0^{h}
Latitude of Spica	−1;54.2°	−2; 0°
Apparent Lunar Latitude	−1;54.8	−2; 0
Longitude of Spica	172; 0.5	172;20
Apparent Lunar Longitude	171;19.1	172; 5
Moon's Semi-Diameter[a]	0;15.0	
Arcus visionis	0; 3	
Difference in Longitude from Spica to Moon's Rim	0;23.4	
Apparent Lunar Velocity in Longitude[b]	0.408[c]	
Correction to Observed Time	+0;57^{h}	

[a] Fotheringham 1915a, 384.

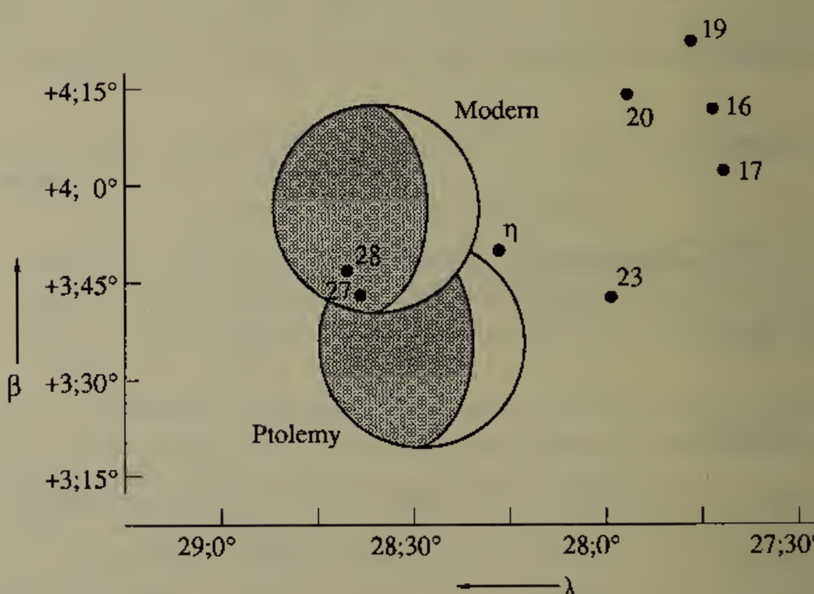
[b] Schjellerup 1881, 227. [c] 0;1° *per* min.



Occultation No. 2: −293 Mar 9

Occultation-Data	Computed	Ptolemy
Apparent Time (Alexandria)	20;38^h	20;40^h
Apparent Lunar Longitude	28;37.4°	29;20°
Latitude	+31;54.5	+3;35
Moon's Semi-Diameter[a]	0;15.8	
Apparent Lunar Velocity in Longitude[a]	0.410[b]	
23 Tauri Longitude	27;59.6°	
Latitude	+3;43.9	
η Tauri Longitude	28;17.1	
Latitude	+3;49.3	
27 Tauri Longitude	28;38.9	29;30
Latitude	+3;41.3	3;40
28 Tauri Longitude	28;40.3	
Latitude	+3;46.1	

[a] Fotheringham 1915a, 384. [b] 0;1° *per* min.



Occultation No. 3: −282 Jan 29

45] and by Peters and Knobel [1915, 36] of the three stars in the Pleiades contained in Ptolemy's star catalogue.[27] Manitius identifies 'the closest following end of the Pleiades' (the 32nd star in Taurus according to Ptolemy) with η Tauri, as does Schjellerup [1881, 229]. Peters and Knobel identify this star with 27 Tauri, and Toomer follows Peters and Knobel in all three identifications. To complicate matters further, in discussing this occultation and no. 5 [*Alm.* vii 3: Toomer, 334–335], Ptolemy assigns a latitude of $+3;40°$ to 'the rearmost end of the Pleiades' but gives $+3;20°$ as the latitude in his star-catalogue [*Alm.* vii 5: Toomer, 45]. Although preserved in all mss. [cf. Peters and Knobel 1915, 190], the catalogue's value is probably an error.

In reducing this occultation, Ptolemy assumes the rearmost end of the Pleiades to be 0;10° east and 0;5° north of the center of the Moon, despite Timocharis' statement that the occulted stars were covered by the southern half of the Moon. Fotheringham [1915a, 388] identifies 'the following third or half part' with 28, 27, and η Tauri, and assumes that these three stars were covered at the time of the observation. This interpretation is consistent with Ptolemy's, if 'the rearmost end of the Pleiades' is identified with 27 Tauri.

At the stated time, 28 and 27 Tauri were covered by the Moon, while η Tauri was just 0;5° west of the Moon's illuminated rim. Since a star of magnitude 3 would barely be visible at this distance [cf. Schoch 1926, 2] the reported time very nearly coincides with the apparent emersion of η Tauri. Since the immersion of 27 Tauri occurred nearly 17 minutes earlier, the limits of the correction to the observed time are $0;0^h$ to $-0;17^h$.

A possible but less likely interpretation of the report, which Schjellerup assumes [1881, 229], is that 'the following third or half part of the Pleiades' refers to η and 23 Tauri, rather than 27, 28, and η Tauri. For 23 Tauri to have been covered, a correction to the observed time of at least -55 minutes is required.

On either assumption the stars are covered by the southern half of the Moon as reported.

[27] In addition to identifying Ptolemy's star 32 Tauri with the modern star η Tauri, Manitius identifies Ptolemy's stars 30 and 31 Tauri with modern 16 and 17 Tauri, respectively. Peters and Knobel (followed by Toomer) identify the last two stars with modern 19 and 23 Tauri. In all cases, the identifications proposed by Peters and Knobel seem more plausible than those proposed by Manitius.

Occultation 4. −282 Nov 8 Alexandria *Alm.* vii 3: Toomer, 336

48 Callipic Period I: 25 Pyanepsion

> [Timocharis] says that ... when as much as half an hour of the tenth hour had gone by, and the Moon had risen above the horizon, Spica appeared exactly touching the northern point on [the Moon].

The computation shows that the Moon's cusp passed 0;3° to the south of Spica, which agrees very closely with Timocharis' description. The time of the observation has been debated because of the difference between the time of Moonrise and the time reported for conjunction. In reiterating Timocharis' description, Ptolemy says only that the Moon had risen (ἀνατεταλκυίας) above the horizon. In reducing the observation, however, Ptolemy notes that the stated time must be corrected, since the Moon 'was rising' (ἀνέτελλε). He then assumes that conjunction occurred at Moonrise, or at $2;30^{h}$ by his computation. The discrepancy between the two times was noted by Schoch, who assumes that conjunction occurred half an hour after Moonrise. Schoch's value for the secular acceleration of the Moon rests entirely on this assumption.

My computation places conjunction at $3;35^{h}$, local apparent time (Alexandria), or 55 minutes after Moonrise. At this time the Moon's apparent altitude was 11;50°; whereas half an hour after Moonrise, it was 6;33°. Either altitude seems sufficiently small to satisfy the description that the Moon 'was rising', while neither adequately satisfies Ptolemy's assumption (and Manitius' interpretation) that the Moon was 'just rising'.[28] The reference to Moonrise, therefore, seems to have been less precise than the observed

[28] Manitius' translations [1912, vii 3, 25] of ἀνατεταλκυίας (having risen) and ἀνέτελλε (was rising) by '*eben aufgegagen*' (having just risen) and '*eben aufging*' (was just rising) seem to be derived from Ptolemy's use of exact Moonrise in his computation, for there is no textual basis for the qualifying adverb. Furthermore, even if we assume an ambiguity in the text, the alternatives are either to assume with Ptolemy that conjunction occurred within a few minutes of Moonrise, or to take the phrase as merely indicating that conjunction occurred sometime after Moonrise. The first possibility is excluded, because it would require an implausibly high value for the Moon's sidereal acceleration ($\approx 13.0''T^2$). Thus, we must conclude that the phrase meant only that the Moon had risen and was not yet high in the sky. This qualification places only broad limits on the possible time of the event, since even at the reported time the Moon's altitude was only 15;20°. Thus, Schoch's assumptions that conjunction must have occurred exactly half an hour after Moonrise, and that this datum is more certain than those from any other ancient lunar observations, are wholly gratuitous.

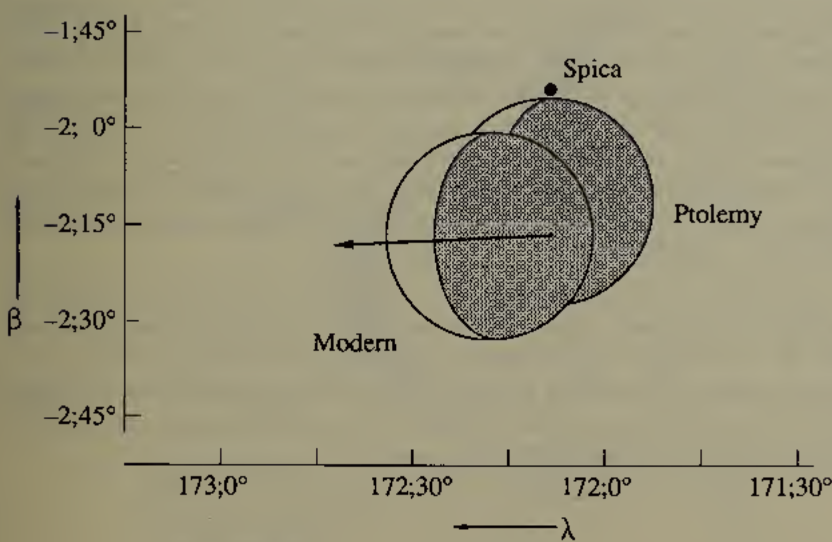
Occultation-Data	Computed	Ptolemy
Apparent Time (Alexandria) (1)	3;52^h	3;7.30^h [a]
Approximate Time of Moonrise	2;40[b]	2;30
Time of Spica's Rising[c]	2;42	
Spica's Longitude	172; 9.6°	172;30°
Latitude	−1;54.2	−2; 0
Apparent Lunar Longitude at (1)	172;18.4	172;30
Latitude	−2;13.8	−2;15
Apparent Lunar Velocity in Longitude[d]	0.530[e]	
Correction to Observed Time	−0;17^h	

[a] The text gives $3\,{}^1\!/_8$ hours, which is probably erroneous, since at this time $3\,{}^1\!/_2{}^{\text{s.h.}} = 3;52^h = 3\,{}^7\!/_8{}^h$. As Toomer [337n75] notes, this could have resulted from calculating the length of daytime instead of the nighttime seasonal hours.

[b] From Schoch 1926, 2, corrected to apparent time.

[c] Schjellerup 1881, 230.

[d] Fotheringham 1915a, 384. [e] 0;1° *per* min.



Occultation No. 4: −282 Nov 8

time, which agrees reasonably well with the computed time of conjunction. The time which Ptolemy assumes for conjunction is badly in error.

Occultation 5. +92 Nov 29 Bithynia *Alm.* vii 3: Toomer, 334

12 Domitian: 7 Metroos

> Agrippa, who observed in Bithynia, records that... at the beginning of the third hour of the night, the Moon occulted the rearmost [eastern], southern part of the Pleiades' with its southern horn.

None of the southeastern stars in the Pleiades (27, 28, η) were occulted and the Moon's rim passed more than $0;20°$ north of η Tauri, the closest of these stars. Even Ptolemy finds that no occultation occurred, and he places the 'rearmost end of the Pleiades' $0;5°$ south of the Moon's southern rim. Since the Moon did occult 19 and 20 Tauri, Fotheringham [915a, 388] assumes that Agrippa meant the northwest instead of the southeast part of the Pleiades. Although this is the simplest explanation, Ptolemy's explicit statement to the contrary should disqualify the observation from being considered in determining the Moon's acceleration.

The computed place of the Moon is uncertain by $\approx \pm 0;2°$ due to the uncertain location of Bithynia. Ptolemy assumes that Bithynia is 20 minutes east of Alexandria, but this is impossible, since the entire province of Bithynia does not extend this far east [cf. Shepherd 1921, 20, 43]. Fotheringham [1915a, 381] seems to identify Bithynia with Nicea ($1;59^h$ E; $40;30°$ N), while Schjellerup [1881, 231] assumes that the observation was made at Nicomedia ($2;0^h$ E; $40;48°$ N). Another possible location is the city of Bithynium, later called Claudiopolis [Shepherd 1921, 20], whose longitude is $2;7^h$ east of Greenwich and whose latitude is $40;42°$ N [P. V. Neugebauer 1929, ii 133]. I have assumed a longitude half way between Nicea and Bithynium, with a probable error of ± 4 minutes.

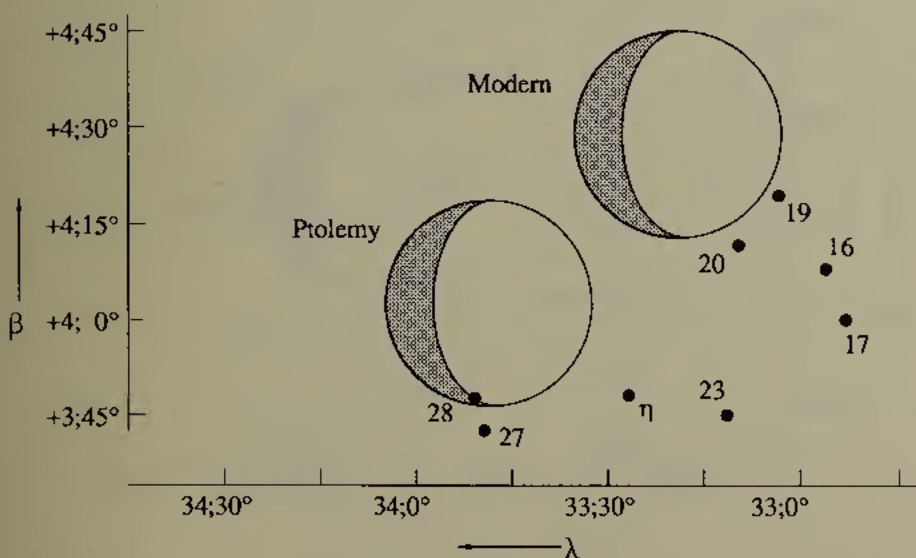
At the time shown 19 and 20 Tauri were respectively $0;5°$ and $0;6°$ from the Moon's illuminated rim, and thus were just becoming visible [cf. Schoch 1926, 2]. In contrast, the immersion of 20 Tauri occurred at $\approx 18;14^h$, or 51 minutes earlier. Thus, if we assume that the report should have indicated that the northwest part of the Pleiades (19 and 20 Tau) was covered, the limits of the error in the stated time are:

	Correction to Observed Time
Apparent emersion (19, 20 Tau)	$0;\ 0^h \pm 0;\ 4^h$
Immersion (20 Tau)	$-0;51^h \pm 0;\ 4^h$

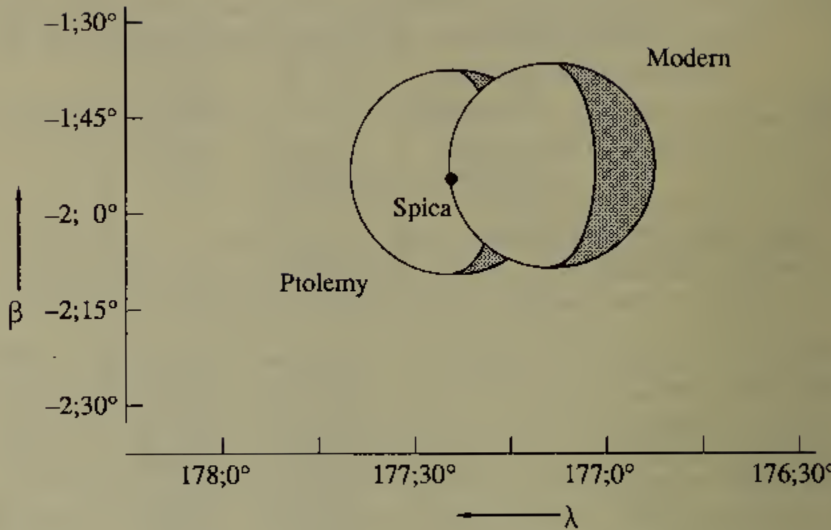
Occultation-Data	Computed	Ptolemy
Apparent Time (Bithynia)	19; 7^{h}	19; 0^{h}
(Alexandria)	19; 3[b]	18;40
Apparent Lunar Longitude	33;19.9°	33;15°
Latitude	+4;29.6	+4; 0
Moon's Semi-Diameter[a]	0;14.95	
Apparent Lunar Velocity in Longitude[a]	0.375[c]	
Star Positions[a]	**Longitude**	**Latitude**
17 Tauri	32;53.8°	+4; 0.0°
19 Tauri	33; 3.2	+4;19.5
20 Tauri	33; 9.6	+4;11.9
23 Tauri	33;10.9	+3;46.0
η Tauri	33;28.6	+3;51.5
27 Tauri	33;50.4	+3;43.5
27 Tauri (Ptolemy)	33;15	+3;40

[a] Fotheringham 1915a, 384–385. [b] ±0;4^{h}.

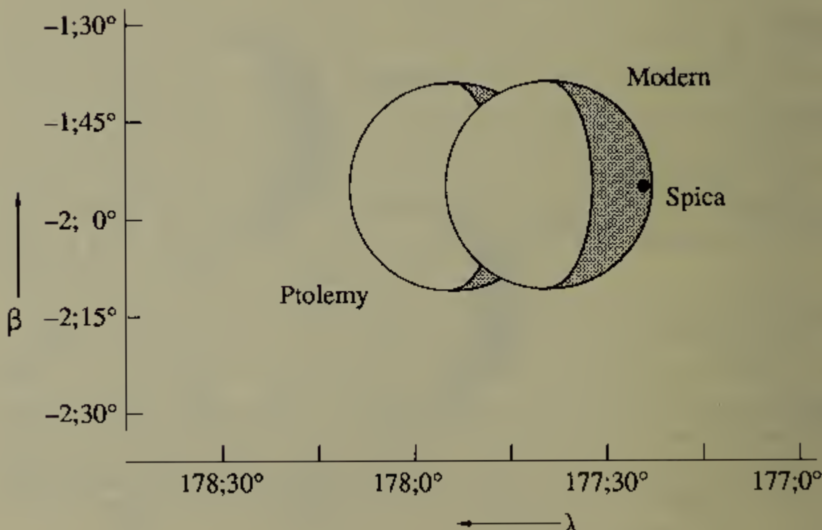
[c] 0;1° *per* min.



Occultation No. 5: +92 Nov 29



Occultation No. 6a: +98 Jan 11



Occultation No. 6b: +98 Jan 11

Occultation 6. +98 Jan 11 Rome *Alm.* vii 3: Toomer, 336–337

1 Trajan: 15/16 Mechir

> the geometer, Menelaus, says that the following observation was made [by him] at Rome.... when the tenth hour was completed, Spica had been occulted by the Moon (for it could not be seen), but towards the end of the eleventh hour it was seen in advance of the Moon's center, equidistant from the [two] horns by an amount less than the Moon's diameter.

At the earlier of the two reported times Spica was just covered by the Moon, so that the computed circumstances agree with those reported. Spica emerged from behind the Moon at $6;13^h$, or 4 minutes after the end of the 11th seasonal hour, and was, as the report says, just equidistant from the two cusps. Menelaus says only that Spica was visible and less than a lunar diameter from the Moon's center at this time. If we assume $0;24^\circ \pm 0;4^\circ$, or three quarters of the Moon's diameter, as the probable distance from Spica to the Moon's center, the error in the second observed time is $+0;25^h \pm 0;10^h$. A similar error in the first reported time would leave Spica covered and very nearly in conjunction with the Moon's center, as Ptolemy assumes.

Phase 1 Data	Computed	Ptolemy
Apparent Time (Rome)	$4;55^h$	$5;\ 0^h$
(Alexandria)	6; 5	6;20
Spica's Longitude	$177;24.6^\circ$	$176;15^\circ$
Latitude	−1;55.5	−2; 0
Apparent Lunar Longitude	177; 9.0	176;15
Latitude	−1;53.4	−2; 0
Moon's Semi-Diameter[a]	0;15.9	
Phase 2 Data		
Apparent Time (Rome)	$6;\ 9^h$	
Apparent Lunar Longitude	$177;38.4^\circ$	
Latitude	−1;55.2	
Apparent Lunar Velocity in Longitude[a]	0.403[b]	

[a] Fotheringham 1915a, 384. [b] $0;1^\circ$ *per min.*

Occultation No. 6: +98 Jun 11

Occultation 7. +98 Jan 14 Rome *Alm.* vii 3: Toomer, 338

1 Trajan: 18/19 Mechir

> Similarly, Menelaus, who observed in Rome, says that ... towards the end of the eleventh hour, the southern horn of the Moon appeared on a straight line with the middle and southernmost of the stars in the forehead of Scorpius (π, δ), and its center was to the rear [east] of that straight line, and was the same distance from the middle star (π) as the middle star was from the southernmost, [and] it appeared to have occulted the northernmost of the stars in the forehead (β), since [it] was nowhere to be seen.

At the stated time the Moon's southern cusp was 0;16° east of the line between π and δ Scorpionis, while its eastern rim was 0;23° west of β Sco. Since Menelaus says only that he did not see Scorpio, while he describes the alignment with π and δ Sco in explicit detail, I have taken the latter as the basis for comparison. The error in the time is, therefore, $-0;45^{\mathrm{h}}$.

Ptolemy assumes that the Moon was in conjunction with Scorpio when observed, and thus tacitly ignores the alignment reported by Menelaus.[29] According to my computation, conjunction occurred roughly 2 hours ($1;51^{\mathrm{h}}$) after the time reported. Menelaus' estimate (measurement?) that the Moon and δ Sco were equidistant from π Sco was in error by $\approx$ 0;20°.

Errors in the occultation-observations and data

Errors in the observed times of lunar occultations. Table 3.6 shows the errors in the observed times as understood by (a) myself, (b) Fotheringham [1915a], and (c) Schoch [1926]. All of the errors have been reduced to the elements derived in appendix 1,[30] so that the differences between the errors

[29] According to Ptolemy's star catalogue, π and δ Scorpionis are on the same latitude-circle, while β Sco is 0;40° in longitude further west. Thus, he implicitly rejects the alignment reported by Menelaus.

[30] See 79n24 above, for corrections to Fotheringham's initial results. To correct the errors in the observations found by Schoch [1926, 2], I have computed the error in his mean longitude from

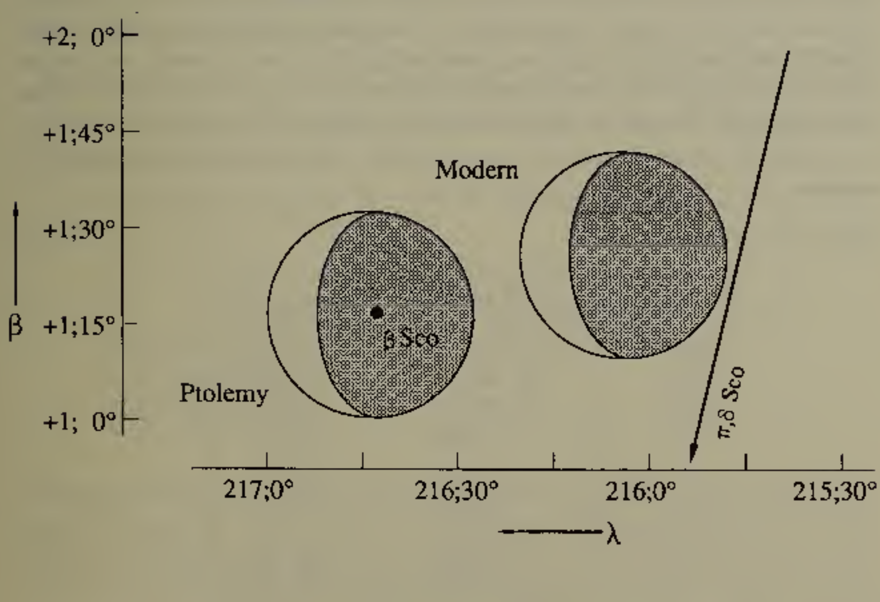
$$\Delta L = +0.13'' - 2.63''T - 1.46''T^2 \text{ (epoch, 1900).}$$

From this correction to his mean longitude of the Moon, I have obtained corrections to his errors in the observations, using the apparent lunar velocity at each occultation. The corrections to Schoch's tabular longitudes and times for

Occultation-Data	Computed	Ptolemy
Apparent Time (Rome)	6; 8^h	6;10^h
(Alexandria)	7;18	7;30
Apparent Lunar Longitude	216; 5.6°	215;55°
Latitude	+1;25.4	+1;20
Moon's Semi-Diameter[a]	0;15.2	
Apparent Lunar Velocity in Longitude[a]	0.346[b]	
Star Positions	**Longitude**	**Latitude**
β Sco	216;44.0°	+1;16.3°
β Sco (Ptolemy)	215;55	+1;20
π Sco	216; 7.2	−1;43.0
δ Sco	216;29.8	−5;12.9
Distance (App. Moon−π Sco)	3;10	
Distance (π Sco−δ Sco)	3;30	

[a] Fotheringham 1915a, 384–385; 1923, 370–371.

[b] 0;1° *per* min.



Occultation No. 7: +98 Jan 14

No.	Date	A Britton	B Fotheringham 1915a	C Schoch 1926
1	−294 Dec 21	$+1;50^h$	$+1;50^h$	$+1;50^h$
2	−293 Mar 9	+0;57	+0;46	+0;46
3	−282 Jan 29	−0; 8[a]	−0;19[a]	+0; 2
4	−282 Nov 8	−0;17	−0;17	−0;42
5	+92 Nov 29	−0;25[b]	−0;40[b]	...[c]
6	+98 Jan 11	+0;25[d]	+0;43[d]	+0;18
7	+98 Jan 14	−0;45	−0;32	−0; 4

[a] $\pm 0;\ 9^h$. [b] $\pm 0;29^h$.

[c] Schoch [1926, 2] does not give an error for occultation no. 5, stating that the longitude of Bithynia is uncertain by $0;8^h$.

[d] $\pm 0;10^h$.

Table 3.6. Errors in Observed Times of Occultations

found for the same observation represent either different interpretations of the times and corresponding phases of the occultations, or differences in the computed place of the Moon. Since Fotheringham's computations only include the lunar inequalities in longitude greater than $0;3^\circ$, while Schoch includes all inequalities greater than $4''$ of arc, discrepancies of up to $\pm 10''$ can arise from differences in the computed positions of the Moon. Unfortunately, Schoch publishes only the results of his analysis, so that it is impossible either to use his more accurate computations or to check his results.

each occultation are:

Occultation Number	Correction to Times	Correction to Longitude
1	$+0;24^h$	$-0;10.8^\circ$
2	+0;26	−0;10.8
3	+0;26	−0;10.7
4	+0;20	−0;10.7
5	+0;19	−0; 7.3
6	+0;18	−0; 7.2
7	+0;21	−0; 7.2

As may be seen by comparing columns (A) and (B), Fotheringham's assumption that ὥρας ἀρχομένης and ὥρας ληγούσης refer to the middle of the first and last thirds of the stated hour reduces the error in the times of occultations nos. 2 and 7 and increases the error in nos. 3, 5, and 6. Thus, as in the case of the eclipses, introducing this assumption makes no material difference.

Schoch does not say how he interpreted these times. In occultation no. 2 he appears to have followed Fotheringham, while the errors of occultations nos. 3 and 6 suggest that he computed for the exact hour. Perhaps he accepted the interpretation yielding the smallest error in each case. In no. 6, Schoch seems to have assumed a shorter distance from Spica to the Moon's center than I have. I cannot explain the discrepancy between Schoch's error for no. 7 and mine or Fotheringham's. As for the lunar eclipses, the signs of the errors are evenly distributed; this may be taken to indicate that the adopted elements are in reasonable accord with the observations.

The error of nearly two hours in the time of occultation no. 1 reported by Timocharis strongly suggests that the reported time is wrong. If not, the average clock-error for this observation amounts to 12 minutes *per* hour if measured from Sunset, and 35 minutes *per* hour if measured to Sunrise. Similarly, the clock-error in Timocharis' second observation is roughly 22 minutes *per* hour, measured from Sunset. In contrast, the clock-errors in his two later observations are less than 3 minutes *per* hour. If the reports of the earlier observations are not in error, then the intervening ten years must have greatly improved Timocharis' method of determining time at night. The clock-errors in Menelaus' observations nearly 400 years later are $\approx \pm 4$ minutes *per* hour by my computation, and are negligible by Schoch's computation.

If we exclude occultation no. 1 as an anomaly, the mean and probable errors in the reported times are:

Investigator	Mean Error	Probable Error
(a) Britton (6 obs.)	$-0;\ 2.2^{h}$	$\pm 0;25^{h}$
(b) Fotheringham (6 obs.)	$-0;\ 3.2$	$\pm 0;26$
(c) Schoch (5 obs.)	$+0;\ 4.0$	$\pm 0;22$

The probable errors include the uncertainty in the phase of the occultation described, and in nos. 1 and 2, the effects of the imprecision of Fotheringham's computations of the Moon's longitudes. Since the probable error from Schoch is heavily influenced by his anomalous error for no. 7, we may assume that the probable error in an observed time of a phase of an occultation was on the order of ±25 minutes. Thus, the times appear significantly less accurate than those reported for lunar eclipses.

Errors in Ptolemy's data for lunar occultations. Ptolemy's data contain three types of errors. First, there are the errors discussed above in the observed times of the indicated events. Second, there are errors in the reductions of the reported times to apparent Alexandrian time. Finally, there are what I shall call 'phase errors', which are apparent errors in Ptolemy's interpretation of the configuration of the Moon and reference body at the reported times. These are most conveniently understood as errors in (lunar) longitude, and are readily transformed into additional time-errors.

These errors are shown in Table 3.7. Column I gives the observational errors from column A in Table 3.6; column II shows the reduction errors; and columns III and IV give the phase-errors in longitude and time, respectively. Finally, column V gives the total error in Ptolemy's datum—i.e., the sum of columns I, II,and IV—expressed in time for each event, while column VI shows the corresponding errors in longitude. As before, these errors are to be understood as corrections to Ptolemy's data.

For all seven observations, the mean (systematic) and probable deviations from each source and collectively are:

Column in Table 3.7	Source of Error	Mean Error	Probable Deviation
I	Observation	$+0;14^h$	$\pm 0;66^h$
II	Reduction	$+0;\ 5$	$\pm 0;14$
IV	Phase	$+0;32$	$\pm 0;35$
V	Total Error in Datum	$+0;51$	$\pm 0;31$

Clearly, these are far greater than the errors which are characteristic of the observations themselves. Furthermore, there is a significant systematic error in the data from all three sources, which is absent from the pure observational errors. Finally, the three sources of error do not seem to be independent, since the probable deviation from the total error is only 60% of what one would expect from combining the deviations of the component source errors ($\pm 0;52^h$). Indeed, if we omit no. 1, the probable deviation in Ptolemy's data drops to ($\pm 0;27^h$), which is nearly identical to the corresponding observational error.

The systematic error in Ptolemy's data is puzzling. Since the value of precession which follows from these occultations corresponds very nearly to the correct sidereal motion of the Moon, one might expect similar errors in Ptolemy's data for occultations of the same star. There seems no reason,

	Star	I	II	III	IV	V	VI
1	β Sco	$+1;50^h$	$+0;\ 6^h$			$+1;56^h$	$-0;51.5°$
2	Spica	$+0;57$	$-0;\ 8$			$+0;49$	$-0;20.8$
3	Pleiades	$-0;\ 8$	$-0;\ 2$			$-0;10$	$+0;\ 4.1$
4	Spica	$-1;17$	$+0;44$			$+0;17$	$-0;\ 9.0$
5	Pleiades	$-0;25$	$+0;23$	$-0;41°$	$+1;49^h$	$+1;47$	$-0;40.1$
6	Spica	$+0;25$	$-0;15$	$+0;\ 5$	$+0;13$	$+0;23$	$-0;\ 9.3$
7	β Sco	$-0;45$	$-0;12$	$-0;38$	$+1;51$	$+0;54$	$-0;1.7$

Table 3.7

however, why pairs of occultations of different stars should exhibit similar errors. In general, one should expect errors with different signs in different pairs of occultations, no matter how they were selected.

Furthermore, when we examine the data-errors in the intervals of longitude for the seven stars we find:

Star	Late Observation	less	Early Observation	yields	Interval Error
β Sco	$-0;18.7°$		$+0;51.5°$		$+0;32.8°$
Spica	$-0;\ 9.3$		$+0;14.9^a$		$+0;\ 5.6$
Pleiades	$-0;40.1$		$-0;\ 4.1$		$-0;44.2$
Average	$-0;22.7$		$-0;20.8$		$-0;\ 1.9$

[a] Average value.

The mean interval-error vanishes for all three pairs, while individual pairs show interval-errors substantially greater than the error of 0;8° in Ptolemy's computed intervals. Thus, the errors in Ptolemy's interval-data are largely offset by the errors in his calculated lunar positions.

To have pairs of occultations show the same predetermined value of precession, Ptolemy would have had to find observations in which the errors in the observations themselves, or his reductions of them, just balanced the errors in his calculations and lunar equations. The probability of finding such pairs at random is obviously very small, since both the errors in Ptolemy's lunar equation and the errors in the observations can take on continuous values with either sign. Thus, no matter how Ptolemy erred in reducing his observations, he must have had a large number of observations to work with.

This number need not have been enormous, however. Ptolemy obtains agreement partially because some of the later reports can be broadly interpreted, and also because he chose observations for which he introduced additional errors in reducing the times to the meridian of Alexandria. Thus, he may have selected some observations because of the ambiguity in their reports and the corresponding flexibility which this allowed him in reducing them. Such a basis of selection would increase the likelihood of obtaining a 'fit', although it would still require a considerable number of observations to choose from.

Whatever the case, it is clear that these observations could not have been selected at random, since the probability is negligible that four random pairs, erroneously reduced, should yield the same 'correct', value for precession. This does not necessarily mean that the observations misrepresent the quality of those available to Ptolemy, for he achieves agreement at least partially by assuming circumstances which seem at variance with what was actually observed. Nevertheless, in view of the errors in Ptolemy's lunar equation [see chapter 4], it seems likely that requiring the observations to yield accordant results would cause Ptolemy to choose observations having somewhat larger average errors than a random selection of such observations would have. The difference between the probable error in the observed times of the occultations (± 25 minutes) and the probable error for eclipses (± 11 minutes) may be due to this cause.

While such errors should not be systematic ones, the larger probable errors for a single observation would increase the chance of having a significant systematic error in a small group of observations. Thus, although the six 'good' occultation-observations agree reasonably well with the elements I have adopted, they are much weaker evidence (mean probable error is $\pm 0;11^h$) of the value of the Moon's acceleration than are the sixteen 'good' eclipses reported by Ptolemy (mean probable error is $\pm 0;3^h$).

In sum, the observations of occultations recorded in the *Almagest* appear to exhibit somewhat larger errors than do the observations of eclipses, which may be related to a requirement that they yield accordant values of precession. Further, the data Ptolemy accepts show much larger errors than the observations themselves, which suggests that Ptolemy reported the observations faithfully. Finally, Ptolemy's use of these observations is an excellent example of his obtaining both correct and consistent results from very poor data.

OBSERVATIONS OF THE MOON'S ELONGATIONS

Ptolemy reports eleven measurements of the elongation of the Moon from other celestial bodies. Three of these are Hipparchus' observations of the elongation of the Moon from the Sun, which are the latest of his known observations. Ptolemy uses these together with a similar observation of his own (139 Feb 9) both to demonstrate the correctness of his lunar model at quadrature and in the octants and also to illustrate his procedure for determining the magnitude of the second lunar inequality.

The remaining eight observations were made by Ptolemy and used to determine the longitudes of Regulus and of each of the planets. For each planet, Ptolemy measures the Moon's elongation and then determines the longitude of the planet from this datum and his computed apparent longitude of the Moon. Each of these observations is accompanied by a direct measurement of the elongation of the planet from a star of known longitude. Thus, three observed data are in effect given for each observation: the distance[31] of the Moon from the planet, the distance of the planet from a star, and implicitly, the distance of the Moon from the star. In discussing the errors in these observations, I shall consider the errors in each of these data as if they were independent observations.

Ptolemy's eight observations of the distance of the Moon from other celestial bodies are distinct from all his other observations. First of all, he made them during the seven months from +138 Dec 16 to +139 Jul 11, and they are the only observations he reported for this interval. More importantly, unlike his other observations, each includes an observable datum from which he could have accurately determined the time of the observation.

For each observation Ptolemy notes the (computed) longitude of the Sun and states the time of the observation with the remark, 'since [such and such] a degree was culminating on the astrolabe'. This suggests a procedure which he may have used generally to determine the time of the observations, but which he does not mention elsewhere in the *Almagest*.

In describing how to use an armillary astrolabe [*Alm.* v 1: Toomer, 218–219], Ptolemy tells us that the ecliptic-ring on the astrolabe is aligned in the plane of the ecliptic by setting one of the rings (the outer) perpendicular to it at the known longitude of some celestial body, and then turning the instrument about the poles of its equator until the reference-body is aligned with this ring. With the ecliptic thus properly oriented, the longitude of

[31] In what follows I shall use 'distance', 'elongation', and 'interval of longitude' as synonyms, except where otherwise noted.

the celestial body to be observed can be found directly by aligning it with the other ring perpendicular to the ecliptic.

Ptolemy does not add that the time of the observation can then be readily determined by observing the degree of the ecliptic which was culminating. The culminating degree could easily be read from the intersection of the meridian-ring and the ecliptic-ring, and the apparent time of the observation could then be determined from the difference in right ascension between the culminating degree and the longitude of the Sun.

By following this procedure Ptolemy could have determined the time of any observation to within at least ± 4 minutes, or even to within half this amount. Moreover, the procedure could be simplified for observations of elongations such as those described below, where it is not necessary to determine the time of an event over which the observer has no control. Thus, Ptolemy could compute in advance the culminating degrees for a group of times, set his astrolabe so that a desired degree culminated, and then wait until the reference body aligned itself on the ring set at its longitude. At that moment he could then observe the longitude of any other celestial body by adjusting only one ring on his instrument.

Ptolemy's reference to the culminating degree in connection with the time of each of his elongation-observations implies that he employed some procedure of this sort. Further, the fact that he quotes all the times to quarter, half, or integral hours suggests that he computed the culminating degrees for convenient times before making the observations, and thus made his measurements at predetermined times. Since he had to compute the culminating degree for each observation in order to determine the Moon's parallax, this procedure would have involved no additional labor while offering the practical advantage of requiring minimum manipulation of the astrolabe at the moment of observation.

If Ptolemy determined the times of his observations in this manner—and it seems probable that he did—then his times, which are quoted to quarters of an hour, may be regarded as the results of an orderly and rational procedure for making observations, and not as rough approximations of the times of his observations. As noted above, by such a method Ptolemy should have been able to determine the time of an observation to within at least ± 4 minutes.[32] Hence, the errors of these observations should be

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[32] In general, this method can determine the time of an observation with considerably more accuracy than can altitude-measurements of comparable precision. The error in the time which would result from an error of $\pm 1^\circ$ in either the culminating degree or in the computed position of the Sun would always be smaller than ± 1.36 minutes. In contrast, the error in the time which would result from an error of 1° in a measurement of the altitude of a body whose declination was

almost entirely due to errors in his measurements of the distance between the Moon and the reference bodies used in the observations.

The main objective of the following remarks is to determine the average error of Ptolemy's observations of elongation. In computing the Moon's positions, I have used P. V. Neugebauer's lunar tables [1912] corrected to the elements adopted above. Although these give the longitude of the Moon to 0.01°, the resulting longitudes are certain only to within +0.1° = +0;6°. In general, this uncertainty will not significantly affect the results of the comparisons.

Elongation 1. −127 Aug 5 *Alm.* v 3: Toomer, 224

51[33] Callipic Period III: 16 Epiphi

> when two thirds of the first hour had passed. 'The speed (δρόμος) was [that of day] 241',[34] he says, 'and while the Sun was sighted [at][35] Leo 8;35°, the apparent position of the Moon was 12;20° Taurus ... '.

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accurately known, and at a place whose terrestrial latitude was also accurately known, is:

$$\Delta(t) = \frac{4 \text{ minutes}}{\cos\varphi \cdot \sin A}\,.$$

For the latitude of Alexandria, therefore, the minimum error in a time determined from an altitude-measurement in error by ±1° is ±4.68 minutes (Azimuth (A) = ±90°), while a similar error in an observed altitude at ±45° of azimuth would produce an error in the time of more than 6 minutes.

33 Cf. Manitius 1912, i 266na: 'All mss have "in the 50th year",' but Ideler and Ginzel (Chron. II: 410) have shown that one must read "51"'.

34 The meaning of 'the δρόμος was 241', attested in all mss., has been the subject of much uncertainty and confusion. Halma emends σμα′ to μέσος and thus understands the phrase to indicate that the Moon was at mean distance. Manitius understands δρόμος to refer to the anomaly and changes σμα′ to σνθ′ (259) in order to make Hipparchus' anomaly agree nearly with that found in Ptolemy (257;47°). Toomer [244n14], following Alexander Jones [1983], gives a convincing explanation, which links this description to a table (of Babylonian origin) of the true motion of the Moon over 248 days (9 anomalistic months). See A. Jones 1983, for a detailed discussion of such tables.

35 Observations nos. 1, 2, and 3 suggest that Hipparchus possessed an instrument similar to Ptolemy's, and that his procedure for the observation was the same as Ptolemy's. Thus, one ring would be set at the computed place of the Sun, Leo 8;35°, and the daily circle turned until the Sun was aligned on that ring, at

This observation, like several others, shows excellent agreement between the equation which Ptolemy obtains and that derived from modern lunar theory, but much poorer agreement between modern theory and the observed data from which Ptolemy derives his result. This agreement is partly because of the error in Ptolemy's solar equation and partly because he uses an erroneous value for the equation of time.

Elongation-Data	Ptolemy	Computed	Δ
Apparent Time (Rhodes)	5;50^h	5;50^h [a]	
True Solar Longitude	128;20°	128;40°	+0;20°
Refraction (Longitude)		−0; 6[b]	
Apparent Solar Longitude	[128;20]	128;34	+0;14
True Lunar Longitude	42; 5	42; 1	
Lunar Parallax (Longitude)	0; 0	−0; 6	
Apparent Lunar Longitude	42; 5[c]	41;55	−0;10
Lunar Equation	+7;40[c,d]	7;41	+0; 1[d]
Apparent Elongation of Moon–Sun	273;45[c]	273;21	−0;24
Measured Angular Distance	86;15[c]	86;39	+0;24

[a] Computed using true Sunrise and φ (Rhodes) = 36;24°.

[b] At the time of the observation the Sun's true altitude was 8;36° and the angle between the ecliptic and the Sun's altitude-circle, 160°.

[c] Observed.

[d] In computing the Moon's mean longitude Ptolemy uses −0;5^h for the equation of time instead of the correct −0;16^h. Thus, his observed lunar equation should be 7;45°.

Elongation No. 1: −127 Aug 5

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which point the ecliptic would be properly positioned, and the elongation of the Moon could be directly determined. Such a procedure is supported by Ptolemy's subsequent reduction of the observation, for he accepts the apparent measured elongation, −86;15°, while at the same time correcting Hipparchus' computed position of the Sun to Leo, 8;20° (accurate, 8;22°). Toomer [227n20] presents a contrary view.

Elongation 2. −126 May 2 *Alm.* v 5: Toomer, 227

197 Death of Alexander: 11 Pharmuthi

Hipparchus records that he observed the Sun and the Moon with his instruments in Rhodes... at the beginning of the second hour [of the day]. He says that while the Sun was sighted [at] Taurus 7;45°[36] the apparent position of the center of the Moon was Pisces 21;40°, and its true position was Pisces 21;27,30°.[37]

Elongation-Data	Ptolemy	Computed	Δ
Apparent Time (Rhodes)	6;20^h	6;23^h [a]	
True Solar Longitude	37;45°	37;34°	−0;11°
Refraction (Longitude)		−0; 2[b]	
Apparent Solar Longitude	[37;45]	37;32	−0;13
True Lunar Longitude	351;27,30	350;47	
Lunar Parallax (Longitude)	+0;12,30	+0;15	
Apparent Lunar Longitude	351;40[c]	351; 2	−0;38
Lunar Equation	−0;46	−1;23	−0;37
Apparent Elongation of Moon–Sun	313;55[c]	313;30	−0;25
Measured Angular Distance	46; 5[c]	46;30	+0;25

[a] Computed using true Sunrise and φ (Rhodes) = 36;24°.

[b] The Sun's altitude at this time was 13;30°, so that the total refraction was 0;4°. The angle between the ecliptic and the Sun's altitude-circle was ≈128°.

[c] Observed.

Elongation No. 2: −126 May 2

Elongation 3. −126 Jul 7 *Alm.* v 5: Toomer, 230

197 Death of Alexander: 17 Payni

observed by Hipparchus, as already mentioned, in Rhodes... at 9 1/3 hours. He says that while at this hour the Sun was sighted at Cancer

[36] This agrees with Ptolemy's solar model.

[37] Ptolemy accepts Hipparchus' computation of the Moon's parallax, although elsewhere [*Alm.* v 19: Toomer, 268] he criticizes Hipparchus' procedure for determining the components of parallax in longitude and latitude.

10;54°,[38] the apparent position of the Moon was Leo 29;0°. And this was its true position too; for at Rhodes, near the end of Leo, about one hour past the meridian, the Moon has no longitudinal parallax.

Elongation-Data	Ptolemy	Computed	Δ
Apparent Time (Rhodes)	$16;\ 0^h$	$16;\ 0^h$ [a]	
True Solar Longitude	100;40°	100;53°	+0;13°
True Lunar Longitude	148;46	149;40	
Lunar Parallax (Longitude)	0; 0	−0; 1	
Apparent Lunar Longitude	148;46[b]	149;392	+0;53
Lunar Equation	+1;26[c]	+2;24	+0;58[c]
Apparent Elongation of Moon–Sun	+48; 6[b]	+48;46	+0;40

[a] Computed using true Sunrise and φ (Rhodes) = 36;24°.

[b] Observed.

[c] In computing the mean longitude and elongation of the Moon, Ptolemy does not seem to have applied his correction for the equation of time ($-0;20^h$), since his computed mean longitude and anomaly are 0;13° and 0;11° greater than those which his tables give [cf. Kempf 1878, 27]. Ptolemy should thus have found the lunar equation (obs.) to be +1;39° instead of +1;26°. The error in the observed lunar equation, properly computed, is, therefore, +0;45°.

Elongation No. 3: −126 Jul 7

Elongation 4. +138 Dec 16 *Alm.* x 4: Toomer, 474

2 Antoninus: 29/30 Tybi

we observed the planet Venus after its greatest elongation as morning star, using the astrolabe and sighting it with respect to Spica: its apparent longitude was Scorpio 6;30°. At that moment it was also between and on a straight line with the northernmost of the stars in the forehead of Scorpius [β Sco] and the apparent center of the Moon, and [Venus] was in advance [west] of the Moon's center one and one half times the amount it was to the rear [east] of [β

38 Ptolemy finds Cancer 10;40° (modern value, 10;42°). As in elongation no. 1 he accepts Hipparchus' observed elongation instead of his lunar longitude.

Elongation Data	Ptolemy	Computed	Δ
Apparent Time (Alexandria)	$4;45^{h}$	$4;46^{h}$ [a]	
Longitude of Venus	216;30	217;18[b]	
Latitude of Venus	(+2;40)	+3; 1	
Longitude of Spica	176;40°	177;58° [c]	+1;18°
Elongation of Venus–Spica	+39;50° [d]	+39;20°	−0;30°
Longitude of β Sco	216;20	217;18[e]	+0;58
Latitude of β Sco	+1;20	+1;15	
True Lunar Longitude	215;45[f]	215;47	
Lunar Parallax (Longitude)	+1; 0	+0;51	−0; 9
Apparent Lunar Longitude	216;45	216;38[g]	−0; 7
Lunar Latitude	+5; 0	+5; 8	
Lunar Parallax (Latitude)	−0;20	−0;16	
Apparent Lunar Latitude	+4;40	+4;52	+0;12
Elongation of Moon–Venus	+0;15	−0;40	−0;55
Venus–β Sco	+0;10	0; 0[h]	−0;10
Moon–Line through Venus and β Sco (Long.)	0; 0[d]	−0;40	−0;40[h]
Elongation of Moon–Spica	+40; 5	+38;40	−1;25

[a] Computed for 152;30° culminating, φ (Alexandria).

[b] Computed from Tuckerman [1962–1964], and reduced by 0;3°, which is my correction to the solar longitude.

[c] Peters and Knobel 1915, 62, corrected for precession to +139.0.

[d] Observed.

[e] Peters and Knobel 1915, 63, corrected for precession. P. V. Neugebauer [1914, 64] makes the longitude of β Sco 217;11° in +139.0.

[f] To compute the Moon's true longitude, Ptolemy takes the lunar equation to be −5;39°; but if accurately computed from Ptolemy's table [*Alm.* v 9: Toomer, 286], it is −0;5;52°. Thus, Ptolemy's computed longitude of the Moon should be reduced by 0;13°. In contrast, the lunar equation from modern theory at the time of the observation was −6;39°. So, the error in Ptolemy's theoretical equation at this observation is −0;47°, while the error in the equation he uses is −1;0°. This accounts for the agreement between Ptolemy's lunar longitude and the modern value, which should differ (on average) by ≈ 1;6°.

[g] At the time reported for the observation, the altitudes of Venus and the Moon were respectively 18;34° and 19;59°; thus, the total refraction of each was ≈ 0;2.5°. Since the angle between the ecliptic and the altitude-circle through Venus was then ≈ 160°, the refraction in longitude of each (not included in the computed longitudes) was ≈ −0;2°.

[h] If 217;11° is the longitude of β Sco, Venus will be 0;7° ahead of β Sco at the time of the observation, and the Moon will be 0;47° behind the line through Venus and β Sco.

Elongation No. 4a: +138 Dec 16 ($4;45^{h}$)

Sco] ... the time was 4;45^h after midnight since the Sun was about Sagittarius 23° and the second degree of Virgo was culminating [on] the astrolabe.

Elongation-Data	Computed	Ptolemy	Δ
Longitude of Venus	217;23°		
True Lunar Longitude	216;48		
Lunar Parallax (Longitude)	+0;38		
Apparent Lunar Longitude	217;26		
Elongation of Venus–Spica	+39;25	+39;50°	−0;25°
Elongation of Venus–β Sco	+0; 5	+0;10	−0; 5
Moon–Venus	+0; 3	+0;15	−0;12
Moon–Line through Venus and β Sco (Long.)	−0; 4	0; 0	+0; 4
Elongation of Moon–Spica	+39;28	+40; 5	−0;37

Elongation No. 4b: +138 Dec 16 (6;35^h)

All of the Moon-planet observations in this group involve two measurements: the distance from the planet to the reference star (here Spica), and distance between the Moon and the planet (in this case with reference to an additional star). These yield a distance between the Moon and the reference star, which I shall treat as if it were an observation.

The difficulty with this observation is not only that all the errors are large, but also that at the time given for the observation the Moon was not even close to being co-linear with Venus and β Sco [cf. Figure 3.1]. The computed longitude of Venus should not be in error by more than +0;3° [cf. Tuckerman 1962–1964, i 6, 12], while the longitude of the Moon is uncertain by no more than +0;6°. To achieve co-linearity, therefore, the Moon's acceleration would have to be increased by at least $4.0''T^2$, a correction which would leave the eclipses and occultations poorly represented.

The circumstances Ptolemy describes could have been observed shortly before Sunrise, which occurred at Alexandria at 6;54^h, or 2;9^h after the time Ptolemy reports for the observation. The calculations for elongation no. 4b show the situation at 6;35^h, which corresponds to an error of exactly one sign (30°) in the culminating degree.

Thus, if the observations were made either 2;0^h (6;46^h) or one culminating zodiacal sign (i.e., at 6;35^h) after the time reported, Ptolemy's description of the alignment of Scorpio, Venus, and the Moon would agree very well with the computed circumstances [cf. Figure 3.2], for both Venus and the Moon would then be east of β Sco. Also the errors in the distances between Venus and Spica and the Moon and Spica would be smaller.

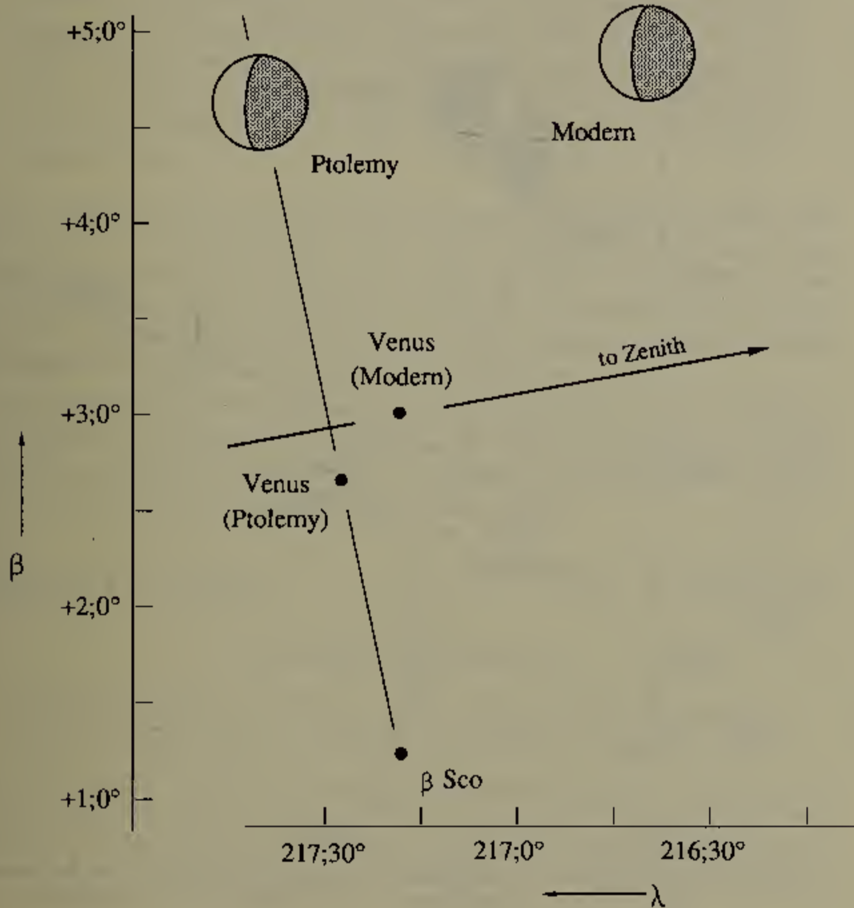

Figure 3.1. Elongation No. 4a: +138 Dec 16 (4;45^h)

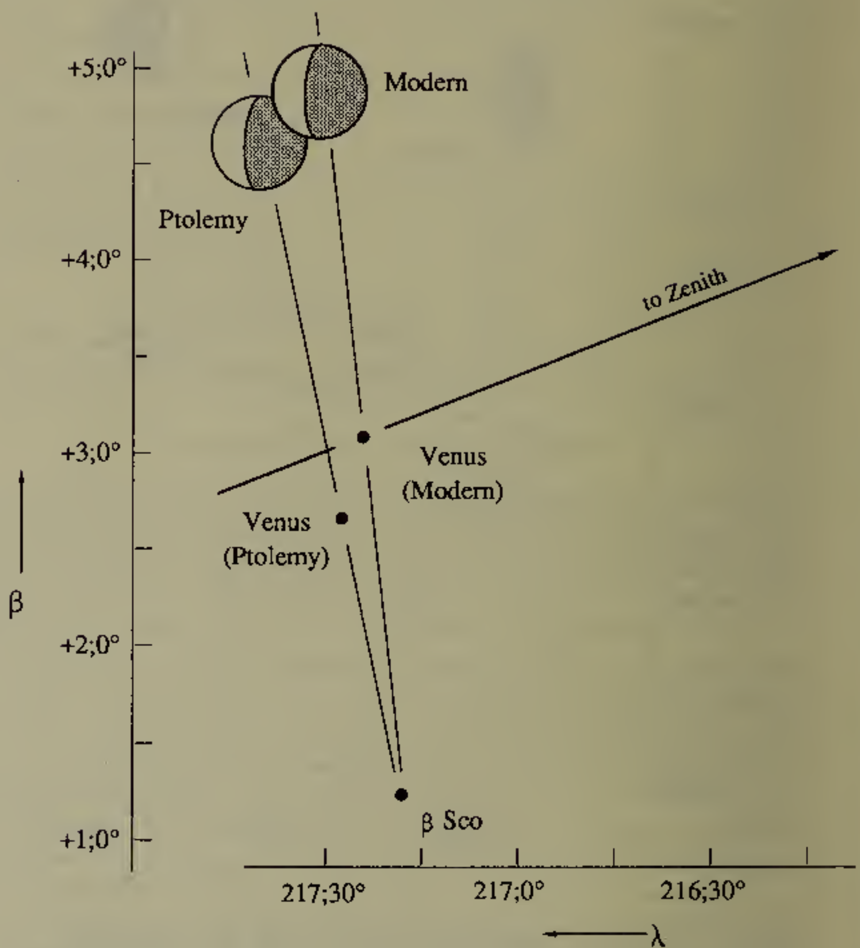

Figure 3.2. Elongation No. 4b: +138 Dec 16 (6;35^h)

Elongation 5. +138 Dec 22 — *Alm.* xi 6: Toomer, 538

2 Antoninus: 6/7 Mechir

> It was 4 equinoctial hours[39] before midnight, for according to the astrolabe the last degree of Aries [30°] was culminating, while the longitude of the mean Sun was Sagittarius 28;41°. At that moment Saturn, sighted with respect to [Aldebaran] was seen to have a longitude of Aquarius 9;4°, and was about $^1/_2$° to the rear [east] of the center of the Moon (for that was its distance from the Moon's northern horn).

At the time reported for the observation Saturn was behind the Moon [see Elongation No. 5a, Figure 3.3]. Thus, like the previous observation, the circumstances Ptolemy describes could not have been observed at the time which he reports. Since the Moon set at 20;4^h,[40] it is possible that the time (or rather the culminating degree) reported by Ptolemy was that at which he observed Moonset and thus the setting of Saturn.

The observed data Ptolemy reports for the distances between the Moon and Saturn and (implicitly) the Moon and Aldebaran agree very closely with the computed circumstances 1;0^h earlier. See Elongation No. 5b for the situation at 19;0^h (apparent time, Alexandria).

Elongation 6. +139 Feb 9 — *Alm.* v 3: Toomer, 223

2 Antoninus: 25 Phamenoth

> We sighted the Sun and Moon... after Sunrise,[41] and 5;15 equinoctial hours before noon. The Sun was sighted in Aquarius 18;50°

[39] Ptolemy seems to have computed the time (20;0^h) from the culmination of 30;30°, using the position of the mean Sun as given, instead of the true Sun. The latter gives 19;56^h for the time, the solar equation being +0;58°. In computing the position of the Moon, I have assumed that 30;30° was culminating on Ptolemy's astrolabe, and thus that the true time of the observation was 19;56^h.

[40] At 19;56^h the Moon's right ascension was 313;9°, its declination was −19;17°, and its hour-angle was 76;14°. For Saturn the corresponding quantities were right ascension 313;1°, declination −19;15°, and hour-angle 76;22°. Both bodies set at t = 77;47°. Thus, Moonset occurred at 20;2,12^h plus 2 minutes for refraction. Saturn (behind the Moon) set at 20;1,40^h + 0;2^h.

[41] True Sunrise occurred at 6;38^h, and apparent Sunrise at 6;36^h. Cf. Ptolemy, *Alm.* ii 13: Sunrise = 6;38^h.

Elongation-Data	Ptolemy	Computed	Δ
Apparent time (Alexandria)	20; 0^h	19;56^h	
Longitude of Aldebaran	42;40°	43;52°	+1;12°
Longitude of Saturn	309; 4	310; 7	
Refraction (Longitude)		+0;20[a]	
Apparent Longitude of Saturn	309; 4	310;27	+1;23
Latitude of Saturn	(−1;21)[b]	−1;25[c]	
Elongation of Saturn–Aldebaran	266;24[d]	266;35	+0;11
Measured Angular Distance	93;36	93;25	−0;11
True Lunar Longitude	309;40	311;11	
Lunar Parallax (Longitude)	−1; 6	−0;57	+0; 9
Apparent Lunar Longitude	308;34	310;14	
Refraction (Longitude)		+0;19[a]	
Refracted Apparent Lunar Long.	(308;34)	310;33	+1;59
True Lunar Latitude	(−1; 0)[b]	−1;10	
Lunar Parallax (Latitude)	(−0;36)[b]	−0;19	
Apparent Lunar Latitude	(−1;36)[b]	−1;29[c]	+0; 7
Elongation of Moon–Saturn	−0;30[d]	+0; 6	+0;36
Elongation of Moon–Aldebaran	265;54	266;41	+0;47

[a] At 19;56^h the altitude of Saturn was 1;7°. and the altitude of the Moon was 1;13°. The total refraction of each was, thus, +0;21° and +0;20°, respectively. At this time, the angle between the altitude-circle through Saturn and the ecliptic was ≈19°.

[b] Cf. Manitius 1912, ii 428–4299n22.

[c] The refraction in latitude of both Saturn and the Moon is +0;6°. I have not included it, since it does not affect the results.

[d] Observed.

Elongation No. 5a: +138 Dec 22 (19;56^h)

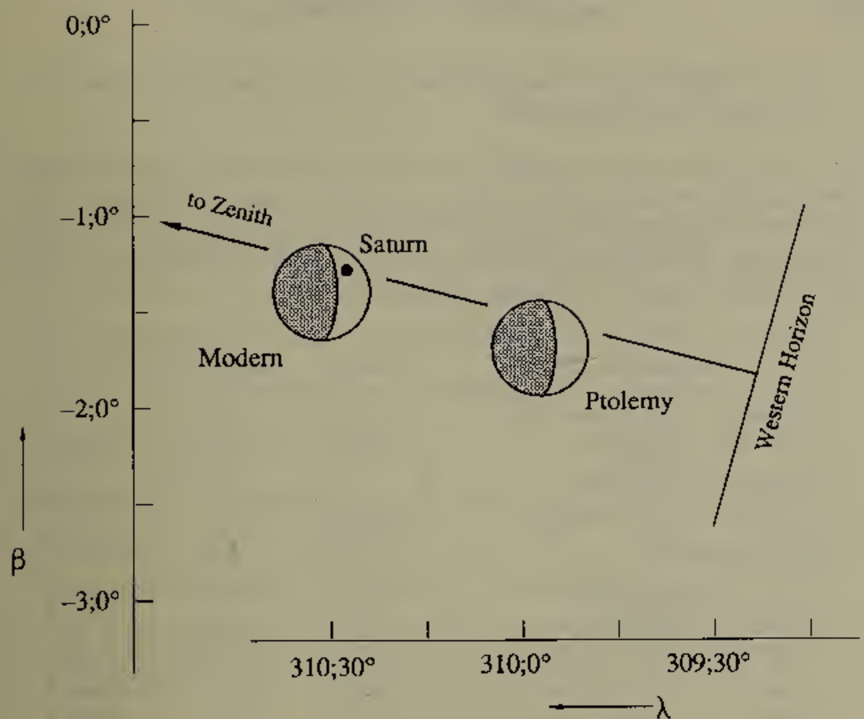

Figure 3.3. Elongation No. 5a: +138 Dec 22 (19;56^h)

Elongation Data	Ptolemy	Computed	Δ
True Lunar Longitude		310;35°	
Parallax (Longitude)		−0;54	
Refraction (Longitude)		+0; 4[a]	
Apparent Lunar Longitude		309;45	
Apparent Longitude of Saturn		310;11[a]	
Elongation of Saturn–Aldebaran	266;24	266;19°	−0; 5°
Elongation of Moon–Saturn	−0;30	−0;26	+0; 4
Elongation of Moon–Aldebaran	265;54	265;53	−0; 1

[a] At 19;0^h the Moon's altitude was 12;40°, Saturn's altitude was 12;15°, and the total refraction was ≈ +0;4° for both. The angle between the ecliptic and the altitude-circle through Saturn was ≈ 24°. Saturn's 'apparent longitude' includes the correction for refraction.

Elongation No. 5b: +138 Dec 22 (19;09^h)

and as Sagittarius 4° was culminating. The apparent position of the Moon was Scorpio 9;40° ...

The observation could not have been made more than 8 minutes earlier, since the Sun would not have been completely above the horizon. Furthermore, even if the observation were made just at Sunrise, the change in refraction would increase the error. Thus, the error shown is very nearly the minimum possible under any assumption.

Elongation-Data	Ptolemy	Computed	Δ
Apparent Time (Alexandria)	6;45^h	6;45^h [a]	
True Solar Longitude	318;50° [b]	319;23°	+0;33°
Refraction (Longitude)		−0;12[c]	
Apparent Solar Longitude	(318;50)	319;11	+0;21
Lunar Equation	−7;40[d]	−7;31	+0; 9
True Lunar Longitude	219;40	220;53	
Parallax (Longitude)	0; 0	0; 4	
Apparent Lunar Longitude	219;40	220;49	+1;19
Elongation of Moon–Sun (Long.)	260;50[d]	261;38	+0;48
Measured Angular Distance	99;10[d]	98;22	−0;48

[a] Computed from 244;30° culminating, β (Alexandria) = 31;12°.

[b] Computed: 318;44°.

[c] At 6;45^h the Sun's altitude was 1;40°, the total refraction was 0;18°, and the angle between the ecliptic and the Sun's altitude-circle was ≈ 132°.

[d] Observed.

Elongation No. 6: +139 Feb 9

Despite the large error in the observation, the lunar equation Ptolemy derives is quite accurate. This is largely due to the error in his solar equation, which is near its maximum (−0;26°), and also to the effect of refraction.

Elongations 7 and 8. +139 Feb 23 *Alm.* vii 2: Toomer, 328

2 Antoninus: 9 Pharmuthi

when the Sun was just about to set in Alexandria,[42] and the last degree of Taurus was culminating, i.e., 5;30 equinoctial hours after

[42] Apparent Sunset at Alexandria (φ = 31;12°) occurred at 17;37^h.

Elongation-Data	Ptolemy	Computed	Δ
Apparent Time (Alexandria)	$17;30^h$	$17;29^h$ [a]	
True Solar Longitude	333; 3°	333;44°	+0;41°
Refraction (Longitude)		−0;21[b]	
Apparent Solar Longitude	(333; 3)	334; 5	+1; 2
True Lunar Longitude		66;11	
Parallax (Longitude)		+0; 5	
Apparent Lunar Longitude	65;10	66;16	+1; 6
Apparent Elongation of Moon–Sun	97; 7,30[c]	92;11	+0; 4

[a] Computed from 59;30° culminating, φ (Alexandria) = 31;12°.

[b] At $17;30^h$ the Sun's altitude was 1;10°, the total refraction was 0;21.5°, and the angle between the ecliptic and the altitude-circle through the Sun was ≈ 10°.

[c] Observed.

Elongation No. 7: +139 Feb 23

Elongation-Data	Ptolemy	Computed	Δ
Apparent Time (Alexandria)	$18;\ 0^h$	$17;59^h$ [a]	
Longitude of Regulus	122;30°	124; 3°	+1;33°
True Lunar Longitude		66;27	
Parallax (Longitude)		−0; 2[b]	
Apparent Lunar Longitude	65;20	66;25	+1; 5
Elongation of Regulus–Moon	57;10[c]	57;38	+0;28

[a] Computed from 67;30° culminating, φ (Alexandria) = 31;12°.

[b] Ptolemy estimates that the Moon's parallax changes by −0;5° between the two observations. In fact, it changes by −0;6°.

[c] Observed.

Elongation No. 8: +139 Feb 23

> noon, ... we observed the apparent distance of the Moon from the Sun (which was sighted at about Pisces 3°) as 92;7,30°. Half an hour later, the Sun now having set and Gemini 7;30° culminating, the Moon was sighted in the same position [with respect to the astrolabe ring], and [Regulus] had an apparent distance from the Moon, [as measured] by means of the other astrolabe [ring], of 57;10° towards the rear [east] along the ecliptic.

In the first observation [see elongation no. 7], the error due to neglecting refraction very nearly compensates for the error in Ptolemy's solar equation (−0;22°). In the second observation [see elongation no. 8], the position of the Moon is in good agreement with the modern position except for the systematic error in Ptolemy's equinox (+1;6°). Thus, apart from this systematic error, the error in the longitude of Regulus arises almost entirely from the error in Ptolemy's measurement of the distance from Regulus to the Moon [cf. Kepler 1607, 383].

Elongation 9. +139 May 17 *Alm.* ix 10: Toomer, 461

2 Antoninus: 2/3 Ephiphi

> We observed the planet Mercury... by means of the astrolabe instrument. It had not yet reached its greatest elongation as evening star. When sighted with respect to [Regulus], it was observed at a longitude of Gemini 17;3°; and at that moment it was 1;10° to the rear [east] of the Moon's center. The time at Alexandria was 4;30 equinoctial hours before midnight... since according to the astrolabe, the 12th degree of Virgo[43] was culminating, while the Sun was in about Taurus 23°.

Ptolemy's distance from the Moon to Regulus is in good agreement with the computed distance, but his distance from Mercury to either body is in error by about $^1/_2$°. Mercury could not have been seen 1;10° ahead of the Moon, since the Sun set only $^1/_2{}^h$ before the time reported for the observation.

[43] Toomer and Halma [1813–1816, ii 183] understand 'the twelfth degree of Virgo', whereas Manitius reads 'Virgo 0;5°'. The first reading is undoubtedly correct since, according to Ptolemy, Virgo 0;5° would culminate at 18;47^h instead of 19;30^h, when Virgo 12;30° culminated.

Elongation-Data	Ptolemy	Computed	Δ
Apparent Time (Alexandria)	$19;30^h$	$19;30^h$ [a]	
Longitude of Regulus	122;30°	124; 3°	+1;33°
True Longitude of Mercury	77;30	78;28[b]	
Refraction (Longitude)		+0; 4[c]	
Apparent Longitude of Mercury	(77;30)	78;32	+1; 2
Elongation of Regulus–Mercury	45; 0[d]	45;31	+0;31
True Lunar Longitude	77;10	78;43	
Parallax (Longitude)	−0;50	−0;52	−0; 2
Apparent Lunar Longitude	76;20	77;51	
Refraction (Longitude)		+0; 4[c]	
Apparent Refracted Lunar Longitude	(76;20)	77;55	+1;35
Elongation of Moon–Mercury	−1;10	−0;37	+0;33
Elongation of Moon–Regulus	−46;10	−46; 8	+0; 2

[a] See 112n42, above.

[b] Computed from Tuckerman [1962–1964, ii] and corrected by −0;3°.

[c] At $19;30^h$ the altitude of Mercury was 13;12° and the altitude of the Moon was 12;35°. The total refraction was 0;4°, and the angle between the ecliptic and the altitude-circle through Mercury was ≈ 23°.

[d] Observed.

Elongation No. 9: +139 May 17

Elongation 10. +139 May 30 *Alm.* x 8: Toomer, 499

2 Antoninus: 15/16 Ephiphi

> three days after the third opposition, ... 3 equinoctial hours before midnight. The twentieth degree of Libra was culminating according to the astrolabe, while the mean Sun was in Gemini 5;27° at that moment. Now when [Spica] was sighted in its proper position [on the instrument] (176;40°), Mars was seen to have a longitude of Sagittarius 1;36°. At the same time it was observed to be the same distance (1;36°) to the rear [east] of the Moon's center.

No error in the time of the observation will significantly alter the error of nearly a degree in the observed distance from Mars to Spica. This datum is curious, since Ptolemy seldom reports measurements made with his astrolabe to fractions other than multiples of 0;10°. If the distance between Mars and the Moon was estimated, rather than measured, the error of 0;36° is not unreasonable, since the distance in latitude between the two was more than 6°.

Elongation-Data	Ptolemy	Computed	Δ
Apparent Time (Alexandria)	21; 0^h	121; 1^h [a]	
Longitude of Spica	176;40°	177;58°	+1;18°
Longitude of Mars	241;36	242; 1[b]	
Latitude of Mars		−3;14	
Elongation of Mars–Spica	64;56	64; 3	−0;53
True Lunar Longitude	239;20	240;22	+1; 2
Parallax (Longitude)	+0;40	+0;39	−0; 1
Apparent Lunar Longitude	240; 0	241; 1	+1; 1
Apparent Lunar Latitude		+3;21	
Elongation of Moon–Mars	−1;36	−1; 0	+0;36
Elongation of Moon–Spica	63;20	63; 3	−0;17

[a] Computed from 200;30° culminating, φ (Alexandria) = 31;12°

[b] Computed from Tuckerman [1962–1964, ii] and corrected by +0;6° to compensate for a correction of −0;3° in the Earth's heliocentric position.

At 21;0^h the altitude of Mars was ≈ 27° and the altitude of the Moon was ≈ 31°. The total refraction of both was less than 0;2° and has been neglected here. The angle between the ecliptic and the altitude-circle through Mars was ≈ 136°.

Elongation No. 10: +139 May 30

Elongation 11. +139 Jul 11 *Alm.* xi 2: Toomer, 520

2 Antoninus: 26/27 Mesore

before Sunrise,[44] i.e., about 5 equinoctial hours after midnight (for the mean longitude of the Sun was Cancer 16;11° and the second degree of Aries was culminating according to the astrolabe). At that

[44] Apparent Sunrise occurred at Alexandria (φ = 31;12°) at 5;0^h, and true Sunrise at 5;2^h.

moment Jupiter, when sighted with respect to [Aldebaran], was seen to have a longitude of Gemini 15;45°, and also had the same apparent longitude as the center of the Moon, which lay to the south of it.

Elongation-Data	Ptolemy	Computed	Δ
Apparent Time (Alexandria)	5; 0^h	4;58^h [a]	
Longitude of Aldebaran	42;40°	43;52°	+1;12°
Longitude of Jupiter	75;45	76;34[b]	
Latitude of Jupiter		−1;55	
Elongation of Jupiter–Aldebaran	33; 5[c]	32;42	−0;23
True Lunar Longitude	74;50	75;38	+0;48
Parallax (Longitude)	+0;55	+0;42	−0;13
Apparent Lunar Longitude	75;45	76;20	
Latitude	(−2;10)	−3;23	
Elongation of Moon–Aldebaran	33; 5	32;28	−0;37

[a] Ptolemy takes 5;0^h as his datum, but this must have been computed using the mean Sun (106;11°) instead of the true Sun (104;41°). With the Sun at 106;11°, the culmination of 2;30° yields a time of 4;59^h, while the same culminating degree with the Sun at 104;41° yields a time of 5;5^h [cf. elongation no. 5]. Since the Sun rose at 5;0^h, the actual time of the observation must have been a few minutes earlier. I have computed the position of the Moon for 4;58^h.

[b] At 5;0^h the altitude of Jupiter was ≈ 25°, while the altitude of the Moon was ≈ 23°. The total refraction of each was ≈ 0;1°, which I have omitted in the computations.

[c] Observed.

Elongation No. 11: +139 Jul 11

At the time of the observation, the center of the Moon had not reached the longitude of Jupiter, but the edge of the Moon's disk was south of the planet.

Errors in Ptolemy's elongation-observations and data

The errors of the observations of elongation discussed above are collected in Table 3.8. Table 3.8a shows the errors of the observations which involve only the Moon and some reference body, while Table 3.8b gives the errors of the observations which involve the Moon, a planet, and a reference star. In Table 3.8a, column I gives the error in the observed distance ($< 180°$) between the Moon and the reference body; column II, the error in the Moon's positive elongation from the Sun or star; and column III, the error in the datum which Ptolemy derives from the observation. Except for elongations nos. 7 and 8, the errors in column III are the errors in the lunar equations which Ptolemy obtains from the corresponding observations.

In Table 3.8b, the first three columns give the errors in the observed distances between (I) the planet and reference star, (II) the Moon and the planet (or the Moon's position relative to the alignment with the planet which Ptolemy describes in elongation no. 4), and (III) the Moon and the reference star. Columns (II′) and (III′) give the errors in the Moon's sidereal elongation corresponding to the errors in columns II and III.

For elongations nos. 4 and 5, the errors in brackets are those which result if we assume that no. 4 was made at a time corresponding to an error of one sign in the culminating degree ($6;35^h$), or $1;50^h$ after the time Ptolemy reports, and that no. 5 was made $1;0^h$ earlier than the reported time. Although there is no evidence that the observations were made at these times, the circumstances Ptolemy describes could not have been observed at the reported times, so these are not merely observational errors. Consequently, I have included only the error in Ptolemy's measurement of the distance between the planet and the reference star in nos. 4 and 5, in determining the average errors.

Disregarding the signs of individual errors, I find the following average errors for different groups of observations:

No.	Elongation Observed	Observer	Average Error	Number of Observations
1	Moon–Sun	Hipparchus	±0;29.6°	3
2	Moon–Sun and Regulus	Ptolemy	±0;26.6	3
3	Planet–Reference Star	Ptolemy	±0;29.7	5
4	Moon–Planet	Ptolemy	±0;27.7	3
5	Moon–Reference Star (Implicit)	Ptolemy	±0;18.7	3

The uniformity in the average error for each group is striking. It is particularly noteworthy that the mean error in Hipparchus' three observations is slightly larger than for Ptolemy's comparable observations; thus, there is no evidence that Ptolemy depended on Hipparchus' 'superior observations' in determining the Moon's second inequality.

The relatively small average error in the Moon's implicit elongation from a reference star is something of an anomaly, since we should expect the av-

No.	Date	Place	Δ Obs. Angle I	Δ Lunar Elong. II	Δ Ptolemy's Datum III
1	−127 Aug 5	Rhodes	+0;24°	−0;24°	+0; 1°
2	−126 May 2	Rhodes	+0;25	−0;25	+0;37
3	−126 Jul 7	Rhodes	+0;40	−0;40	+0;58
6	+139 Feb 9	Alexandria	−0;48	+0;48	−0; 9
7	+139 Feb 23	Alexandria	+0; 4	+0; 4	+0; 4
8	+139 Feb 23	Alexandria	+0;28	+0;28	−0;28

Table 3.8a. Errors in Single Observations of Lunar Elongation from the Sun or a Star

No.	Date	Place	Δ Observed Distance			Δ Elongation	
			Star–Planet I	Moon–Planet II	Moon–Star III	Moon–Planet II′	Moon Star III′
4	+138 Dec 16	Alexandria	−0;30° [−0;25]	+0;40°	−1;25°	−0;40° [+0; 4]	−1;25° [−0;37]
5	+138 Dec 22	Alexandria	−0;11 [−0; 5]	+0;36	−0;47	+0;36 [+0; 4]	+0;47 [−0; 1]
9	+139 May 17	Alexandria	+0;31	−0;33	−0; 2	+0;33	+0; 2
10	+139 May 30	Alexandria	−0;53	−0;36	−0;17	+0;36	−0;17
11	+139 May 30	Alexandria	−0;23	+0;14	−0;37	−0;14	−0;37

Table 3.8b. Errors in Multiple Observations of Lunar Elongation from a Planet or Star

erage error in the result of two independent measurements to be larger than the average error for only one such measurement. Although the number of observations in each group is too small to support a firm conclusion, it seems likely that this distance was one of the observed data in at least some of the observations. In particular, it seems probable that this was the case in no. 10, where the distance between the Moon and Spica was found to be 63;20°, while distances between the Moon and Mars and Spica and Mars were found to be 1;36° and 64;56°.

Combining these results we find:

	Mean and Probable Error	Number of Observations
All Lunar Elongation-Observations excluding Nos. 4 and 5	$+0;\ 1.5° \pm 0;20°$	12
All Observations excluding Lunar Observations in Nos. 4 and 5	$-0;\ 2.2\ \ \pm 0;20$	17

Although the signs of the errors in the observed distances are not evenly distributed, the signs of the corrections to the Moon's positive elongations are so distributed (6–6). Thus, although Hipparchus' observations all understate the distance observed, while Ptolemy's tend to overstate it, these systematic errors do not appear in the errors of the observed (positive) elongations. Indeed, from the twelve secure lunar observations the mean systematic error in the Moon's elongation is only +0;1.5°, which may be regarded as negligible.[45] We may, therefore, take the probable error in an observation of the Moon's (positive) elongation from a star, a planet, or the Sun to be ±0;20°.

Ptolemy's reductions of the observations have normal errors of ±0;5° in his computed parallax, and ±0;9° due to his neglecting refraction. Moreover, where the Sun is used as the reference body, as in Ptolemy's determinations of the lunar equations from elongations nos. 1, 2, 3, and 6, we should expect an additional average error of ±0;15° because of the error in Ptolemy's solar equation. Assuming a random distribution of such errors, we should thus expect the probable error in Ptolemy's concluded data to be ±0;27° from observations involving the Sun and ±0;22.5° otherwise.

The errors which we actually find are ±0;23° from observations involving the Sun and ±0;20° for all other observations excluding nos. 4 and 5.

[45] Due to the relatively large errors in the observations the probable error of this mean systematic error is ± 0;5.8°. Thus, although the observations are in excellent agreement with the adopted elements, they are of little value for determining the Moon's acceleration [see 122n47, below].

Ptolemy's results from both types of observations are thus slightly better than what we would expect from the errors of the observations and the errors in his reductions. The difference, however, is small and possibly accidental. I shall, therefore, assume that the probable error in the data which Ptolemy derives from such observations is ±0;25°.

Since each of the elongation-observations discussed above yields results which agree exactly with either those of another observation[46] or (in the case of the Moon–planet–star observations) with Ptolemy's computed position of the Moon, these are probably neither random observations nor the only ones of their sort which Ptolemy made. Although it is possible that Ptolemy altered the reports of the observations to yield these accordant results, assuming that he did so fails to explain how he obtained his values for the second lunar inequality and for precession, which agree closely with what he should have found. Furthermore, in reducing observations nos. 1, 2, 3, 4, and 6, Ptolemy makes significant mistakes in computing the positions of the Sun or Moon, which if corrected would destroy the exact agreement which he appears to find and also reduce the quality of his results. These errors suggest, therefore, that the observations themselves are honestly reported.

This is not to say that all of the observations are accurately reported. As noted above, Ptolemy could not have observed β Sco, Venus, and the Moon in a straight line at the time he reports in elongation no. 4, and Saturn was covered by the Moon at the time reported for no. 5. In view of the other evidence, however, it is more plausible to assume that Ptolemy either mistakenly recorded the culminating degree or used the time of a different observation on the same night in working up these observations, than to assume that his reports were elaborate fabrications.

To explain the apparent agreement among Ptolemy's results we need only assume that he possessed a considerable number of similar observations and that he selected those which illustrated the point he wanted to demonstrate. Such agreement need not have been forced, since for a sufficient number of observations the random errors in both the observations and their reductions should yield a certain number of accordant results.

We have no way of knowing whether the observations Ptolemy reports reflect the general quality of the observations available to him. The criteria of selection discussed above, however, should not greatly affect the quality

[46] In the case of the observation of Regulus the result yields exactly the value of precession which Ptolemy should have found, and which he demonstrated with observations of lunar occultations and stellar declinations. Furthermore, he says that he also found the same result from similar observations of the other bright stars along the ecliptic [*Alm.* vii 2: Toomer, 15].

of observations chosen. If anything, the errors in these observations may be slightly larger than the errors in a truly random sample, since the results agree with Ptolemy's solar and lunar models and so to some extent reflect the errors in these models. On the other hand, if Ptolemy 'fudged' his reductions to obtain this agreement, then we have no reason to assume that the observations are not typical. In either case, it seems doubtful that the errors found in the observations reported should differ significantly from the errors of such observations in general.

SUMMARY

Table 3.9 summarizes the average errors found for different types of observations together with the errors in the data which Ptolemy derives from these observations.[47] Where the errors are in the observed time of an event, the corresponding error in (correction to) the Moon's observed mean longitudes or elongations is also shown.

In general, these errors are consistent with what we should expect from careful, naked-eye observations given the precision of the reports of the different types of observations. The lunar eclipse-reports seldom state the times with a precision greater than half an hour. If these eclipses had been accurately observed and their times correctly given to the nearest half hour, we would expect an average error of $\pm 7\frac{1}{2}$ minutes. Thus, the average additional error due to 'clock-errors' and to errors in observing the recorded phases is probably on the order of ± 8 minutes. In contrast, the times of the occultations are, with one exception, reported in integral hours, leading us to expect an error of ± 15 minutes from the imprecision of the reported times alone. This would leave an average error from other sources of about ± 20 minutes, or more than twice that which appears characteristic

[47] The mean systematic errors and *their* probable errors for observations of eclipse-times, occultations, and elongations are:

Type of Observation	Mean Error	Weight
Eclipse-times (16)	$+0;0.6^\circ \pm 0;1.4^\circ$	17.2
Occultations (6)	$+0;1.1 \pm 0;5.2$	1.2
Elongations (12)	$+0;1.5 \pm 0;5.8$	1.0
Average	$+0;0.68 \pm 0;1.9$	19.4

This error (epoch: -250) corresponds to a correction to the assumed acceleration in elongation of $-0.09''T^2 \pm 0.17''T^2$ or to $S_{D'} = 2.53 \pm 0.17''$.

Type of Observation	No.	Errors in the Observations	Errors in Ptolemy's Data
Lunar Eclipse-Times[a] (Elongations)	16	$-0;1.3^h \pm 0;10.6^h$ $(+0;0.6^\circ \mp 0;\ 5.6^\circ$	$\pm 0;18^h$ $\mp 0;\ 9.1^\circ)$
Lunar Eclipse-Magnitudes			
Babylonian	7	$+0.7^d \pm 0.4^d$	$+0.7^d \pm 0.4^d$
Alexandrian	5	$-0.1^d \pm 0.25^d$	$-0.1^d \pm 0.25^d$
Occultations: Times[b] (Longitudes)	6	$-0;2.2^h \pm 0;25^h$ $(+0;1.1^\circ \mp 0;12.7^\circ$	$+11;\ 1^h \pm 0;29^h$ $-0;25^\circ \mp 0;12^\circ)$
Measurements of Elongation[c]	12	$+0;1.5^\circ \pm 0;20^\circ$	$\pm 0;25^\circ$

[a] Mean epoch = −285. [b] Mean epoch = −123. [c] Mean epoch = +64.

Table 3.9

of eclipses. This larger error is probably to be expected from the difficulty of seeing stars near the illuminated disk of the Moon, as well as from the ambiguity of some of the reports.

The probable error found in the elongation-observations Ptolemy reports is somewhat larger than what we might expect from the precision of ±0;10° which he claims to attain with his astrolabe. Each of these observations, however, required two accurate sightings, one of the Moon or body to be observed, and the other of a reference body. Thus, the probable error in a single reading of the instrument which corresponds to an error of ±0;20° in the measured elongation is 0;14°. In view of the difficulties of observing the centers of the Sun and Moon accurately, such an error is not unreasonable.

In computing the errors in each group of observations, I have excluded the few observations which seemed so discordant as to suggest that significant non-observational errors affected their reports. It is not surprising that some of the observations should indicate such errors, in view of the high probability of either an inadvertence on the part of the observer in working up his observations at a later time, or, in the case of the pre-Ptolemaic observations, of scribal errors. Newcomb [1878, 53] encountered the same problem when analyzing the Arabian observations of lunar and solar eclipses reported by Ibn Yūnus, finding that the errors in roughly 80% of the observations were normally distributed, while the rest were 'so far from fulfilling this condition as to show conclusively that the law in question (normal distribution) does not hold, and therefore that the arithmetical mean is not the most probable final result'.

The presence of anomalous errors is thus not unique to Ptolemy's reports, and is probably to be expected in any group of early astronomical observations. Because such errors disproportionately affect the results of averaging, excluding them more accurately represents the general quality of this type of observation.

Such discordant observations, however, were part of the corpus of observations available to Ptolemy, and they do reflect the quality of the data with which he worked. Hence, in averaging Ptolemy's errors I have generally included the errors from such observations. These errors account for part of the difference between the average errors in Ptolemy's data and those in the actual observations. Errors in Ptolemy's reductions of the observations and solar model account for the rest of the increase.

The relatively large errors characteristic of the observations of occultations and elongations strongly supports Ptolemy's preference for using lunar eclipses wherever possible [*Alm.* iv 1: Toomer, 192]. Not only were eclipses free from parallax, but they also gave the Moon's position (relative to the Sun) with substantially greater accuracy than the other types of observations. Thus, Ptolemy's rejection of observations other than eclipses in establishing his lunar model at syzygy was practically, as well as logically, sound.

4

The Errors of Ptolemy's Lunar Parameters Compared with the Errors of His Observations

The aim of this chapter is to determine whether the accuracy of Ptolemy's lunar parameters is consistent with the average errors in the observations which he reports, as found in the preceding chapter. For convenience, I will divide these parameters into two groups. One consists of the mean motions of Ptolemy's lunar arguments and the values of these arguments in Ptolemy's time; the other includes the parameters of the model by which Ptolemy depicts the inequalities in the Moon's motion.

The parameters in the first group can be compared directly with their modern equivalents and their errors thus easily determined. To compare these errors with what we would expect from the errors of Ptolemy's observations, I have departed from Ptolemy's actual procedure and assumed that each parameter was determined independently of the others, and also that in each determination no error was introduced by errors in the other parameters. In fact, Ptolemy determines several of his parameters simultaneously, so that the errors are not independent. This does not significantly affect the results, however, since my purpose is to ascertain the minimum probable errors of such determinations.

The parameters which depict the inequalities in the Moon's motion according to Ptolemy's lunar model are more difficult to compare meaningfully with modern theory. This is partly because Ptolemy's model is not formally equivalent to Kepler-motion in an ellipse, so that the errors in his inequality do not arise from the errors in his parameters alone. Instead, the limitations of his model, even with optimal parameters, produce periodic errors which are often greater than the errors due to his parameters. Thus, one aim in discussing the periodic errors in Ptolemy's lunar model will be to distinguish between the errors due to his parameters and those due to the model which he adopts.

A further difficulty arises in choosing the proper quantities in the modern lunar theory with which to compare Ptolemy's parameters. It is possible to express the general lunar inequality according to Ptolemy's model as a trigonometric series and to compare the coefficient of each term with the coefficient of the term with the same argument in modern theory. Such a comparison may be found in Biot 1848, 703, and also in Kempf 1878, 35, where the errors in Ptolemy's parameters appear as errors in the coefficients of the principal terms of the two largest lunar inequalities [cf. also Tannery 1893, 213].

The principal shortcoming of such a comparison is, on the one hand, that it does not accurately reflect the circumstances from which Ptolemy derived his parameters and, on the other, that the quantities which are compared with the modern coefficients differ from the quantities Ptolemy actually determined. This is particularly true of the comparison of the 'principal elliptic term' with the corresponding coefficient of $\sin \bar{a}$ in the expansion of the general inequality according to Ptolemy's model, since the concept of a general term equivalent to the mean equation of center for all elongations plays no role in Ptolemy's theory. Instead, Ptolemy first determines the Moon's equation of center at syzygy and then introduces a further inequality based on observations at quadrature and octant, which varies with the Moon's elongation. Thus, an evaluation of Ptolemy's principal lunar inequality should be made for syzygy rather than for all elongations; whereas an evaluation of his 'lunar inequality depending on the Sun' [*Alm.* v 3: Toomer, 264] should be made for quadrature and octant.

At these synodic configurations many of the higher harmonics in modern lunar theory take on the arguments of the principal terms and, thus, should be included in the comparison. These terms are ignored by Kempf [1878, 31], who compares Ptolemy's lunar equation at these elongations with the principal terms of Damoiseau's lunar theory [1827]. Although the neglected terms are small, they do affect the results of the comparisons and, in particular, the coefficients of the omitted terms at these elongations.[1] Consequently, it seems more convenient to make a new comparison of the

[1] In general, the contribution of such harmonics to a given coefficient differs for different synodic configurations. Thus, for example, the coefficient of the annual equation, whose principal term is $-11'10''$, takes on the values $14'20''$, $8'1''$, and $11'10''$ at syzygy, quadrature, and octant respectively, due to the inclusion of terms with arguments $a_s \pm 2nD$, which appear as terms with argument a_s at these synodic configurations.

terms in Ptolemy's lunar equation with their modern equivalents taken from Brown [1919, 8] than to attempt to revise the details of Kempf's analysis.

Detailed descriptions of Ptolemy's lunar model may be found in Delambre 1817, ii 142–239; Biot 1843, 694–703; Kempf 1878, 1–37; Tannery 1893, 211; and O. Neugebauer 1957, 193–198 and 1975, i 53–144. In order to introduce the terminology and symbols used in the following discussion, let us here review the principal features of this model.

In Figure 4.1, O is the center of the Earth and OS points in the direction of the mean Sun. Therefore, the relationships shown in Figure 4.1 occur in a reference system which rotates with direct motion relative to the equinoxes with a velocity equal to the mean motion of the Sun. The center of the Moon's epicycle is at C, where $\angle COS$ is equal to the mean elongation of the Moon from the Sun, $\bar{D}$. The distance of the center of the Moon's epicycle from O is determined by letting a point F, at a distance e_1 from O, revolve in the opposite direction to OC in such a way that $\angle SOF = -\bar{D}$ and, thus, $\angle FOC = 2\bar{D}$. The distance FC is taken to be constant and equal to $1 - e_1$. Thus, the distance, $OC = R$, may be found from the relationship,

$$R^2 - 2Re_1 \cos 2\bar{D} - (1 - 2e_1) = 0.$$

At syzygy ($\bar{D} = 0°, 180°$), R becomes 1;0, its maximum; but at quadrature it reaches its minimum, $1 - 2e_1$.

The Moon at M moves on an epicycle of radius r in the direction shown. The Moon's mean anomaly is measured from the line NCH, where N is the point on the extension of FO which is at a distance e_1 from O and in the opposite direction from F. At syzygy and quadrature, NCH coincides with the line OC, so that the mean anomaly ($\bar{a}$) is measured from the apogee of the Moon's epicycle (A) as seen from O. At other elongations, however, the *prosneusis* (k) must be added (algebraically) to the mean anomaly in computing the lunar equation.

The lunar equation (g) is the difference between the Moon's true elongation from the mean Sun and its mean elongation. For any given value of $\bar{D}$, the equation may be represented by motion on an epicycle of radius r and distance R from 0. This is equivalent to eccentric motion on a circle having a radius of 1;0 and eccentricity $e = r/R$. In discussing the errors in Ptolemy's lunar equation at syzygy, quadrature, and octant, I shall use the term 'eccentricity' (in reference to Ptolemy's model) as synonymous with the 'radius of the lunar epicycle at unit distance'. Where it is desirable to distinguish between parameters in Ptolemy's theory and their modern equivalents, I have used primes to denote Ptolemy's parameters.

In discussing the errors characteristic of different types of lunar observations in chapter 3, I used the terms 'probable error' and 'average error'

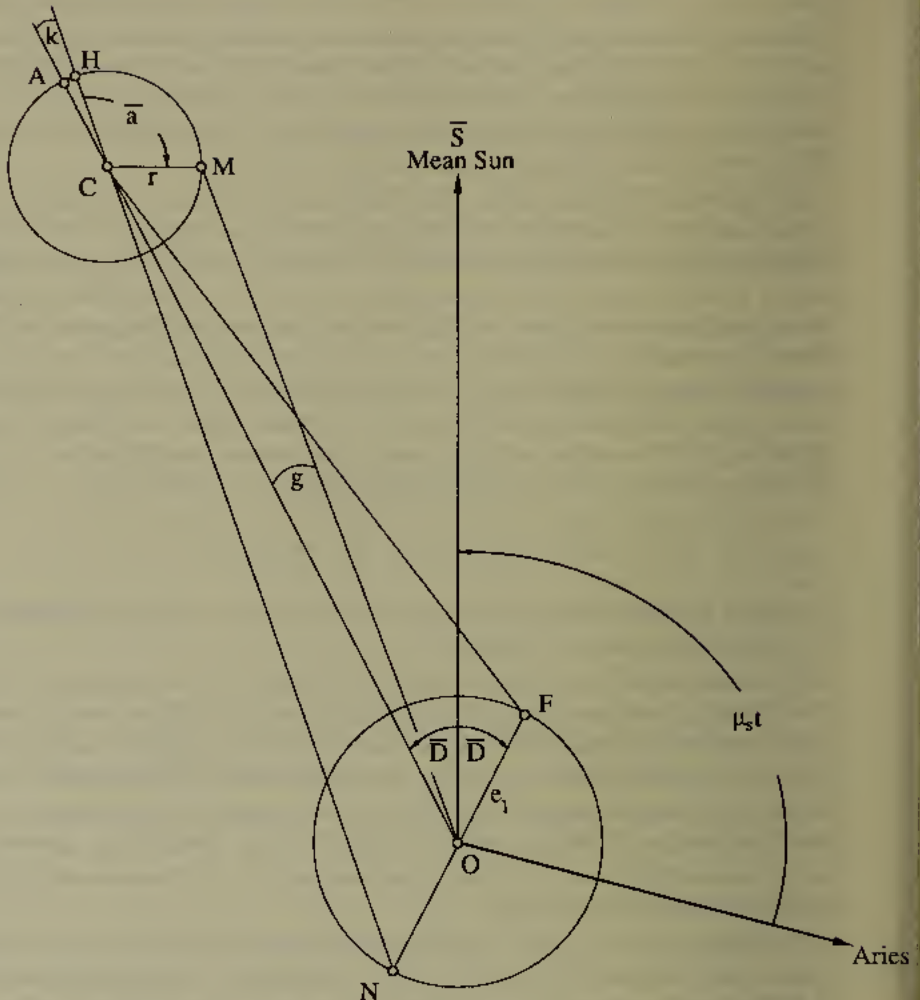

Figure 4.1. Ptolemy's Lunar Model

interchangeably. In analyzing the observational errors, I have disregarded the few errors which seemed too large to have been caused solely by errors of measurement, and take 67.4% of the standard deviation of the rest as the probable error of the group of observations being considered.

In what follows, I shall assume that these characteristic errors of observation obey the usual rule for combining independent errors, namely, that the probable error ($\bar{\Delta}$) resulting from the combination of several independent errors (Δ_i) is

$$\bar{\Delta} = \sqrt{\sum_i \Delta_i^2}. \tag{1}$$

To compare the errors in Ptolemy's lunar model with the errors in his observational data [cf. chapter 3], I have determined the probable error which arises from each term in the trigonometric series expressing the error in Ptolemy's lunar inequality. Assuming that all values of a term's argument are equally likely, the probable error (i.e., the median error disregarding sign) of a term, $c_i \sin A_1$, is[2]

$$\Delta_i = c_i \sin 45^\circ = \frac{1}{\sqrt{2}} c_i; \tag{2}$$

whereas the composite probable error for the whole expression is[3]

$$\bar{\Delta} = \frac{1}{\sqrt{2}} \sqrt{\sum_i c_i^2}. \tag{3}$$

---

[2] In a sinusoidal distribution of errors, the probable error is the same as the standard deviation s found from

$$s^2 = \frac{1}{2\pi} \int_0^{2\pi} c_i{}^2 \sin^2 A \cdot \mathrm{d}A_i = {}^1\!/_2 c_i{}^2.$$

It seems preferable, then, to use the probable error found in (2) instead of the (smaller) average error,

$$\bar{\Delta}' = \tfrac{2}{\pi} c_i.$$

[3] Equation (3) holds for expressions which include terms of the form $c_i \sin(nA_i)$ and $c_i \cos(nA_i)$, as well as terms, $c_i \sin(nA_i)$, where the arguments are independent. This follows from the fact that

$$\tfrac{1}{2\pi} \int_0^{2\pi} (a^2 \sin^2 A_i + b^2 \cos^2 A_i) \mathrm{d}A_i = {}^1\!/_2 (a^2 + b^2)$$

and

$$\tfrac{1}{2\pi} \int_0^{2\pi} (a^2 \sin^2 A_i + b^2 \sin^2 2A_i + c^2 \sin^2 3A_i \ldots) \mathrm{d}A_i = {}^1\!/_2 (a^2 + b^2 + c^2) \ldots$$

Errors in the mean motions of Ptolemy's lunar arguments

Four lunar arguments are tabulated in the *Almagest*. These are the Moon's mean motion in longitude ($\bar{L}'$), anomaly ($\bar{a}'$), argument of latitude ($\bar{F}'$), and elongation ($\bar{D}'$). Only three of these are independently determined, since the mean motion in longitude is derived from the mean motion in elongation and the mean motion of the Sun ($\bar{L}'_s$).

The two arguments, $\bar{L}'_m$ and $\bar{D}'$, have counterparts in the fundamental elements of modern lunar theory and, thus, are directly comparable with the mean motions and the values of these arguments. Ptolemy's arguments of anomaly ($\bar{a}'$) and latitude ($\bar{F}'$) are, however, slightly different from those used in modern theory. Ptolemy counts the Moon's anomaly from the apogee of its epicycle, which is equivalent to the apogee of its orbit, whereas today the anomaly is counted from the Moon's perigee. Thus, if $\bar{L}'_m$ is the Moon's modern mean longitude, and P the longitude of its perigee, the angle equivalent to Ptolemy's mean anomaly is

$$\bar{a} = L_m - P \pm 180^\circ_. \qquad (4)$$

Similarly, Ptolemy's argument of latitude is measured from the northernmost point of the Moon's orbit, instead of from the ascending node as is the modern practice. Accordingly, if N is the longitude of the ascending node, the angle equivalent to Ptolemy's argument of latitude is found from the fundamental elements used in modern lunar theory by

$$\bar{F} = \bar{L}_m - N + 90^\circ_. \qquad (5)$$

The mean motions of Ptolemy's arguments are, of course, equivalent to the mean motions of their modern counterparts, since the phase-angle disappears on differentiating.

Ptolemy derives $\bar{D}'$, $\bar{a}'$, and $\bar{F}'$ from observations of eclipses in such a way that these arguments are unaffected by the error in his mean motion of the Sun. From (4) and (5), however, it is evident that Ptolemy's positions and mean motions of the Moon's apogee and node are affected by the same error in the Sun's mean longitude as is the mean longitude of the Moon. Consequently, this error affects only the system of reference in which the Moon moves and not the arguments from which the lunar inequalities are derived.

Table 4.1 gives the mean (Julian) centennial motions of the fundamental elements and arguments. Those shown in part I are from modern theory [Nautical Almanac Office 1961, 98] for epoch 1900.0, corrected in accordance with the elements derived in appendix 1. In part II, the same elements and the principal arguments derived from them are reduced to epoch

I. Modern Elements 1900.0	
$\mu(\bar{L}_m)$	$+1336^r\ 307°\ 53'\ 36.89'' + 21.52''T + 0.0204''T^2$
$\mu(\bar{L}_s)$	$+100^r\ 0°\ 46'\ 10.79'' + 4.18''T$
$\mu(\bar{D})$	$+1236^r\ 307°\ 7'\ 26.10'' + 17.34''T + 0.0204''T^2$
$\mu(P)$	$+11^r\ 109°\ 2'\ 2.52'' - 74.34''T - 0.135\ ''T^2$
$\mu(N)$	$-5^r\ 134°\ 8'\ 31.23'' + 14.96''T - 0.024\ ''T^2$
II. Modern Elements AD 0.0	
$\mu(\bar{L}_m)$	$+1336^r\ 307°\ 46'\ 55.38'' + 20.74''T + 0.0204''T^2$
$\mu(\bar{L}_s)$	$+100^r\ 0°\ 44'\ 51.37'' + 4.18''T$
$\mu(\bar{D})$	$+1236^r\ 307°\ 2'\ 4.02'' + 16.56''T + 0.0204''T^2$
$\mu(P)$	$+11^r\ 109°\ 24'\ 46.22'' - 69.21''T + 0.135\ ''T^2$
$\mu(N)$	$-5^r\ 134°\ 13'\ 24.13'' + 15.87''T - 0.024\ ''T^2$
$\mu(\bar{a})$	$+1325^r\ 198°\ 22'\ 9.16'' + 89.95''T - 0.1554''T^2$
$\mu(\bar{F})$	$+1342^r\ 82°\ 0'\ 19.51'' + 4.87''T + 0.0444''T^2$
III. Ptolemy's Mean Motions	
$\mu(\bar{L}'_m)$	$+1336^r\ 307°\ 21'\ 37.47''$
$\mu(\bar{L}'_s)$	$+100^r\ 0°\ 19'\ 42.76''$
$\mu(\bar{D}')$	$+1236^r\ 307°\ 1'\ 54.71''$
$\mu(\bar{a}')$	$+1325^r\ 198°\ 29'\ 56.27''$
$\mu(\bar{F}')$	$+1342^r\ 82°\ 2'\ 42.82''$
IV. Errors in Ptolemy's Mean Motions	
$\Delta\mu(\bar{L}'_m)$	$+0°\ 25'\ 17.91'' + 20.74''T + 0.0204''T^2$
$\Delta\mu(\bar{L}'_s)$	$+0°\ 25'\ 8.61'' + 4.18''T$
$\Delta\mu(\bar{D}')$	$+0°\ 0'\ 9.31'' + 16.56''T + 0.0204''T^2$
$\Delta\mu(\bar{a}')$	$+0°\ 7'\ 47.11'' + 89.95''T - 0.1554''T^2$
$\Delta\mu(\bar{F}')$	$+0°\ 2'\ 23.31'' + 4.87''T + 0.0444''T^2$

Table 4.1. Expressions for the Centennial Mean Motions of the Fundamental Lunar Arguments and for the Errors in Ptolemy's Mean Motions

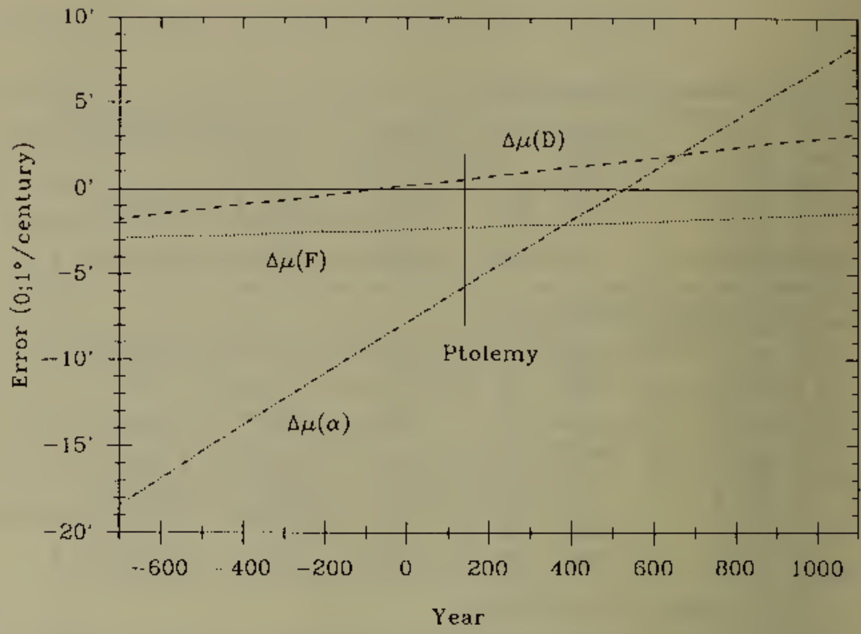

Figure 4.2. Errors in the Centennial Mean Motions of Ptolemy's Lunar Arguments

AD 0.0. In part III are Ptolemy's mean motions for the corresponding arguments; and in part IV, the corrections which must be applied to Ptolemy's motions to reduce them to motions from modern theory [cf. Figure 4.2].

The improvement in the accuracy of the mean motions of each of the principal arguments ($\bar{D}'$, $\bar{a}'$, and $\bar{F}'$) over that of the Sun's longitude is striking. Apart from the term due to the Moon's acceleration, about which Ptolemy of course knew nothing, the difference between his mean motion in elongation and that derived from modern elements nearly vanishes throughout the whole period for which he reports observations.

For −293, the midpoint between the dates of the two eclipses which Ptolemy uses to correct his provisional (Babylonian)[4] mean motions in elongations and anomaly [*Alm.* iv 6], the error in his mean motion in elongation is only −0;0.65° *per* century. In the 8.5 centuries between these two eclipses, this amounts to an error of −0;5.6° in the observed motion in elon-

[4] Cf. Kugler 1900, 6–8; Aaboe 1955 and 1974; O. Neugebauer 1956 and 1975, i, 309–315; Toomer 1980, for discussions of the Babylonian origin of the provisional mean motions of the lunar arguments which Ptolemy takes from Hipparchus.

gation, or a total error in the measured interval between the two eclipses of -12 minutes of time. Since the error in Ptolemy's value for the difference in longitude between Alexandria and Babylon slightly improves the agreement between his mean motion and the modern value, the total error in the interval between the two eclipses would have been $\approx \pm 20$ minutes, if the eclipses had been reduced with the proper longitude-difference.

In chapter 3, I showed that the probable error in the time of a single eclipse-midpoint used by Ptolemy was ± 18 minutes. Thus, we would expect an average error of ± 25 minutes in measurements of the time between two eclipses, corresponding to an error in the Moon's elongation of $\pm 0;12.8°$.

Although the error in Ptolemy's solar eccentricity is effectively reduced at syzygy by the Moon's annual equation [see 144, below], the additional probable error in each determination from this source and neglected terms is $\pm 0;10.3°$ or, for two independent determinations, $\pm 0;14.6°$. The total probable error of an observed interval in elongation should, therefore, be $\pm 0;19.4°$. This is more than four times the error of the progress in elongation which Ptolemy obtains from these two eclipses, and roughly twice the error in the progress which he would have found had he used the correct longitude-difference between Babylon and Alexandria. Thus, his value for the Moon's mean motion in elongation (or, more accurately, of the Babylonian System B value, which Ptolemy accepts) is considerably more accurate than the value we would expect from a single determination based on a random pair of eclipses.

Ptolemy's mean motion in anomaly agrees less closely with its modern equivalent than his value for the mean motion in elongation. For -293, the effective epoch of Ptolemy's determination, the error is $-0;12.2°$ per century, which corresponds to an error of $-1;44°$ in the Moon's progress in anomaly in 8.54 centuries.

To compare this with the error we would expect from the errors of the observations and the limitations of Ptolemy's model, I assumed that the Moon's motion at syzygy can be described by a simple epicyclic model in which the radius of the epicycle is e and that of the deferent is 1;0. I further assumed that e and the mean longitude ($\bar{L}$) are accurately known, and that the equation (g), which is equal to the difference between the mean and the true longitude, is directly determined from observation.

Since g is small,

$$\tan g \cong g = \frac{-e\sin\bar{a}}{1 + e\cos\bar{a}}, \tag{6}$$

so that

$$\frac{\Delta\bar{a}}{\Delta g} \cong \frac{-(1 + e\cos\bar{a})^2}{e(e + \cos\bar{a})}, \quad e < 1. \tag{7}$$

Also, $\Delta\bar{a}/\Delta g$ is a minimum when $\bar{a} = 180°$, at which point

$$\frac{\Delta\bar{a}}{\Delta g} = \frac{1-e}{e}. \tag{8}$$

Using Ptolemy's value for e, 0;5,15, the *minimum* error ($\Delta\bar{a}$) which would arise from an error Δg in the observed equation is

$$\Delta\bar{a} \geq 10.4\Delta g. \tag{9}$$

Since $\Delta\bar{a}/\Delta g$ becomes infinite at $\bar{a} = \arccos(-e)$, the probable error in $\bar{a}$ caused by a given error in g will be considerably larger than that shown in (9). The average error is difficult to evaluate, however, and the minimum will suffice for our purposes.

Following the same procedure as we used to obtain the probable error in a determination of the mean motion in elongation, we find the probable error in a single observed equation to be ±0;13.7°. From (9) such an error would produce a probable error greater than ±2;22° in the mean anomaly obtained from a single determination. Thus, the probable error in the progress in anomaly between two observations is greater than ±3;21°, corresponding to an error of ±0;24° *per* century. In contrast, the error in Ptolemy's mean motion in anomaly is just half this amount; and his correction to his provisional mean motion of −0;1,59.5° *per* century slightly improves his value for this parameter.

As noted at the beginning of this chapter, the assumptions (except for that of epicyclic motion) from which I derived the minimum error in $\bar{a}$ as a function of that in g do not exactly correspond to Ptolemy's actual procedure, since he determines $\bar{L}$, $\bar{a}$, and e simultaneously from a triad of eclipses. Since, however, the estimates derived above are for the circumstances most favorable for determining the anomaly, Ptolemy's procedure should lead to a substantially larger probable error. In any case, it is clear that Ptolemy's mean motion in anomaly is considerably better than would be expected from a single determination, in view of the errors of the observations available to him and the imperfections of his solar model.

To determine the correction to his provisional mean motion in the argument of latitude, Ptolemy selects two eclipses which satisfy the conditions that:

(a) they be separated by the greatest possible interval of time,

(b) they be of the same magnitude, and

(c) they occur when the Moon is at the same distance from the Earth.

As Ptolemy points out, the Moon will be at the same distance from the same node in the same direction during two eclipses satisfying these conditions.

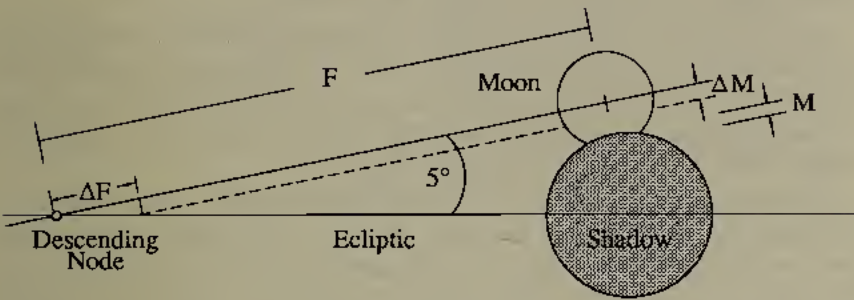

Figure 4.3

The two eclipses Ptolemy chose for this correction occurred in −490 Apr 25 and +125 Apr 5, so that the effective epoch for this determination is −183. For this date, the error in Ptolemy's mean motion in argument of latitude is −0;2.54° *per* century, which corresponds to an error of −0;15.6° in the progress in argument of latitude during the intervening 6.15 centuries.

Figure 4.3 shows the configuration of the Moon, the Earth's shadow, and the descending node at the time of these two eclipses. The error in $\bar{F}$ caused by an error (ΔM) in the recorded magnitude will be very nearly

$$\Delta\bar{F} = \Delta F \cong \frac{0;2.5\Delta M}{\tan 5^\circ}(^\circ), \tag{10}$$

since both ΔF and ΔM are small.

Since a systematic error in the estimate of the magnitudes will have the opposite effect of the error in F, depending upon whether the Moon has passed the node or not, we may combine the systematic and random errors found for the Babylonian eclipse-magnitudes. The probable errors in the estimates of the magnitudes then become [cf. Table 3.5]

$$\Delta M = \pm 1 \text{ digit, for Babylonian eclipses, and} \tag{11}$$

$$= \pm 0.25 \text{ digit for Alexandrian eclipses.} \tag{12}$$

Consequently, from the error in the eclipse-magnitudes alone, we should expect an error in $\bar{F}$ from two eclipses of ±0;29.6°. To this must be added the probable error, ±0;19.4°, due to the uncertainty in the combined times of the eclipses and to the error in Ptolemy's solar equation as reduced by his omission of the Moon's annual equation. Thus, the total probable error in the mean progress in argument of latitude determined from a pair of eclipses should be $\pm 0;35°$, corresponding to an error in the mean motion in the argument of latitude of $\pm 0;5.8°$ *per* century.

Again, Ptolemy's actual error is less than half the probable error deduced from the errors of the recorded observations. In this instance, however, his correction to his provisional motion in argument of latitude, $\pm 0;1.5°$ *per* century, worsens the agreement with the modern value, and accounts for roughly 60% of the total error in this parameter.

In conclusion, we have seen that, although Ptolemy's mean motion of the Moon in longitude and the mean motions of the lunar apse and the node are all affected by the error in his mean (tropical) motion of the Sun, the mean motions of the lunar arguments which are determined directly from lunar eclipses are not. Furthermore, the error in the mean motion of each of these arguments is significantly less than what we would expect from the average errors found in Ptolemy's data and the errors introduced into his determinations by failing to take account of the annual equation.

This may, of course, merely reflect the excellence of Ptolemy's provisional mean motions, since his corrections to them are very small (he makes no correction to the mean motion in elongation). Indeed, Delambre [1817, i xxvii] has suggested that Ptolemy's corrections were introduced merely to increase his readers' confidence in his determinations. This unsupported assumption is doubtful, however, since the mean motion which Ptolemy does not correct is the most accurate of the three, and since he also would have had no reason not to correct the provisional mean motions had he found significant discrepancies.

Indeed, what is curious is that Ptolemy did not obtain larger corrections than those which he applied, not because his provisional mean motions required them, but because from the errors of the observations we would expect significant deviations in individual determinations. In general, the procedures which Ptolemy describes should have led to mean motions less accurate rather than more accurate than those with which he started. Consequently, it is difficult to avoid the conclusion that either Ptolemy was very fortunate in his choice of eclipses or he had better reasons than he states for adopting the mean motions which he did.

Errors in the mean arguments

Although it is possible to attribute the excellence of the mean motions of Ptolemy's lunar arguments to the accuracy of his provisional mean motions, it is much more difficult to explain the consistent accuracy of Ptolemy's mean arguments themselves. Table 4.2a shows the corrections necessary

$\Delta(\bar{L}'_m)$	$+0;29,50° + 0;25,17.91°T + 10.37''T^2 + 0.0068''T^3$
$\Delta(\bar{D}')$	$-0;\ 0,16° + 0;\ 0,\ 9.31°T + 8.28''T^2 + 0.0068''T^3$
$\Delta(\bar{a}')$	$-0;\ 1,\ 8° - 0;\ 7,41.11°T + 45.00''T^2 + 0.0518''T^3$
$\Delta(\bar{F}')$	$+0;\ 5,\ 1° - 0;\ 2,23.31°T + 2.44''T^2 + 0.0148''T^3$
$\Delta(\bar{L}'_s)$	$+0;30,\ 6° + 0;26,\ 8.61°T + 2.09''T^2$

Table 4.2a. Errors in Ptolemy's Lunar Arguments, AD 0.0

Year	$\Delta\bar{L}'_M$	$\Delta\bar{D}'$	$\Delta\bar{a}'$	$\Delta\bar{F}'$	$\Delta\bar{L}'_s$
−700	−2;18,49°	+0; 5,22°	+1;29, 7°	+0;23,38°	−2;24,11°
−600	−1;55,18	+0; 3,45	+1;11,48	+0;20,46	−1;59,30
−500	−1;32,20	+0; 2,24	+0;55,56	+0;17,57	−1;34,44
−400	−1; 8,33	+0; 1,19	+0;41,33	+0;15,12	−1; 9,54
−300	−0;44,30	−0; 0,30	+0;28,39	+0;12,53	−0;45, 0
−200	−0;20, 4	−0; 0, 2	+0;17,14	+0; 9,51	−0;20, 2
−100	+0; 4,44	−0; 0,17	+0; 7,18	+0; 7,27	+0; 5, 1
0	+0;29,50	−0; 0,16	−0; 1, 8	+0; 5, 1	+0;30, 6
+100	+0;55,19	+0; 0, 2	−0; 8, 4	+0; 2,40	+0;55,17
+200	+1;21, 8	+0; 0,36	−0;13,30	+0; 0,24	+1;20,32
+500	+2;40,39	+0; 3,58	−0;20,44	−0; 5,54	+2;36,41
+1000	+5; 0,13	+0;15,12	−0; 2,11	−0;12,33	+4;45, 1
+1500	+7;27,42	+0;33,37	+0;55;11	−0;20,50	+6;54, 5

Table 4.2b. Errors in Ptolemy's Lunar Arguments: Tabular Values

to reduce Ptolemy's mean arguments to those computed from Brown (corrected in accordance with the adopted accelerations of the mean longitudes of the Sun and Moon), and Table 4.2b records the values of these corrections for centennial dates from −700 to +1500.

The errors shown in Table 4.2b are plotted in Figure 4.4. Evidently, the error in each of the three principal lunar arguments is much smaller in Ptolemy's time than at the time of the Babylonian eclipse-observations which he used, together with his own observations, to determine the necessary corrections to the mean motions. This is more than a little curious, since nothing in Ptolemy's procedure for determining the mean elongation and anomaly should lead to a more favorable result at his own time than at the earlier epoch. Indeed, as chapter 3 shows, the three eclipses Ptolemy observed are in no better agreement among themselves than are the three Babylonian eclipses (ca. −720) which he uses in this determination.

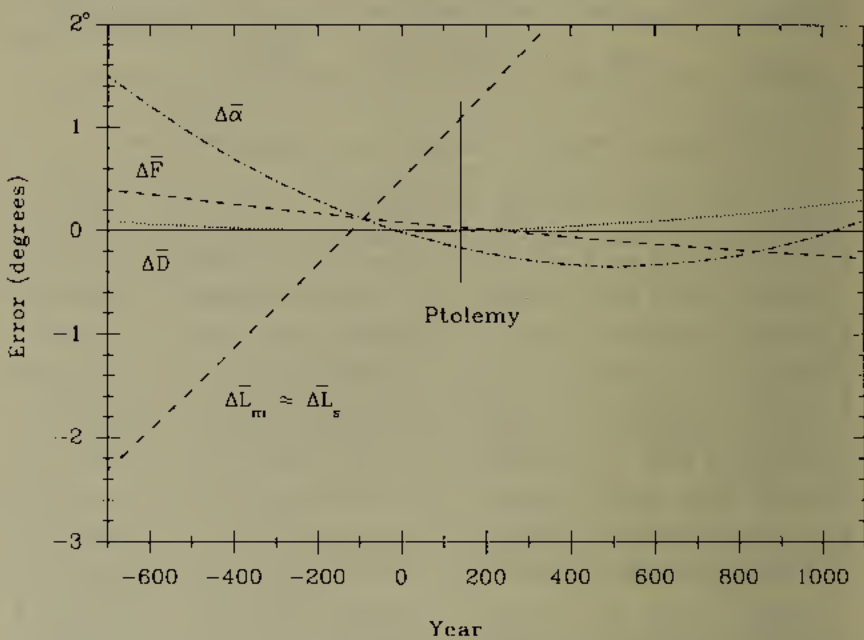

Figure 4.4. Errors in Ptolemy's Mean Lunar Arguments

Even more striking, moreover, is the small error at Ptolemy's time in his mean argument of latitude. In contrast to his procedure for determining the epoch of the mean elongation and anomaly, he does not use the same eclipses that he used to correct its mean motion in order to determine the epoch of his mean argument of latitude. Instead, to avoid making assumptions about the relative sizes of the diameter of the Moon and the Earth's shadow, he uses two other Babylonian eclipses which occurred at −719 Mar 8 (also used in determining the mean elongation and anomaly) and −501 Nov 19. These eclipses were required to satisfy the same conditions as in the determination of the correction to the mean motion, except that they had to occur at opposite nodes rather than at the same node.[5]

Apart from this difference—which enables Ptolemy to solve a simple linear equation in order to find the distance of the Moon from the node—the procedure (and, thus, the probable error of the determination) is similar to that used to correct the mean motion in argument of latitude. Since none of the eclipses used in the two determinations are the same, however, the epoch established for this parameter at Thoth 1 of Nabonassar 1 [cf. 54n9, above], is not directly connected to any observations in Ptolemy's time. It is, then, all the more remarkable that the argument of latitude—like the mean elongation and anomaly—is substantially closer to its modern equivalent at Ptolemy's time than at the time of the earlier observations which Ptolemy used to determine it. Also remarkable is the fact that the errors in Ptolemy's lunar arguments are so small, especially at Ptolemy's epoch. Comparing the probable error in each argument if determined from a single observation with the errors in arguments in +135 and −700, we find:

Argument	Probable Error from A Single Observation	Actual Error +135	−700
$\bar{D}'$	± 0;13.5°	+0; 0.2°	+0; 5.4°
$\bar{a}'$	(>) ± 2;20	−0;10	+1;29
$\bar{F}'$	± 0;25	+0; 2	+0;24

In each case the errors for both +135 and −700 are less than the probable errors from a determination based on a single observation. For Ptolemy's time, moreover, the errors in his arguments are all less than the probable error by more than a factor of 10. Thus, the error in Ptolemy's mean elongation in +135 is less than $^1/_{60}$ of what we would expect from a single observation,[6] while the errors in the mean anomaly and argument of latitude are both less than $^1/_{14}$ of their expected errors.

[5] Ptolemy could as easily have used eclipses occurring on opposite sides of the same node, but not eclipses on either the same side of the same node (such as he used to correct the mean motion in argument of latitude) or on opposite sides of different nodes. For a thorough discussion of the methods of Ptolemy and Hipparchus for determining the epoch of the Moon's argument of latitude, cf. Pedersen 1974.

[6] This is somewhat less than the probable uncertainty of the modern value; but even allowing for this uncertainty, Ptolemy's error should be less than $^1/_{20}$ of the probable error for a single determination.

The smallness of these errors, particularly for Ptolemy's time, cannot be explained merely by the excellence of his mean motions of these arguments. Even had he chosen his eclipses to demonstrate values for the mean motions of his arguments similar to the values of his provisional mean motions, we would expect to find errors in the mean arguments at the times of both sets of eclipses similar to the probable errors shown above.

These comparisons show clearly that, apart from Ptolemy's erroneous value for the mean motion of the Sun, the error introduced into his lunar model by the arguments and their mean motion is very small over the period -750 to $+150$, and negligible at his own time (the maximum error due to the error in anomaly is less than 0;1°). Although this speaks well for Ptolemy's lunar model, it unfortunately raises more questions than it answers. The principal question, of course, is how Ptolemy obtained these values, which in *all six instances* are significantly closer to the modern values than one would expect in view of the average errors of the observation which he reports and those introduced by his reductions of these observations. Not only are all of Ptolemy's mean motions accurate to well within the observational error over the entire period for which observations were available, but the values of these arguments in his own epoch are exceptionally accurate. Since Ptolemy obtains his mean motions from pairs of observations, we would expect some compensating errors so that his mean motions ought to be in better agreement with their modern equivalents than are the actual values of the arguments themselves. We find just the opposite, however, since at Ptolemy's time the values of each of his lunar arguments are in even better accord with the modern values than are his mean motions.

Excellent agreement between the Ptolemaic and modern values for one or two of these parameters would not be remarkable, since such agreement could be accidental. But the probability of accidentally achieving much better values for all six parameters seems too small to support the assumption that this accuracy was wholly fortuitous. A more likely explanation is that these parameters represent the result of a larger number of determinations. Such procedure would reduce the probable error of a single determination quite sharply, especially since the errors introduced by the reduction to the mean arguments would tend to cancel each other.

I shall return to the question of whether Ptolemy might plausibly have followed such a procedure, after discussing the periodic errors in his lunar model. Suffice it to note here that, in contrast to his model for the Sun, the mean motions and values of his lunar arguments are in excellent agreement with the modern values for his own time and that, in general, they remain so until well into the medieval period.

Periodic errors in Ptolemy's lunar model

The symbols and terms used in what follows as well as the characteristics of Ptolemy's lunar model at different elongations are described at the beginning of this chapter [cf. Figure 4.1]. In each comparison, I have used the value of the parameters which Ptolemy adopts in his tables.[7] As above, subscripted 'm' and 's' denote quantities pertaining to the Moon and Sun respectively.

Errors at syzygy. Ptolemy's lunar model at syzygy is formally equivalent to simple eccentric motion on a circle of radius 1;0 and $r = e' = 0;5,15$. For eccentric motion, the equation may be represented by[8]

$$g' = e' \sin \bar{a} + \frac{1}{2}e'^2 \sin 2\bar{a} - \frac{1}{3}e'^3 \sin 3\bar{a} + \frac{1}{4}e'^4 \sin 4\bar{a} \ldots \tag{13}$$

On substituting for $e' = 0;5,15 = 0.0875$ and converting from radians to degrees, we obtain the coefficients shown in Table 4.3, which also presents the corresponding coefficients from Brown 1919, 8.

The differences in the last column form the coefficients of a new series of sine-terms which describes the error in Ptolemy's lunar equation. Only the error in the coefficient of $\sin \bar{a}$ is due primarily to the error in the radius of the epicycle (e'): the errors of the coefficients on the higher harmonics arise for the most part from the assumption of eccentric (i.e., epicyclic) rather than elliptic motion. If in (13) we replace e' by $2e$ (where e is the eccentricity of the Moon's actual orbit), the error in Ptolemy's equation becomes

$$\Delta' g = +4.2'' \sin \bar{a} - 4'44.4'' \sin 2\bar{a} + 26.9'' \sin 3\bar{a} - 2.3'' \sin 4\bar{a}. \tag{14}$$

Thus, the error shown in (14) can be attributed to the limitations of Ptolemy's model rather than to the inaccuracy of his value for the lunar eccentricity.

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[7] These are not always the precise values which Ptolemy finds in computing his parameters, nor are they the values which result from an accurate recomputation of the parameters from his data. For example, he obtains 0;5,13 and 0;5,14 for the radius of the lunar epicycle at syzygy from the three Babylonian eclipses (ca. −720) which he uses and from the three eclipses which he observed. Furthermore, there is an error in his computed longitude of the Sun at the time of the first Babylonian eclipse (−720), the correction of which would yield yet another value for the radius of the lunar epicycle. For reasons of consistency, and also because of the large probable errors in individual determinations of such parameters, it seemed preferable to use the values Ptolemy adopted in constructing his tables.

[8] See appendix 3, for proof.

n	I Ptolemy	II Brown	III Δ(II–I)
1	$-5°\ 0'\ 48.2''$	$-5°\ 3'\ 15.1''$	$-2'\ 26.9''$
2	$+13'\ 9.4''$	$+8'\ 58.1''$	$-4'\ 11.3''$
3	$-46.1''$	$-22.5''$	$+23.6''$
4	$+3.1''$	$+1.0''$	$-2.1''$

Table 4.3. Coefficients of $\sin(n\bar{a})$ in Lunar Equation for $\bar{D} = 0°, 180°$

Subtracting the coefficients of each term in (14) from the errors in the corresponding terms given in Table 4.3, we obtain for the error in Ptolemy's equation (which is due to the error in his eccentricity),

$$\Delta_e g = -2'31.9'' \sin \bar{a} + 33.1'' \sin 2\bar{a} - 3.3'' \sin 3\bar{a} + 0.1'' \sin 4\bar{a} \ldots \qquad (15)$$

Thus, the errors in the coefficients of the higher harmonics of $\bar{a}$ are due almost entirely to the limitations of Ptolemy's model rather than to the error in his eccentricity. Since Ptolemy could not have significantly reduced these errors without changing his model, the optimal value for the radius of the lunar epicycle is that which would make the coefficient of $\sin \bar{a}$ equal to $5;3,15°$, or

$$e'_{\text{optimal}} = 0;5,17,35 \ (R = 1). \qquad (16)$$

For this value of e', the equation at $\bar{a} = \pm 90°$ would be $\mp 5;2,27°$, and the maximum equation would be $\mp 5;3,39.4°$.

In accordance with the procedure described above [cf. equation (3)], the coefficients shown in column III of Table 4.3 result in a probable error in a single computed lunar equation of

$$\bar{\Delta} g = \pm 0;3.45°. \qquad (17)$$

The maximum error, which occurs near $\bar{a} = \pm 57°$, is $\mp 0;5.9°$.

In addition to the errors discussed above, there are a number of inequalities at syzygy of which Ptolemy is unaware, and which thus contribute to the errors of his computed positions and also to his determinations of the mean elongation or argument of latitude from observed positions. The inequalities with coefficients greater than $0;1°$ are:

(a) Annual equation	$+14'19.8'' \sin \bar{a}_s - 15.9'' \sin 2\bar{a}_s$
(b) Reduction to ecliptic	$-7'52.6'' \sin 2\bar{F}$
(c) Miscellaneous	$-5'33.2'' \sin(\bar{a}_m + \bar{a}_s)$
(d) Miscellaneous	$+3'11.7'' \sin(\bar{a}_m + \bar{a}_s)$

Since the arguments of each of these terms can take on any values independently of the others, the probable error of a computed longitude due to the omission of these terms is, in accordance with equation (3),

$$\bar{\Delta}\lambda = \pm 0;12.5^\circ \tag{18}$$

When the longitude of the Moon is determined with reference to the Sun, as is the case in eclipses and direct measurements of the Moon's elongation, the mean elongation resulting from the application of Ptolemy's equation will be affected not only by the errors in this equation, including the contribution from omitted terms, but also by the error in Ptolemy's solar equation, which alters the computed place of the Sun. As shown previously [cf. 46, above] the latter error is

$$\Delta g'_s = +23'24'' \sin \bar{a}'_s - 1'9'' \sin 2\bar{a}'_s + 9'12'' \cos \bar{a}'_s.$$

Since for an observed elongation

$$D = \bar{L}_m + g_s - (\bar{L}_m + g_s), \tag{19}$$

where g_m and g_s are the actual equations of the Moon and Sun, it follows that

$$\bar{D} = D_{obs} - (g'_m - g'_s) - (\Delta g'_m - \Delta g'_s), \tag{20}$$

where g'_m and g'_s are Ptolemy's equations for the Moon and Sun and where $\Delta g'_m$ and $\Delta g'_s$ are the errors in Ptolemy's equations. Accordingly, the error in the mean elongation of the Moon determined from an accurate observation will be

$$\Delta D = \Delta g'_s - \Delta g'_m. \tag{21}$$

Since the error due to omitting the annual equation has the same sign as the principal term in the error of Ptolemy's solar equation, the two errors will tend to cancel each other. Combining these errors, we find for the error depending only on the solar anomaly, which I shall call the 'apparent annual equation',

$$\Delta D(a'_s) = -9'4'' \sin \bar{a}'_s + 55'' \sin 2\bar{a}'_s - 8'6'' \cos \bar{a}'_s, \tag{22}$$

where $\bar{a}'_s$ is the Sun's mean anomaly according to Ptolemy, and where $\Delta(\bar{a}'_s)$ has the same sign as the required correction to a computed longitude (hence, the opposite sign from ΔD). The probable error in a single determination due to errors depending only on the solar anomaly is, thus,

$$\bar{\Delta}(\bar{a}'_s) = \pm 0;9.2^\circ, \tag{23}$$

making the total probable error due to neglected terms equal to

$$\bar{\Delta} D = \pm 0;11.2^\circ \tag{24}$$

For eclipses, however, where the reduction to the ecliptic may be neglected, the probable error from neglected terms becomes

$$\bar{\Delta} D_{\text{ecl}} = \pm 0;9.7^\circ \tag{25}$$

The combined probable error in Ptolemy's equation during eclipses due to both neglected terms (25) and to the errors of the particular model which he assumes (17) is, then, $\pm 0;10.3^\circ$, the major part of which is due to the error in his solar equation.

Taking this error with the average error in elongation found in Ptolemy's eclipse-data, $\pm 0;9.1^\circ$, we should expect the average error in individual determinations of the 'observed' equation to be $\pm 0;13.7^\circ$. Under optimal conditions ($\bar{a} \cong \pm 90^\circ$), this would correspond to an error in the radius of the lunar epicycle of $\approx \pm 0;14$ ($R = 1;0$), although, in general, the error would be considerably larger than this. As noted in the discussion of the errors in his argument of anomaly and its mean motion, Ptolemy's actual determination involves three eclipses and simultaneous solutions for the mean elongation and anomaly as well as the eccentricity. This procedure, however, should not greatly affect the probable error of each determination.

It seems, then, that the probable error of a single determination of the Moon's eccentricity is roughly five times as great as the difference between Ptolemy's value for this parameter and the optimal value (0;5,17,35). Moreover, the contribution of the latter error to the total error of a computed longitude is negligible in contrast to the error originating from the use of an eccentric model and from the omission of inequalities with arguments other than the mean anomaly. Indeed, Ptolemy's value for the radius of the lunar epicycle is sufficiently accurate that no improvement upon it would significantly alter the accuracy of his equation at syzygy, unless accompanied by both the use of an equant model and the introduction of inequalities equivalent to the omitted terms listed above.

Errors at quadrature. Ptolemy's lunar model at quadrature [*Alm.* v 2–4: Toomer, 259–269] is similar to his model at syzygy except that the distance of the center of the epicycle from the observer (R) is 0;39,22,(30) instead of 1;0. Thus, the equation in this synodic situation is identical with that of an eccentric model with eccentricity $e' = 0;8 = 0.1333$. Following the procedure described above, we obtain the coefficients of $\sin(n\bar{a})$, which are shown in Table 4.4 together with corresponding values from Brown 1919, 8.

n	I Ptolemy	II Brown	III Δ(II–I)
1	$-7°\ 38'\ 23.0''$	$-7°\ 29'\ 57.9''$	$+8'\ 25.1''$
2	$+30'\ 38.4''$	$+15'\ 36.3''$	$-15'\ 2.1''$
3	$-2'\ 43.0''$	$-0'\ 46.7''$	$+1'\ 56.3''$
4	$+16.5''$	$+2.8''$	$-13.3''$

Table 4.4. Coefficients of $\sin(n\bar{a})$ in Lunar Equation for $\bar{D} = \pm 90°$

Although the principal coefficient agrees less well with its modern equivalent than in the case of syzygy, the major cause of the error is still the limitations of the eccentric model rather than the effective eccentricity assumed by Ptolemy. The error in the lunar equation—assuming Kepler-motion in an ellipse and substituting e', Ptolemy's value for the eccentricity (i.e., 0;8), for $2e$—is

$$\Delta' g = +8'9.8'' \sin\bar{a} - 4'29.6'' \sin 2\bar{a} + 19.8'' \sin 3\bar{a} - 1.4'' \sin 4\bar{a} \ldots, \quad (26)$$

but the irreducible error of the model assuming eccentric motion as well as the modern value for the eccentricity would be

$$\Delta_e g = +14.2'' \sin\bar{a} - 13'50.9'' \sin 2\bar{a} + 1'47.2'' \sin 3\bar{a} - 12.4'' \sin 4\bar{a} \ldots \quad (27)$$

The value of the radius of the epicycle which would yield the same coefficient for the principal terms as Brown's at distance $R = 1;0$ would be

$$e'_{\text{optimal}} = 0;7,51,14,$$

while the distance R which would yield the same coefficient for $r = 0;5,15$ (r is the radius of epicycle) would be

$$R_{\text{optimal}} = 0;40,6,29 \quad (e_1 = 0;9,56,45).$$

At quadrature the values of the neglected inequalities are:

Annual equation	$+8'0.7'' \sin\bar{a}_s$
Reduction to ecliptic	$-5'50.8'' \sin 2\bar{F}$
Miscellaneous	$+1'34.2'' \sin(\bar{a}_m + \bar{a}_s)$
Miscellaneous	$+1'45.5'' \sin(\bar{a}_m + \bar{a}_s)$.

The 'apparent annual equation' [see 144, above], however, is

$$\Delta D(\bar{a}'_s) = -15'23'' \sin \bar{a}_s + 55'' \sin 2\bar{a}_s - 9'0'' \cos \bar{a}_s. \tag{28}$$

Taken together, the probable error in a computed longitude due to the omitted terms is

$$\bar{\Delta} D = \pm 0;7.2^\circ \tag{29}$$

if the actual annual equation is included, and

$$\bar{\Delta}' D = \pm 0;13.5^\circ \tag{30}$$

if the apparent annual equation is included.

Since Ptolemy's determination of the magnitude of the lunar equation at quadrature depends upon the computed position of the Sun, (30) represents the probable error in a single determination due to the omitted terms and the error in his solar eccentricity. Furthermore, since Ptolemy seeks to determine the equation when the mean anomaly is near $\pm 90^\circ$, the error in the coefficient of the term with argument $2\bar{a}$ [cf. Table 4.4] would affect his results very little. Thus, we need consider only the error in his observed elongations, in addition to that shown in (30) above, to determine the probable error in a single determination of the equation at quadrature.

In chapter 3, the average error of Ptolemy's concluded data from measurements of lunar elongations was found to be $\approx \pm 0;25^\circ$. Taking this as the probable error of a single observation, we find that the probable error in a single determination of the equation at quadrature is

$$\bar{\Delta} g_{\text{obs}} = \pm 0;28.4^\circ. \tag{31}$$

In contrast, the error in the principal term of Ptolemy's equation is only 0;8.4°. Once again, Ptolemy's value is significantly more accurate than would be expected from a single observation, although the disparity is not as large as in the case of syzygy. Conversely, if we assume a normal distribution of errors, we find that the probability that two observations should both yield an error in the principal coefficient of less than 0;10° and with the same sign is on the order of 0.01.

Errors at octant. Ptolemy's lunar model at octant consists of an epicycle of radius $r = 0;5,15$, whose center is $R = (0;49,41^2 - 0;10,19^2)^{1/2} = 0;48,36,(1.5)$. Thus, the effective eccentricity in these synodic configurations ($\bar{D} = \pm 45^\circ, 135^\circ$) is $e' = 0;6,29 = 0.108023$, which value can be used in the standard expression (13) for the lunar equation in eccentric motion.

In contrast to the situation at syzygy and quadrature, however, the *prosneusis* (k) does not disappear at octant, so that the anomaly is increased by $k(\bar{D})$, where k is positive for $\bar{D} = 45°, 225°$ and negative for $\bar{D} = 135°, 315°$.

In consequence, the expression for the equation at octant becomes

$$\begin{aligned} g' &= -e' \sin(\bar{a} \pm k) + \frac{1}{2}e'^2 \sin 2(\bar{a} \pm k) - \frac{1}{3}e'^3 \sin 3(\bar{a} \pm k) \ldots \\ &= e' \cos k \sin \bar{a} + \frac{1}{2}e'^2 \cos 2k \sin 2\bar{a} - \frac{1}{3}e'^3 \cos 3k \sin 3\bar{a} \ldots \\ &\quad \mp e' \sin k \cos \bar{a} \pm \frac{1}{2}e'^2 \sin 2k \cos 2\bar{a} \mp \frac{1}{3}e'^3 \sin 3k \cos 3\bar{a} \ldots, \end{aligned}$$

where the upper signs apply at $D = 45°, 225°$ and the lower signs at $D = 135°, 315°$. Since at octant

$$k = \arcsin\left(\frac{e_1}{1 - e_1}\right) = 1;59.3°,$$

the numerical values of the coefficients become those shown in Table 4.5a–b.

n	I Ptolemy	II Brown	III Δ(II–I)	IV $C(n\bar{a})_{\text{Ptol}}/\cos k$
1	−6° 3′ 16.1″	−6° 17′ 55.9″	−14′ 39.8″	−6° 11′ 17.1″
2	+18′ 19.8″	+13′ 19.6″	−5′ 0.2″	+20′ 3.4″
3	−1′ 10.2″	−37.7″	+32.5″	−1′ 26.6″

Table 4.5a. Coefficients of $\sin(n\bar{a})$ in Lunar Equation for $\bar{D} = \pm 45°, 135°$

Column IV of Table 4.5a shows the values of Ptolemy's coefficients of $\sin(n\bar{a})$ which would result if the anomaly were not affected by *prosneusis*. Thus, the difference between column IV and column I may be taken as the effect of *prosneusis* on the principal terms of the equation of center.

n	I Ptolemy	II Brown	III Δ(II–I)
1	∓1° 17′ 6.5″	∓1° 19′ 37.9″	∓2′ 31.4″
2	±8′ 8.9″	±3′ 35.0″	±4′ 33.9″
3	∓50.9″	∓14.2″	±36.7″

Table 4.5b. Coefficients of $\cos(n\bar{a})$ for $\bar{D} = 45°, 225°$

As shown above for other synodic configurations, the major part of the errors in the coefficients of the higher harmonics arises from the assumption of eccentric circular motion, and so is due more to the model than to its parameters. The coefficients of the two principal terms, however, merit further attention. The coefficient of $\cos\bar{a}$, *which arises solely from Ptolemy's prosneusis*, is analogous to the term known as the evection [cf. Brown 1896, 128], whose modern coefficient equals 1°16′26″ (the difference between this value and 1°19′37.9″ is due to the fact that the latter includes the coefficients of the sines of other harmonics which become coefficients of $\mp\cos\bar{a}$ for $\bar{D} = \pm 45°, 225°$). Thus, regardless of whether we consider the evection proper, or its apparent equivalent at octant, it is evident that Ptolemy's value for the evection is very nearly as accurate as his value for the radius of the lunar epicycle at sygyzy.[9]

In contrast to Ptolemy's coefficient corresponding to the evection, that of the so-called 'principal elliptic term' agrees less well with its modern equivalent. The reason for this is partly the effect of the *prosneusis* and partly the fact that Ptolemy's mechanism for increasing the apparent size of the lunar eccentricity by pulling in the center of the lunar epicycle does not represent the apparent eccentricity very well except near the extreme distances.

Although the average of the coefficients of $\sin\bar{a}$ at syzygy and quadrature according to Ptolemy's theory, 6°19′36″, agrees very well with the modern term, 6°17′56″ [cf. Table 4.5a], this mean value occurs neither at $\bar{D}' = \pm 45°$ nor at mean distance (R = 0;49,41), but at a mean elongation slightly greater than ±45°. Without *prosneusis*, the coefficient of $\sin\bar{a}$ at $\bar{D}' = \pm 45°$ (6°11′17″) is roughly 0;8° less than the corresponding modern value; while with *prosneusis* this difference roughly doubles, so that the total difference between the coefficient of $\sin\bar{a}$ at octant and the mean of the coefficients at syzygy and quadrature is 0;16,20°.[10]

[9] The 'apparent evection' found from Brown 1919, 8 becomes ± 1;13,22° at syzygy and quadrature in contrast to 1;19,38° at octant. For Ptolemy, the apparent evection at syzygy and quadrature (i.e., half the difference between the coefficients of $\sin\bar{a}$) is ± 1;18,45°; whereas the coefficient of $\cos\bar{a}$ at octant would be somewhat less than 1;17,6°, if the center of the epicycle were at mean distance at octant. Thus, Ptolemy's apparent evection exhibits a variation similar to that found in modern theory, but with precisely the opposite phase. This variation is not indicated in Tannery's analysis [1893, 211] since the principal term in its coefficient is $e'e_1{}^3$, whereas Tannery neglects all terms smaller than $e_1{}^2$.

[10] The greater part of this last variation is represented by the term in Tannery's concluded expression for the equation, the magnitude of which is given as $e'e_1{}^2$, and which Tannery [1893, 213–213] says is roughly 0;18°. In fact, however, the total coefficient is slowly convergent, while $e'e_1{}^2$ is equal to only 0;8,52°.

At octant, the principal terms in the Moon's equation which are omitted in Ptolemy's model are:

(a) The variation	$\pm 39'29.9''$ $(\bar{D} = \pm 45^\circ, 225^\circ)$
(b) The annual equation	$\pm 11'9.9'' \sin \bar{a}_s - 1'20'' \cos \bar{a}_s$
(c) Reduction to the ecliptic	$-6'51.6'' \sin 2\bar{F} + 50'' \cos 2\bar{F}$.

Including the error in Ptolemy's solar equation, we obtain for the 'apparent annual equation'

$$\Delta D(\bar{a}'_s) = -12'14'' \sin \bar{a}_s + 10'32'' \cos \bar{a}_s. \tag{32}$$

Excluding the variation, the probable error in the elongation due to omitted terms is

$$\bar{\Delta}_1 D = \pm 0;12.7^\circ,$$

and the probable error of a computed elongation due to the errors in the coefficients of the terms in the equation is

$$\bar{\Delta}_2 D = \pm 0'11.5^\circ.$$

The combined probable error in a computed elongation, disregarding the variation, is thus very nearly

$$\bar{\Delta} D = \pm 0;17.1^\circ.$$

Combining this with the probable error of an observed elongation, $\approx \pm 0;25^\circ$, we obtain for the probable error in the determination of any additional inequality from a single observation

$$\bar{\Delta} D = \pm 0;30.3^\circ,$$

an uncertainty which is nearly equal to the magnitude of the variation.

The complete coefficient,

$$e' e_1{}^2 \, (1 + {}^5\!/_2 \, e_1 + 5 e_1{}^2 + {}^{75}\!/_8 \, e_1{}^3 \ldots),$$

is equal to roughly $0;14,45^\circ$. The remaining difference between this amount and $0;16,20^\circ$ is due to a small additional term whose coefficient equals

$$e' e_1{}^3 \, (1 + 4 e_1 + 11 e_1{}^2 \ldots),$$

which is neglected by Tannery.

In conclusion, the parameters of Ptolemy's lunar model represent very well the principal terms of the two largest inequalities in the Moon's motion. At the same time the constraints imposed by the geometrical model Ptolemy assumes introduce errors significantly larger than those due to his parameters, especially at intermediate elongations between syzygy and quadrature. It is also evident that Ptolemy's introduction of the *prosneusis* is needed merely to yield the proper correction for the evection at elongations other than syzygy and quadrature and, consequently, that it does not correspond to a partial correction for an additional inequality at octant. On the other hand, the mechanism by means of which Ptolemy represents the evection—i.e., by drawing in the center of the lunar epicycle and applying the further correction for *prosneusis*—automatically produces a secondary inequality, only part of which is due to the *prosneusis*. This residual inequality should not be considered a partial correction for the variation, as Tannery [1893, 214] implies, since its argument differs from the argument of the variation. Indeed, in discussing *prosneusis* [*Alm.* v 5], Ptolemy does not claim that he is introducing a further inequality, but instead treats *prosneusis* as a necessary part of the correction for the 'inequality based on the Sun'.

If one includes the variation, the probable error of an elongation near octant computed from Ptolemy's theory is $\pm(0;39.5 \pm 0;17.1)°$. Although this error is only slightly larger than the effective probable error in a single observed elongation ($\pm 0;30.3°$), the apparent secondary inequalities produced by the limitations of Ptolemy's model would significantly increase the difficulty of discovering and evaluating the inequality corresponding to the variation. Thus, it is difficult to see how Ptolemy could have substantially improved his lunar model without fundamentally altering it.

In summary, the deficiencies of Ptolemy's lunar theory may be attributed almost wholly to the model itself, and not to the parameters deduced from observations. It is also clear that however we define Ptolemy's correction corresponding to the evection, the magnitude of this correction is very nearly as accurate as Ptolemy's value for the radius of the lunar epicycle at syzygy, despite the fact that the observations from which he determined his second lunar inequality were much less accurate than the observations of eclipses.

Both inequalities, moreover, are markedly more accurate than one would expect from the procedures he describes, the errors of his observations, and the residual errors introduced into his reductions by the constraints of his model. Since this is true not only of the mean motions of all three lunar arguments, but—more significantly—also of their actual values at Ptolemy's time, Ptolemy must have obtained his parameters in some other manner than the one he describes, given that the probability is negligible that all eight determinations should by accident be significantly more accurate than would be expected.

The most natural explanation for this unexpected accuracy is that each of these parameters resulted from many determinations based on a considerably larger body of observations than Ptolemy reports. Indeed, it is difficult to see how Ptolemy could have avoided using some kind of average, since, in general, two separate determinations of the same parameter would show marked differences due to the errors in the observations and their reduction. If Ptolemy did not use some averaging procedure, it is unclear how he could have chosen among conflicting determinations, let alone consistently chosen 'correctly'. At the same time, the conclusion that Ptolemy himself, rather than Hipparchus, must have followed such a procedure is supported most strongly by the small errors in the actual values of all three lunar arguments at the time when Ptolemy observed, as well as by the high accuracy of the correction equivalent to the evection, which is indisputably due to Ptolemy.

We may ask why Ptolemy did not describe such a procedure for arriving at the values of his parameters, if this was in fact the manner in which he determined them. The absence of an explicit description, however, is not at all inconsistent with his general treatment of his own contributions to the substance of the *Almagest*. Although he frequently mentions the contributions of Hipparchus and occasionally discusses the difference between Hipparchus' solution to a problem and his own, nowhere does he attempt either to give a chronological account of his own work or to explain how he arrived at the particular models with which he accounts for the motions of Moon and planets. Indeed, the general objective of the *Almagest* is didactic rather than historical, and for the most part Ptolemy seems more concerned to show how, and from what sort of observations, a given result can best be derived than to justify the results of his own derivations.[11]

Whenever possible, moreover, these demonstrations are both formal and rigorous, exceptions occurring only in those instances where either no rigorous solution can be achieved or where Ptolemy appears not to know the formal solution. This close adherence to the standards of geometrical rigor suggests a further reason why Ptolemy may have chosen to finesse the question of how he actually arrived at his parameters, since he could not have justified with comparable rigor any method of treating errors.

[11] The two parameters whose correctness Ptolemy supports with more than minimal evidence are the mean motion of the Sun and its related motion, precession. This leads one to wonder if Ptolemy may not have had more reason to doubt the accuracy of these parameters than his others.

Thus, if Ptolemy was less than candid concerning the manner by which he arrived at many of his results, his intent may well have been to avoid having to find a logical justification for the treatment of errors, rather than to trick his readers into accepting his results.

APPENDIX 1

Secular Accelerations of the Sun and Moon

The foregoing investigation draws importantly on the evidence of the errors in Ptolemy's solar and lunar observations that is obtained from comparisons with modern theory. Consequently, it is desirable to minimize the possibility of introducing significant systematic errors from modern theory into the results. The inequalities in the motions of the Sun and Moon are presently known with far greater accuracy than such comparisons require. The modern values for the mean longitudes of the Sun and Moon at ancient epochs, however, are affected by considerable uncertainty as to the magnitudes of the secular accelerations of the mean motions of both celestial bodies.

This uncertainty arises primarily from the apparent difference between the results obtained from analyses of modern observations and those derived from ancient observations. It is also, however, reflected in the different results derived from investigations of ancient observations—differences which arise partly from divergent evaluations of the quality of the empirical evidence from antiquity and partly from variations in the observations investigated, methodologies employed, assumptions made, and even errors committed. Finally, a small but additional element of uncertainty arises from the use by the various investigators of slightly different elements—and, hence, of different effective epochs—thus complicating the comparison of their results.[1]

The following discussion reviews the principal attempts to determine the accelerations of the Sun and Moon down to the 'definitive' determination by Spencer Jones [1939], which has been adopted in national ephemerides (i.e., 'modern theory') since 1952. Its purpose is to identify the values of these parameters least likely to introduce significant errors into comparisons of Ptolemy's observations with modern theory.

[1] Fotheringham's researches [1909, 1915a, 1918, 1920, and 1923] are a particularly troublesome example of these difficulties, since nearly all are based on different lunar elements.

My principal finding is that de Sitter's [1927] ostensibly definitive analysis of the ancient observational evidence, which Jones [1939] incorporated in his determination, was seriously flawed by several significant errors, the correction of which causes the apparent difference between the accelerations derived from ancient and modern observations to disappear. This correction leads to a significantly smaller value of the Moon's apparent non-gravitational acceleration ($+3.6''$) than that ($+5.22''$) currently used by the Nautical Almanac Offices [1961, 98, 107], and to a slightly smaller value ($+1.1''$) of the Sun's apparent acceleration than is presently accepted ($+1.23''$). These values are also smaller than those found by Schoch and adopted by P. V. Neugebauer [1929, 1934] and Tuckerman [1962–1964] in their tables. They are also significantly different from those derived from ancient observations by Newton [1969, 1970] and Muller and Stephenson [1975], but are consistent with the results obtained from ancient observations by Curott [1966] and from modern observations by Morrison and Ward [1975]. Moreover, a recent analysis of ancient and medieval observations by Stephenson and Morrison [1984], which includes extensive data from cuneiform sources, suggests accelerations for the period covered by Ptolemy's observations which are only slightly higher than those used here, although lower than those of Fotheringham, Schoch, and, most recently, Newton [1985].

To facilitate the comparison of historical investigations, I have followed the convention of using the term 'acceleration' to denote the coefficient of the term in T^2 in the polynomial expression for any element, where T is expressed in Universal (rather than Ephemeris) Time. Thus, except where otherwise noted, the accelerations referred to denote the apparent accelerations resulting from both gravitational and non-gravitational causes. The symbols used in equations are as follows:

- S_m Sidereal lunar acceleration in longitude
- S'_m Non-gravitational lunar acceleration in longitude, $S_m - 6.05''$
- S_s Sidereal, non-gravitational apparent solar acceleration in longitude due to the slowing of the Earth's rate of rotation
- S_D Acceleration of the Moon's mean elongation, $S_m - S_s$
- S'_D Non-gravitational acceleration in elongation, $S'_m - S_s = S'_D - 6.05''$

Ever since Clemence's paper [1948, 172], it has been customary to use Ephemeris Time as the independent variable and to consider $\Delta T = ET - UT$ (the cumulative effect of the Earth's variable rotation) in place of S_s, and to use $^1/_2\,\dot{\eta}_m$ (the resulting non-gravitational retardation of the Moon's sidereal longitude) in place of S'_m. To facilitate comparisons with recent studies, I note the following relationships between the accelerations discussed here and related parameters discussed by others. (The approximate relationship for S'_m results from the adoption of different effective epochs for the modern mean motions.)

$$S_s = \frac{\Delta T}{24.35T^2} = \frac{1/2\,\dot{e}}{24.35} = \frac{\dot{\psi}/w_\epsilon}{15.46}$$
$$S'_m = \frac{\mu_m}{\mu_s}S_s + \frac{1}{2}\dot{\eta}_m \cong 13.168S_s + \frac{1}{2}\dot{\eta}_m.$$

Early determinations of the Moon's acceleration

The first to suggest that the Moon exhibited a sensible acceleration was Edmund Halley. On October 19, 1692, he read a paper before the Royal Society proposing that certain discrepancies among the terrestrial longitudes ascribed to such places as Babylon and Antioch could be reconciled by supposing that the Moon (and planets) were retarded by the aether [MacPike 1932, 229].[2] This retardation, Halley concluded, showed the impossibility of the world's eternity. Subsequently, on October 18, 1693, he promised [MacPike 1932, 232]

> to make out the necessity of the world's coming to an end, and consequently that it must have had a beginning, which hitherto had not been evinced from anything that has been observed in nature.

Although the *Journal Book* of the Royal Society [see MacPike 1932, 232] notes that Halley was ordered to print a dissertation on this subject, his only published reference to the Moon's acceleration appeared in 1695 as a postscript to an article discussing the ruins of Palmyra [Halley 1695, 174].[3]

[2] MacPike [1932, 210] has collected the references to Halley in Thomas Birch's *History of the Royal Society*, which includes the contents of the Society's *Journal Book* up to December 1687. MacPike also published further references to Halley from the Society's *Journal Book* from January 1687/8 to July 1, 1696. The quotations in the text are from this source.

[3] One frequently encounters the statement that Halley first proposed the existence of a lunar acceleration in an earlier paper published in 1693 [Halley 1693], in which he discusses four eclipses described by al-Battani and corrects some of the numbers given in the two editions of al-Battānī [1537, 1645] then available. Although it is possible that his discovery of the acceleration arose from comparing his reconstructed epochs for al-Battānī's lunar arguments with values computed from contemporary lunar theory, Halley makes no mention of the phenomenon in this paper. Cf. Houzeau and Lancaster 1882–1889, ii col. 1197.

In this he asked 'any curious traveller residing there' to make observations of lunar eclipses in Baghdad, Aleppo, and Alexandria, so as to enable him to re-determine the longitudes of these places. With secure values for these longitudes, he

> could then pronounce in what proportion the Moon's motion does accelerate; which that it does I think I can demonstrate, and shall (God willing) one day make it appear to the publick.

The promised publication never appeared, and it seems that Halley never succeeded in determining the amount of this acceleration. In the second edition of the *Principia* [1713, 421], Newton did mention that Halley was the first to discover the Moon's acceleration as shown by Babylonian eclipses and eclipses observed by al-Battānī. This reference, however, was suppressed in the third edition of 1727 for reasons I have been unable to discover.[4] Moreover, Halley makes no reference to this acceleration in his lunar tables [1749], which were completed (although not published) by 1720, suggesting that he was unable to satisfy himself that it really existed.

After Halley, the question of the Moon's acceleration was taken up by Richard Dunthorne [1749, 162] who attempted to determine the amount of 'that acceleration of the Moon's motion which Dr. Halley suspected'. In his determination, he rejected eclipses observed by Tycho Brahe and Bernard Walther as being too near his own epoch, and also those observed by al-Battānī because of the uncertainty of the longitudes of Antioch and Racca. Instead, he used three solar eclipses—two of which were reported by Ibn Yūnus (977 and 978) and the other by Theon (364)—and three lunar eclipses reported by Ptolemy (−720, −382, −200). He chose the latter because each occurred near Sunrise or Sunset and thus afforded a partial check on the times reported by Ptolemy. From these eclipses, Dunthorne concluded that the magnitude of the Moon's acceleration was roughly 10″, an estimate which has proven to be very nearly correct.

Values of the accelerations similar to Dunthorne's were subsequently obtained by Mayer [1752] and Lalande [1757], but neither introduced any additional observational evidence or significantly improved upon Dunthorne's rough analysis.[5] Concurrently, the Moon's acceleration was proving an embarrassment to theoretical astronomers, since no gravitational explanation

[4] I can find no reference to this question in the published correspondence of either Halley [MacPike 1932] or Newton [Turnbull 1959–1961, Edleston 1850, Cohen 1958, Rigaud 1841].

[5] Mayer [1752, 389–392] discusses only the two Arabian eclipses used by Dunthorne and remarks on the unsatisfactory nature of the Ptolemaic eclipse reports.

for this phenomenon could be found. As a result several papers appeared, most notably by Lagrange [1773], Jean Bernoulli [1773], and LaPlace [1773], in which the authors emphasized that the empirical evidence supporting the existence of this phenomenon was not decisive, particularly in view of the (ostensibly) dubious reliability of Ptolemy's reports. Curiously, all these authors considered only the eclipses discussed by Dunthorne and ignored the 16 others described in the *Almagest*.

A theoretical explanation of the Moon's acceleration was finally achieved by LaPlace [1786], who showed that it resulted from a slow variation of the eccentricity of the Earth's orbit. Moreover, LaPlace's initial computation of the magnitude of the acceleration, 11.135″, agreed well with the empirical determinations of Dunthorne, Lalande, and Mayer.

The close agreement between the theoretical and empirical values of the Moon's acceleration reduced the suspicion with which Ptolemy's eclipse-reports had been regarded. It also reduced the necessity of a more precise empirical determination, since the magnitude of the acceleration could be computed from gravitational theory using elements known with high accuracy from modern observations. In his *Mécanique celeste*, LaPlace [Bowditch 1829–1839, iii 643] justified his final value for the Moon's acceleration, 10.18″ . . ., with the remark,

> This secular equation is placed beyond doubt by Mr. Bouvard, by a profound discussion of the ancient eclipses which were known to astronomers and also of those he has obtained from an Arabian MMS of Ibn Yunis.[6]

Bouvard, however, seems not to have published this paper, and LaPlace evidently did not think it necessary to discuss his results further. Elsewhere LaPlace [1835, 492–494] showed that his own computed values of the accelerations of the Moon's elongation, anomaly, and argument of latitude yielded values for these arguments at Thoth 1, Nabonassar 1 (Ptolemy's epoch) that were in good agreement with Ptolemy's tabular values, values which LaPlace took as representative of Ptolemy's eclipse-data.

In his tables, Mayer includes a correction for the Moon's acceleration equivalent to $+6.7''T^2$ (epoch: 1700), without indicating how he arrived at this number. In a subsequent revision of his tables, Mayer [1770] changed the magnitude of the acceleration to +9.00″, again without explanation.

Lalande [1751, 430] obtained the value of +9.886″, using the same eclipses as Dunthorne, but after making small corrections to the Moon's mean anomaly at the time of the Arabian eclipses (+977,8).

[6] A text and translation of the observations reported by Ibn Yūnus were published by Caussin in 1804.

As a result of LaPlace's work, it was generally accepted that the available ancient observations supported the magnitude of the Moon's acceleration computed from gravitational theory, which in turn was considered more accurate than any empirical determination. Consequently, ancient eclipses received little attention during the first half of the nineteenth century, except for occasional attempts [cf., e.g., Wurm 1817, Zech 1851] to improve the modern values of the Moon's mean motions in anomaly and argument of latitude.

By 1850, improvements in the accuracy of the lunar theory made it possible to use the path of totality of solar eclipses as evidence of the magnitude of the Moon's acceleration. Airy [1853, 1857], and Hansen [1854, 8] investigated the circumstances of a few ancient solar eclipses which appeared to have been total at known places, and showed that these reports could be satisfied by a small increase in the value of the secular acceleration found by LaPlace. As a result, Hansen adopted the value $12.18''$ for the sidereal acceleration of the Moon in his lunar tables published in 1857, even though this value differed from the theoretical value.

Shortly before the publication of Hansen's lunar tables, Adams [1854] showed that certain terms in the development of the theoretical value of the acceleration, which LaPlace and others had neglected as insensible, were not insensible at all; and that, when these were included, the value for the acceleration was roughly half that obtained by omitting them. This discovery precipitated a heated controversy, but was eventually accepted. The definitive value for the Moon's theoretical sidereal acceleration was found by Brown [1909, 148; 1919] to equal $+6.05'' \pm 0.02''$ (1900).

By destroying the apparent agreement between the theoretical value of the secular acceleration and that found from ancient eclipses, Adams' discovery re-established the desirability of securely determining the secular acceleration from ancient observations. The problem should have been straightforward, since, as Newcomb [1878, 25] pointed out, the secular acceleration could be determined from the Ptolemaic and Arabian eclipses with a probable error of $\pm 0.4''$ and $\pm 0.8''$ respectively, if the Moon's mean centennial motion could be determined from modern observations with an equivalent accuracy. The latter seemed possible given the number and precision of observations since 1750, provided that the deviations from theory since 1750 could be attributed to either observational errors or errors in theoretical terms of short period. Thus, the principal requirements for a straightforward solution were merely that the coefficients of the significant theoretical inequalities of long period be accurate and that the ancient observations be free of large systematic errors.

As it turned out, neither requirement could be satisfied with certainty. The first condition—that Hansen's lunar theory should adequately represent the inequalities of long period in the Moon's motion—was initially challenged by Delaunay [1863], who showed that a large term which Hansen had found to arise from the action of Venus, $+21.47'' \sin(8V - 13G + 4;44°)$, was virtually insensible ($0.272''$) when its development was completed. Due to the difficulties attending the development of the planetary terms in the lunar theory, this conclusion (like Adams') was also questioned for some time. But subsequent investigations confirmed Delaunay's calculation, and virtually eliminated the possibility that a term of this magnitude would remain undetected.

Since Hansen [1854] had shown that his theory, including the questionable Venus-term, satisfied the observations from 1750 to 1850 well, the correction of this term meant that the Moon exhibited unexplained deviations from its theoretical position. These deviations, moreover, could not be adequately described by the observations in this interval, since the period of the inequality supposed to account for them (239 years) was more than twice the interval for which reliable observations were available. Thus, the determination of the secular acceleration from ancient observations came to require also a resolution of the discordance between modern theory and observations, in order to permit establishing the Moon's mean motion securely from modern observations.

Modern determinations of the accelerations of the Sun and Moon

The problem of re-determining the Moon's acceleration from ancient observations was first attacked intensively by Newcomb. In 1870, he showed that Hansen's theory, even with the erroneous Venus-term, failed to satisfy both a number of eclipses prior to 1750 and the most recent observations since 1850. This removed any possibility of describing the Moon's deviation from theory solely by means of observations from the period 1750–1850, and caused Newcomb to investigate observations of occultations and eclipses made by 17th and 18th century astronomers (later extended in his second memoir to include observations of occultations to 1908).

Having extended the interval for which lunar observations could be used to obtain the necessary corrections to Hansen's theory, Newcomb made two separate attempts to determine these corrections. The first, published in 1878, used observations of occultations and eclipses from 1620 to 1750 together with the errors deduced from Hansen's theory by eliminating the above-mentioned Venus-term. The second, published in 1912, extended the comparisons of occultations to 1908 and introduced certain corrections to

Hansen's elements and planetary terms. In both investigations, Newcomb rejected all ancient reports of ostensibly total solar eclipses,[7] and determined the Moon's acceleration from the times of the lunar eclipses reported by Ptolemy and of the lunar and solar eclipses described by Ibn Yūnus.

The results of these two investigations were very nearly identical, despite the several refinements and great amount of additional observational material included in the later paper. After removing the empirical Venus-term, Newcomb [1878] found the following corrections to Hansen's mean longitude for 1800:

$$\begin{aligned}\Delta L_{1878} &= -1.14'' - 29.17''T - 3.86''T^2 + 15.5'' \sin(1.32°T + 93.9°)\\ S'_D &= 2.27'' \\ S_D &= 8.30'',\end{aligned} \tag{1}$$

while in his later paper he found the correction to be:

$$\begin{aligned}\Delta L_{1912} &= -0.31'' - 26.57''T - 4.22''T^2 - 0.0067''T^3\\ &\quad + 12.95'' \sin(1.31°T + 100.6°)\\ S'_D &= 1.91'' \\ S_D &= 7.94''.\end{aligned} \tag{2}$$

Subsequently, Brown [1913, 699; 1915, 513] found that Newcomb omitted some planetary terms of long period in his second paper which, when included, made Newcomb's final result for 1800:

$$\begin{aligned}\Delta' L_{1912} &= -1.14'' - 27.24''T - 3.378''T^2 - 0.0067''T^3\\ &\quad + 12.95'' \sin(1.31°T + 100.6°)\\ S'_D &= 2.75'' \\ S_D &= 8.77''.\end{aligned} \tag{3}$$

In his papers of 1878 and 1912, Newcomb followed slightly different procedures in arriving at his corrections to Hansen's elements, but both solutions were based on the assumption that the deviation from theory in modern times was properly described by a mean motion and sinusoidal term which minimized the squares of the deviations. The major part of Newcomb's correction to the Moon's mean motion and his entire correction to the mean

[7] Cf. Newcomb 1878, 28–34; 1912, 228–246, for an excellent critical discussion of the quality of the ancient reports of total eclipses as evidence for determining the amount of the accelerations of the Sun and Moon.

longitude at epoch thus arise from solving the equations of condition derived from modern observations on the assumption of a periodic deviation.

Furthermore, a substantial part of Newcomb's correction to Hansen's acceleration was due to his resulting correction to Hansen's mean motion. Thus, as shown in his earlier paper, Newcomb's correction to Hansen's mean motion by itself required a corresponding correction to Hansen's acceleration of

$$\Delta S_m = -1.25''(1800)$$
$$S_m = 10.9''(1800)$$

in order to satisfy the solar eclipses of Thales (−584), Larissa (−556), and Agathocles (−309), which Hansen used. Thus, the effective difference between the secular acceleration Newcomb derived from the Ptolemaic and Arabian lunar eclipses (1878) and the acceleration satisfying these three solar eclipses was $\approx 2.1''$, equivalent to roughly 20 minutes in the time of an eclipse at Ptolemy's epoch and to 35 minutes at −400.

Newcomb's work raised two important problems. The first was whether it was proper to assume that an unexplained deviation from gravitational theory in the Moon's motion was periodic over the interval for which modern observations were available and, thus, whether Newcomb's reduction of Hansen's mean motion was justified. Although there appears to be no formal justification for doing so [cf. van der Waerden 1961], the absence of a more satisfactory procedure has made it common practice to determine the Moon's mean motion by a periodic least-squares analysis, which minimizes the deviations shown by modern observations. Thus, most of Newcomb's reduction of Hansen's mean motion has been accepted.

The second problem, which Newcomb discussed in his paper of 1878, was whether the Ptolemaic and Arabian eclipses did not require a smaller value in the Moon's acceleration than that which appeared to satisfy certain ancient solar eclipses. This question became a matter of controversy even before Newcomb published his second paper and eventually occasioned a re-examination in bits and pieces of all of the relevant ancient observations.

In a series of memoirs, Ginzel [1882–1884] discussed reports of over 50 solar eclipses ranging in date from −752 to 1415. From 29 of these, he obtained corrections to Hansen's elements which slightly reduced Hansen's acceleration, but which increased his mean motion in 1800 by $9''$. Ginzel also arrived at a correction to the motion of the Moon's perigee which was considered too large to fall within the limits of uncertainty of either modern theory or modern observations. Finally, in his *Spezieller Kanon der Finsternisse* [1885, 5], Ginzel published small additional corrections. In 1887, Oppolzer published his *Kanon der Finsternisse* [cf. Oppolzer 1962], which was based upon Hansen's elements modified by a different empirical correction than Ginzel's. Newcomb [1912, 238] showed that Oppolzer's correction to the Moon's mean motion and secular acceleration was virtually identical with his own, but that Oppolzer also incorporated inadmissible corrections to the mean motion of the node and the secular acceleration of the perigee, both of which were thought to be determined securely from gravitational theory.

In 1905 and 1906, Cowell analyzed reports of six ancient solar eclipses, which seemed to indicate that totality was visible at specific locations. Except for the eclipses of −309 (Agathocles) and −430 (Thucydides), neither Newcomb nor Airy had previously discussed any of these eclipses. Cowell concluded that five solar eclipses (−1062, −762, −647, −430, and 197) could be satisfied only by decreasing the secular acceleration of the Moon's node or increasing the secular acceleration of the Sun and Moon by 3.5″.

Newcomb challenged Cowell's results, arguing that such a reduction in the acceleration of the node was inadmissible on theoretical grounds, while his own analyses of modern observations of the Sun and Mercury rendered implausible the existence of a solar acceleration only a third as large as Cowell proposed. Nevertheless, although the numerical results of Cowell's analysis were never widely accepted, his suggestion that the Sun exhibited a perceptible acceleration was eventually confirmed by subsequent investigators.

After Newcomb's last memoir, Fotheringham took up the problem of determining the secular accelerations of the Sun and Moon from ancient observations. In a series of papers extending from 1909 to 1927, Fotheringham analyzed not only the observations of solar and lunar eclipses [1920a–b] which had been previously utilized for these purposes, but also the equinox-observations of Hipparchus [Fotheringham 1918], the lunar eclipse-magnitudes reported by Ptolemy [Fotheringham 1909a], and the lunar occultations reported in the *Almagest* [Fotheringham 1915a]. His final estimate of the values best satisfying the eclipses and occultations was $S_m = +10.8''$, $S_s = +1.5''$, and $S_D = 9.3''$ ($S'_D = 3.27''$), applied to a mean motion and epoch (1800) very nearly identical to Newcomb's [cf. Fotheringham 1920b, 125].

Fotheringham's values for the secular accelerations derived from different types of observations are shown in Table A1.1. His discussion of the non-Babylonian eclipses reported by Ptolemy led to nearly the same acceleration of the Moon's mean elongation as the one Newcomb had obtained from his analysis of both Ptolemaic and Arabian eclipses. His investigations of other ancient data, however, indicated both a larger secular acceleration of the Moon and the existence of a sensible acceleration of the Sun. The latter was perhaps Fotheringham's most significant finding, and was attested directly by the Alexandrian eclipse-magnitudes and Hipparchus' equinox-

	Lunar Eclipses	Lunar Eclipse Magnitudes	Occultations	Equinoxes (Hipparchus)	Solar Eclipses (Totality)
S_m			$10.3'' \pm 0.74''$ [a]		$10.8''$
S_s		$1.78'' \pm 0.45''$		$1.95 \pm 0.27''$	$1.5''$
S_D	$7.9''$				$9.3''$

[a] Corrected from 10.8″ in accordance with Fotheringham 1923, 273.

Table A1.1. Fotheringham's Accelerations of the Sun and Moon from Different Ancient Observations

observations, as well as indirectly by the difference between the values for the lunar acceleration derived from occultations and the acceleration in elongation derived from lunar eclipses.

The individual values for the Sun's acceleration determined from the different sets of observations were not entirely consistent, and the discrepancies appeared to support a relatively high value for this acceleration. The occultations and lunar eclipses suggested a solar acceleration of 2.4″ (originally 2.9″, close to Cowell's value), compared with roughly 1.9″ (originally 1.0″) from equinoxes, 1.8″ from eclipse-magnitudes and 1.5″ from solar eclipses. Similarly, the acceleration of the Moon's elongation found from occultations, equinoxes, and eclipse-magnitudes was 8.4″, compared with 9.3″ from solar eclipses and 7.9″ from lunar eclipses. Thus, Fotheringham's results appeared to confirm the discrepancy, first suggested by Airy [1853] half a century earlier, between the acceleration in elongation implicit in the lunar eclipse-times and that derived from other ancient data.

Fotheringham's results became an important element in the derivation of the accelerations presently accepted as 'modern theory'. Accordingly, the specific values which he obtained from different types of observations deserve critical scrutiny.

First, in determining the Sun's secular acceleration from Hipparchus' equinoxes, Fotheringham [1918] assumes a constant error in declination (−0;4.4°), which he derives from Hipparchus' declinations of seven stars near the equator [cf. Ptolemy, *Alm.* vii 3]. He then applies this error, which differs appreciably from the mean systematic error of +0;0.7° for all 18 declinations [cf. Pannekoek 1955, 64], to Hipparchus' spring equinoxes from −134 to −127 in order to obtain his 'definitive result',

$$S_s = +1.95'' \pm 0.27''.$$

In the same paper, Fotheringham showed that assuming an error in declination which would yield the best fit for all equinoxes ($-0;7.6° \pm 0;0.46°$) would make the most probable acceleration

$$S_s = +1.0'' \pm 0.18''.$$

Thus, while the probable errors obtained from the discordances are relatively small, the determination is very sensitive to the assumed systematic error in declination. On balance, the lower result seems at least as probable as the higher, but virtually any value for the secular acceleration of the Sun between $\approx +0.8''$ and $+2.0''$ is arguably consistent with Hipparchus' equinox-observations.

Much the same can be said of Fotheringham's determinations based on the reported lunar eclipse-magnitudes and occultations. In the case of the former, he [1909a] excludes the Babylonian eclipses, which would increase the secular acceleration, while taking no account of the uncertainty of the motion of the node. As a result his final determination,

$$S_s = +1.78'' \pm 0.45'',$$

is uncertain by a considerably larger amount than the error he estimates.

In the case of the occultations, the result, which Fotheringham deduced from a set of seven very discordant observations, depends largely on his assumptions about the probable clock-errors. Using three different assumptions—(1) that the clock-error was proportional to the time from Sunrise or Sunset, whichever was closer to the event; (2) that the clock-error was independent of the time from Sunrise or Sunset; and (3) that the clock-error was proportional to the time from Sunset alone (which he describes as 'improbable,')—Fotheringham [1915a, 393] found:

$$\begin{aligned}
&\text{(a)}\ \ S_m = +10.8'' \pm 0.7'' \\
&\text{(b)}\ \ S_m = +10.8'' \pm 0.9'' \\
&\text{(c)}\ \ S_m = +10.0'' \pm 0.8''.
\end{aligned}$$

Of these, he accepted (a) as the most probable. Subsequently, Fotheringham [1923] corrected an error in his comparisons, thereby modifying the above values (assuming the same modern mean motions) to:

$$\begin{aligned}
&\text{(a}'\text{)}\ \ S_m = +10.1'' \pm 0.7'' \\
&\text{(b}'\text{)}\ \ S_m = +10.1'' \pm 0.9'' \\
&\text{(c}'\text{)}\ \ S_m = +\ 9.3'' \pm 0.8''.
\end{aligned}$$

From (a′) and a further correction to Cowell's value for the Moon's mean motion, Fotheringham [1920b, 125] concluded that the Moon's sidereal acceleration best satisfying Ptolemy's occultations was

$$S_m = 10.3'' \pm 0.74''.$$

The probable error of this, however, could easily be increased by a slightly different estimation of the probability of assumptions (a) and (c).

In 1920, at the conclusion of a paper re-investigating the ancient solar eclipses, Fotheringham [1920b, 126] announced his oft-quoted values for the secular accelerations of the Sun and Moon,

$$S_m = +10.8'' \quad S'_m = +4.75''$$
$$S_s = +\ 1.5'' \quad S'_D = 3.25'',$$

which he asserted best satisfied all classes of ancient data. As shown by the graph on [1920b, 123] of that paper, these eclipses give extremely uncertain and discordant results. Indeed, Fotheringham seems to have obtained his final values by assuming the value of the secular acceleration of the Moon previously derived from the Ptolemaic occultations (10.8″), and accepting the largest solar acceleration consistent with this value and the condition that the eclipse of −128 be total at the Hellespont. His subsequent correction of the Moon's acceleration as determined from the occultations would have satisfied the eclipse of −128, with values for the solar acceleration ranging from +0.9″ to +1.25″; while his lower value for the Moon's acceleration derived from the occultations under assumption (c) would have satisfied the eclipse of Hipparchus, together with several others with a solar acceleration ranging from +0.5″ to +0.9″.

If we disregard Fotheringham's determination of the Sun's acceleration from eclipse-magnitudes and Hipparchus' equinoxes as too uncertain, or, alternatively, if we accept the value $S_s = +1.0''$ derived from his intitial analysis of the equinoxes as equally probable as his concluded value (+1.95″), then the bulk of the solar eclipses, including that of Hipparchus, would be satisfied by the accelerations:

$$S_m = +9.9'' \pm 0.4''$$
$$S'_m = +3.85'' \pm 0.4''$$
$$S_s = +0.9'' + 0.2''\text{[8]}$$
$$S'_D = +2.95'' \pm 0.6''.$$

These values, moreover, agree with the corrected results of Fotheringham's analysis of the occultations on either assumption concerning the clock-errors, as well as with his initial determinations of the secular acceleration

[8] Cf. Fotheringham 1923, 123.

of the Sun from Hipparchus' equinoxes. They also agree very nearly with Newcomb's final determination (as corrected by Brown) of the acceleration of the Moon's elongation from both Arabian and Ptolemaic eclipses, the discordance being reduced to $\approx 0.2''$.

Following Fotheringham's investigations, Schoch [cf. 1926, 3; 1931] recomputed the occultations described in the *Almagest* with greater precision than Fotheringham had and also re-investigated the circumstances of a number of ancient solar eclipses. Schoch's procedure for determining the values of the two accelerations from this material contrasted sharply with both Newcomb's and Fotheringham's. Whereas they had derived their results from the average deviations of a relatively large number of observations, Schoch's values, as far as I can make out, were determined from two events, the occultation of Spica observed by Timocharis in −282 Nov 8 [Ptolemy, *Alm.* vii 3: Toomer, 336] and the solar eclipse of −128 Nov 20 associated with Hipparchus. Concerning the former, Schoch noted a discrepancy (previously remarked by Ptolemy) between the time reported for the occultation and the comment that it occurred 'just as the Moon was rising'. Accepting the second designation as more accurate and interpreting it to mean that the occultation took place half an hour after Moonrise, Schoch concluded that the sidereal secular acceleration of the Moon was

$$S_m = +11.09''.$$

Although he gives no details, he says in the same work [1926, 3] that the Sun's acceleration was determined from the ancient solar eclipses, of which 'the best criterion for [determining] the element is the eclipse of Hipparchus in −128'. Since Schoch's adopted value, $S_s = +1.511''$, would make this eclipse central at the Hellespont, given the lunar acceleration noted above, his result appears to rest on this assumption.

Having determined the accelerations in this manner, Schoch [1926, 2] dismissed the lunar eclipses reported by Ptolemy as 'worthless', and showed that his values agreed more or less with various solar eclipse-reports and with a lunar eclipse in −424 Oct 9 recorded in a cuneiform text [Kugler 1913–1935, 233]. Since both Fotheringham and Newcomb showed that some eclipses can always be more or less satisfied by any pair of reasonable accelerations, Schoch's procedure scarcely enhances the credibility of his results. In this respect, it is also unfortunate that Schoch did not publish more of the details of his computations and comparisons.

The results obtained by Fotheringham and Schoch were further analyzed by de Sitter in a paper published in 1927, which was generally accepted by contemporary astronomers as the definitive discussion of the ancient observational evidence. In it de Sitter sets up separate equations of conditions for:

(i) the accelerations of the Sun determined by Fotheringham from

(a) Hipparchus' equinoxes,

(b) the solar eclipses, and

(c) the lunar eclipse-magnitudes;

(ii) the accelerations of the Moon determined from

(d) Ptolemy's occultations and

(e) ancient solar eclipses;

(iii) the relationship between the two accelerations found by Fotheringham from

(f) the eclipse of Hipparchus (-128); and

(iv) the acceleration of the Moon's elongation determined by Fotheringham from

(g) the Alexandrian lunar eclipses and

(h) Schoch's discussion [1926, 3] of the Babylonian lunar eclipse of -424.

After weighting these equations according to Fotheringham's and Schoch's estimates of the probable error of each determination, de Sitter [1927, 23] obtained the non-gravitational accelerations (1900),

$$S'_m = (5.22'' \pm 0.30'')R$$
$$S_s = (1.80'' \pm 0.16'')R,$$

where $R = T^2 + 1.33T - 0.26$. R was introduced to minimize the effect of the corrections on the agreement between theory and modern observations, and makes the effective epoch of the mean motions 1833.5.

De Sitter's procedure in arriving at these results affords several grounds for criticism, and it is hard to understand why others have accepted his analysis so uncritically as representing the evidence of ancient observations. In the first place, he treats a number of Fotheringham's results—e.g., the accelerations of the Sun and Moon derived from solar eclipses, and the relation between them derived from the solar eclipse of Hipparchus (-128)—as independent determinations, when in fact they are independent neither of each other nor of the rest of Fotheringham's results. Indeed, the only evidence afforded by the solar eclipses alone which supports the relatively high value for the lunar acceleration adopted by Fotheringham is the so-called Eclipse of Babylon in -1062. Since there is considerable doubt as to whether this vague report refers to an eclipse at all [cf. Fotheringham 1920b, 105–106], there is no justification for counting it a condition to be satisfied.

A second criticism of de Sitter's procedure is that he adopts Fotheringham's estimates of probable error as the basis of weighting his equations without taking any account of the sensitivity of Fotheringham's results to slightly different assumptions about the observational procedures or their possible systematic errors. This is particularly true of the values of the secular acceleration of the Sun determined from Hipparchus' equinox-observations and from lunar eclipse-magnitudes and of the lunar acceleration determined from occultations.

Finally, and most significantly, de Sitter's results are vitiated by important numerical errors. In deriving the equation of condition for the Moon's secular acceleration as determined from the occultations—which is the only independent evidence in support of a lunar acceleration greater than 10″—de Sitter not only disregards Fotheringham's subsequent correction of his first determination, he also computes ΔL incorrectly, arriving at a figure 610″ too large. Even worse, in his equations derived from the accelerations of the Moon's elongation found by Fotheringham, he includes the total difference, $S_D = S_m - S_s$, into the computation, although the rest of his equations and his solution are for only the non-gravitational component, S'_D. To correct for this, the numbers +2950″ and +2320″ [de Sitter 1927, 22] must be replaced by +620″ and +660″, respectively.

When these corrections are made and de Sitter's weights for individual equations of conditions are revised to reflect somewhat larger estimates of the probable errors in each determination than Fotheringham's, significantly lower values for both accelerations result. Furthermore, de Sitter's use of Fotheringham's revised determination of the Sun's acceleration from Hipparchus' equinoxes (1.95″) instead of his initial solution (1.0″) seems unjustified in view of the several questionable assumptions which Fotheringham made in arriving at the higher value. Although these observations are at best tenuous evidence of the magnitude of the Sun's acceleration, it seems preferable to accept the lower value with a probable error equal to roughly the same amount (±1.0″) in combining determinations from different types of observations.

With these corrections, and using the mean of Fotheringham's corrected results for the occultations deduced from assumptions (a′) and (c′) [see 164, above], I find on re-solving de Sitter's equations:

$$\begin{aligned} S_m &= +9.67'' \pm 0.5'' \\ S'_m &= +3.62'' \pm 0.5'' \\ S_s &= +1.14'' \pm 0.3'' \\ S_D &= +8.53'' \pm 0.6'' \\ S'_D &= \ \ 2.48'' \pm 0.6''. \end{aligned}$$

These values satisfy all of the Ptolemaic observations; and the acceleration of the Moon's elongation, S_D, is very close to what Newcomb deduced from Ptolemaic lunar eclipses. To satisfy the majority of the ancient solar eclipses discussed by Fotheringham [1920b] would require that $S_D = 8.9''$ and, thus, either a somewhat larger lunar acceleration ($\approx +10.1''$) or a smaller solar acceleration ($\approx +0.8''$); but the uncertainties and ambiguities attending these reports greatly diminish their value as evidence of either acceleration [cf. Newcomb 1912, 228–246]. Furthermore, the Arabian eclipse reports discussed by Newcomb are best satisfied by opposite corrections, namely, an increase in the Sun's acceleration or a decrease in the Moon's acceleration. Since these eclipses are nearer the modern epoch, and since there are difficulties with some of the reports as well as systematic differences among observations made by different observers, they cannot be taken as conclusive evidence. Nevertheless, they seem at least as valuable as the ancient reports of total solar eclipses and so tend to offset the evidence of the latter.

De Sitter's paper [1927] also addressed the correlation between the apparent accelerations and fluctuations (unexplained discrepancies between observations and gravitational theory) in the longitudes of the Sun, Moon, and planets. If these are due entirely to variations in the Earth's rotation, then their magnitudes should be in proportion to their mean motions. He found this to be true for the accelerations and fluctuations of the Sun and inner planets, clearly not true in the case of the Moon's acceleration, and unclear with respect to the Moon's fluctuations.

After removing the effects of the accelerations derived from ancient observations, de Sitter compared the total fluctuations (including Newcomb's 'great empirical term') of the Sun, Moon, Mercury, and Venus. He found that the best solution to the residuals gave

$$Q\ {}^{n_i}/_{n_m} = 1.25\,{}^{n_i}/_{n_m}$$

as the most probable ratio of the magnitudes of the fluctuations of the Sun and planets to those of the Moon (here n_i is the mean motion of the Sun or planet in question and n_m that of the Moon).

Subsequently, in 1939, Morgan and Scott demonstrated that the meridian-observations of the Sun from 1900 to 1937 could be satisfied by assuming $Q = 1.00$. In the same year, Spencer Jones [1939] reviewed the entire body of modern observations of the Sun, Moon, Mercury, and Venus. Using de Sitter's value for the non-gravitational acceleration of the Moon, $+5.22''$, Jones first solved the equations of conditions for Q and the Sun's acceleration, obtaining

$$Q = 1.025 \quad S_s = +1.25''$$

and

$$Q = 1.062 \quad S_s = +1.26'',$$

depending on whether observations of the Sun's right ascension were included in the analysis. From these results, Spencer Jones concluded that Q was indeed unity.

Re-solving for $Q = 1.00$, Spencer Jones found for the Sun's acceleration

$$S_s = +1.14'' \pm 0.11'' \quad \text{(from solar observations)} \tag{4}$$

and

$$Ss = +1.24'' \pm 0.04'' \quad \text{(from Mercury transits)}, \tag{5}$$

giving a weighted mean of

$$S_s = +1.23'' \pm 0.04''. \tag{6}$$

These values depend upon the assumption that de Sitter's value for the non-gravitational acceleration of the Moon, $+5.22''$, represents the actual non-gravitational acceleration of the Moon over the period for which modern observations are available. As Spencer Jones pointed out, any change $(\Delta S'_m)$ in this value would require a corresponding change,

$$\Delta S_s = \frac{n_s}{n_m} \Delta S'_m = 0.0747 \Delta S'_m,$$

in the value of the secular acceleration of the Sun to satisfy the condition that $Q = 1$ for the fluctuations.

In discussing the discrepancy between his results and de Sitter's, Jones determined the value of $\Delta S'_m$ which would give the same ratio between the non-gravitational accelerations of the Moon and Sun as de Sitter's values. He concludes [1939, 555–556],

> The best values that we can assign for the (non-gravitational) secular accelerations of the Sun and Moon at the present time (or more strictly the best average values for the past two hundred and fifty years) are therefore:
>
> For the Moon $\quad S'_m = +3.11'' \pm 0.57''$
>
> For the Sun $\quad S_s = +1.07'' \pm 0.06''.$
>
> These values of the accelerations will not satisfy any of the ancient observations of eclipses and occultations, which are on the whole in very good agreement with each other in requiring appreciably larger values. There seems to be no escape from the conclusion that the effects of tidal friction are appreciably less at the present time than the average effects over the past two thousand years.

In view of the errors in de Sitter's analysis, and of the evidence discussed above that most of the ancient observations are well satisfied by lower values for both the lunar and solar accelerations than de Sitter found, Spencer Jones' conclusion seems untenable. Indeed, if we replace de Sitter's published value for the non-gravitational acceleration of the Moon (+5.22″) with that found by re-solving his equations with appropriate corrections [see 168, above], the secular accelerations from Jones' analysis become:

$$\begin{aligned} S'_m &= 3.62'' \pm 0.5'' \quad \text{(de Sitter revised)} \\ S_s &= 1.11'' \pm 0.06'' \quad \text{(S. Jones revised)} \\ S_D &= 2.51'' \pm 0.5'' \quad \text{(S. Jones revised)} \end{aligned} \tag{7}$$

Alternatively, if we assume, following Spencer Jones, that the ratio of the accelerations has remained constant (i.e., $^{3.62}/_{1.14}$), we obtain:

$$\begin{aligned} S'_m &= 3.50'' \pm 0.5'' \\ S_s &= 1.10'' \pm 0.06''. \end{aligned} \tag{8}$$

Both sets of values, (7) and (8), are in excellent agreement with those found from re-solving de Sitter's equations with correct data and revised weights. Thus, the apparent discrepancy between the accelerations determined from ancient and modern observations arose mainly from errors committed by de Sitter and unwittingly introduced into accepted theory by Spencer Jones.

In 1948, Clemence transformed Jones' non-gravitational acceleration of the Sun to an expression for ΔT, being the difference between observed Universal Time and an invariable Ephemeris Time (originally called Newtonian time by Clemence). Expressed in Ephemeris Time (ET) Jones' non-gravitational acceleration of the Moon becomes

$$\frac{1}{2}\dot{\eta}_m = 5.22 - 13.368 \cdot 1.23'' = -11.22''$$

or

$$\dot{\eta}_m = -22.44\,''/\text{cy}.$$

Interestingly, the first person to publish an analogous calculation of the Moon's secular retardation was Schoch [1926, 34] who found

$$\frac{1}{2}\dot{\eta}_m = -14.84'' \qquad \text{or} \qquad \dot{\eta}_m = -29.68\,''/\text{cy}.$$

In 1952, Spencer Jones' and Clemence's accelerations were adopted by the International Astronomical Union, and they have since been incorporated in the ephemerides prepared by the American and British Nautical Almanac Offices [1961, 94]. Thus did Fotheringham's results and de Sitter's errors become part of modern theory.

Recent investigations of the secular accelerations

Since the adoption of Spencer Jones' accelerations in 1952, there have been several further studies of these parameters, which on the whole have left the matter as uncertain as ever. In 1952, Brouwer revised and extended Spencer Jones' analysis, excluding de Sitter's results and using modern data from lunar occultations through 1948 and the results of Newcomb's analysis of Ptolemaic and Arabian eclipse-times for ancient data. From these, he obtained

$$\begin{gathered} S_s = 1.01'' \quad \text{(epoch: 1715)} \\ S'_m = 2.22'' \quad S'_D = 1.21'' \end{gathered} \tag{9}$$

as the accelerations best fitting the ancient and modern data, although he noted [Brouwer 1952, 141] that this result is sensitive to how the Moon's (modern) fluctuations are treated. Brouwer showed that these appeared to be random instead of periodic, and his solution was based on this premise.

In 1961, van der Waerden extended Brouwer's methodological discussion and tried to reconcile the observed accelerations with Jeffreys' theory [1952, 225] which suggested that the ratio of the apparent accelerations, s_m/s_s, should be roughly 6.9, far higher than that resulting from Spencer Jones' accelerations (4.2), let alone Brouwer's (2.2). Van der Waerden derived revised accelerations from four data-points having mean epochs of: 1962 (based on extrapolations from 1958.0 lunar data; 1635 (based on Newcomb's analysis of observations by Gassendi and Hevelius); 950 (based on Brouwer's data derived from Newcomb's study of Arabian eclipse-times); and -386 (based on his own analysis of three apparently critical ancient observations). These last were (a) the Babylonian lunar eclipse of -424 Oct 9 [cf. also Schoch 1926; de Sitter 1927]; (b) the lunar eclipse of -382 Dec 23 observed in Babylon and reported by Ptolemy [see 61–63, above]; and (c) the lunar occultation observed by Timocharis on -282 Nov 8 [see 86–88, above]. These three observations give very discordant results, and van der Waerden's result does not represent any one of them very well, let alone all three. Nevertheless, from these data, he finds accelerations of:

$$\begin{aligned} S_s &= 1.31'' \pm 0.10'' \quad \text{(epoch: 1755)} \\ S'_m &= 6.28'' \pm 0.82'' \\ S'_D &= 4.97'' \pm 0.9'' \end{aligned} \tag{10}$$

In 1966, Curott investigated ancient records of solar eclipses using Ephemeris Time and Spencer Jones' (de Sitter's) value for the Moon's acceleration (5.22″) together with other modern parameters. He found an apparent solar acceleration of 1.10″ ± 0.06″ (epoch: 1900), which becomes

$$S_s = 1.25'' \pm 0.07'' \quad \text{(epoch: 1780)},$$

a result virtually identical with Jones'. Curott, moreover, found an average value for ${}^{\Delta S_s}/_{\Delta S_m}$ of 0.12 for the relevant eclipses, so that for

$$\Delta S'_m = -1.6'' \ \pm 0.5'',$$
$$\Delta S_s = -0.19'' \pm .06''$$

or

$$\begin{aligned} S'_m &= 3.62'' \pm 0.5'' \\ S_s &= 1.06'' \pm 0.09'' \quad \text{(epoch: 1780)}, \end{aligned} \tag{11}$$

a result virtually identical with Spencer Jones' as revised [cf. 171, above].

In 1969, R. R. Newton announced that he had re-analyzed all of the traditional (i.e., non-cuneiform) ancient and medieval observations and found that the apparent accelerations of both the Sun and Moon varied significantly with time. In particular, he found the following (average) accelerations since 1900 for ancient and medieval observations, respectively:

$$\begin{array}{lcc} & \text{Before 500} & \text{After 500} \\ & \text{Epoch: } -200 & \text{Epoch: } 1000 \\ S_s = & 1.79'' \pm 0.22'' & 1.45'' \pm 0.23'' \\ S'_m = & 1.34'' \pm 2.15'' & 3.12'' \pm 3.05'' \\ \dot{\eta}_m = & -41.6'' \ \pm 4.3'' & -42.3'' \ \pm 6.1''. \end{array} \tag{12}$$

Comparing these with a value for $\dot{\eta}_m = -20.1'' \pm 2.6''$, which he [1969, 826] had previously found by analyzing modern data, Newton [1970, 280] concluded that (the average effective value of) $\dot{\eta}_m$ varied in time as

$$\dot{\eta}_m = -22'' + 3.3''\,T + 0.114''\,T^2 \quad (T_0 = 1900),$$

and, thus, that there was a 'strong presumption that $\dot{\eta}_m$ has changed by a factor of 2 within historical times'.

Newton's full analysis was published in 1972. It was followed in the same year by an analysis of 379 additional medieval solar eclipses, from which he found accelerations of:

$$\begin{aligned} S_s &= \ \ 2.75'' \pm 0.65'' \\ S'_D &= \ \ 5.32'' \pm 7.9'' \\ \dot{\eta}_m &= -78.9'' \ \pm 15.9''. \end{aligned} \tag{13}$$

These have an effective epoch of 976, and are clearly inconsistent with the values shown above, values which Newton found from Islamic observations around the same date. Subsequently, Newton implicitly abandoned both sets of results.

In 1975, Muller and Stephenson carefully investigated the circumstances of 25 reports of solar eclipses from ancient and medieval times. From these they found accelerations equivalent to:

$$\begin{aligned} S_s &= 1.88'' \pm 0.21'' \quad \text{(epoch: 1770)} \\ S'_m &= 6.32'' \pm 02.5'' \\ S'_D &= 4.44'' \pm 02.5''. \end{aligned} \tag{14}$$

Of the 25 eclipses, however, the authors regarded only seven as certain while only two contributed evidence defining the lower boundary of S_s. Of these two, one was a partial eclipse observed at an inferred location in China in 120, and the other was a total eclipse observed near the Kerulen River by the party of Ch'ang-ch'un in 1221 [Muller and Stephenson 1975, 491–493].

Furthermore, in 1975, Morrison and Ward re-investigated all of the transits of Mercury from 1677 to 1973. Assuming Spencer Jones' value for the apparent solar acceleration, they [1975, 197–198] found:

$$\begin{aligned} S_s &= 1.23'' \quad \text{(S. Jones)} \\ S'_m &= 3.45'' \pm 2'' \\ S'_D &= 2.22'' \pm 2'' \\ \dot{\eta}_m &= -26.0''. \end{aligned} \tag{15}$$

This result is close to Spencer Jones' when the latter is adjusted to correct for de Sitter's errors, and supports the assumption of constant accelerations since ancient historical times.

Following his polemic against Ptolemy, Newton [1979–1984] attacked Jones' methodology and concluded that the solar and lunar accelerations at the modern epoch (1900) were radically different from those derived by Jones. In addition, he concluded that $\dot{\eta}_m$ is probably constant and equal to $-28.4'' \pm 5.7''$ (in contrast to his earlier finding), but that the rate of the Earth's rotation exhibits a sensible acceleration which he attributed mainly to a change in the gravitational constant. In a subsequent work, Newton [1985b, 324] found S_s to vary as

$$S_s = 0.70'' - 0.0668''T - 0.0015''T^2 \quad (T_0 = 1900). \tag{16}$$

This combined with the value $\dot{\eta}_m = -28''$ results in the following parameters for ancient and modern epochs:

$$
\begin{array}{lcc}
 & \text{Epoch: } -300 & \text{Epoch: } 1900 \\
S_s = & 1.44'' & 0.62'' \\
S'_m = & 5.25'' & -5.71'' \\
S'_D = & 3.81'' & -6.1''.
\end{array}
\tag{17}
$$

While these are inconsistent with his earlier findings, it is interesting that Newton's most recent accelerations for -300 are very similar to those found by Fotheringham and Schoch.

Recently, a number of investigators have used different techniques to measure the lunar acceleration ($\dot{\eta}_m$) directly. As summarized by Stephenson and Morrision [1984, 50], the most accurate of these are:

Investigator	Method	$\dot{\eta}_m$ ″/cy
Morrison/Ward [1975]	transits of Mercury	-26.0 ± 2.0
Lambeck [1980]	numerical tidal model	-29.6 ± 3.1
Cazenave [1982]	artificial satellites	-26.1 ± 2.9
Dickey/Williams [1982]	lunar laser ranging	-25.1 ± 1.2

These results suggest that the current value of $\dot{\eta}_m$ lies between $-24''$ and $-26''$, which compares favorably with the value of $-23.2''$ derived from de Sitter's analysis of ancient observations as corrected.

Stephenson and Morrison [1984] and Newton [1985b] have published new attempts to describe the variation of the Earth's rotation, assuming a constant value for $\dot{\eta}_m$ and using ancient and medieval observations incorporating extensive Babylonian data from cuneiform sources. Though their methods and conclusions differ, they all find that a constant acceleration will not account well for both ancient and medieval observations. For -300, the accelerations implicit in their studies, assuming $\dot{\eta}_m = -25''$, are:

$$
\begin{array}{lcc}
 & \text{Stephenson/Morrison} & \text{Newton} \\
S_s = & 1.26'' & 1.32'' \\
S'_m = & 4.33'' & 5.13'' \\
S'_D = & 3.07'' & 3.81''.
\end{array}
\tag{18}
$$

Investigator	S_s	S'_m	S'_D	$\dot{\eta}_m$	Epochs
van der Waerden [1961]	1.31″	6.28″	4.97″	−22.4″	−380/1780
Muller/Stephenson [1975]	1.88	6.32	4.44	−37.6	0/1770
S. Jones [1939]	1.23	[5.22]	3.99	−22.4	−200/1780
Newton [1985][a]	1.32	5.13	3.81	[-25.0]	−300/1790
de Sitter [1927]	1.80	5.22	3.42	−37.7	−200/1833
Schoch [1926]	1.51	5.04	3.53	−30.3	−200/1800
Fotheringham [1920b]	1.50	4.75	3.25	−30.6	−250/1800
Stephenson/Morrison [1984][a]	1.26	4.33	3.07	[-25.0]	−300/1900
Newcomb [1912][a]	[1.23]	[3.97]	2.75	[-25.0]	−300/1800
Curott [1966]	1.06	[3.62]	2.56	−21.1	≈ 0/1780
S. Jones (revised)[b]	1.11	[3.62	2.51	−22.4	−200/1780
de Sitter (revised)[c]	1.14	3.62	2.48	−23.2	−200/1833
Morrison/Ward [1975]	[1.23]	3.45	2.22	−26.0	1677/1973
Newton [1970]	1.79	3.13	1.34	−41.6	−200/1900
Brouwer [1952]	1.01	2.22	1.21	−20.5	−300/1900

[a] Calculated from $\dot{\eta}_m$ and S'_D. [b] Cf. 171, above. [c] Cf. 169, above.

Table A1.2. Summary of Recent Determinations of the Accelerations of the Sun and Moon

The results of the investigations discussed above, beginning with Newcomb [1912], are summarized in Table A1.2. Since, for ancient observations, the acceleration in elongation (S'_D) is the best determined parameter, the findings are listed in order of S_D'. Parameters which are assumed from other studies and not independently derived are shown in [ ]. More than half ($^7/_{13}$) the results give values for S_D between ≈ 2.5″ and 3.5″, with the values of S_s falling between roughly 1.1″ and 1.5″. At present, the best estimates of the (average) accelerations for −300/1900 seem to be:

$$
\begin{aligned}
S_s &= 1.15'' \pm 0.15'' \\
S'_D &= 2.85'' \pm 0.5'' \\
S'_m &= 4.00'' \pm 0.6'' \\
\dot{\eta}_m &= 25'' \pm 2''.
\end{aligned}
\tag{19}
$$

These are very close to Stephenson and Morrison's implicit findings [1984] and to Newcomb's results [1912] when adjusted for the Sun's acceleration.

Elements of the Sun and Moon used in this work

When this study was first completed, the accelerations which seemed to fit the ancient and medieval data best were:

$$\begin{aligned} S_s &= 1.0'' \\ S'_m &= 3.62'' \quad (S_m = 9.76'') \\ S'_D &= 2.62'' \quad (S_D = 8.67''), \end{aligned} \tag{20}$$

and these parameters were adopted in this work. Recently, the combination of better modern techniques for estimating $\dot{\eta}_m$ and the use of more extensive Babylonian data in estimating S_s have suggested that slightly higher accelerations may in fact apply. These would affect the calculated times of lunar phenomena reported by Ptolemy by no more than 10 minutes. In view of the uncertainties which still attend the values of these parameters, I have left unchanged the parameters originally adopted.

The following table shows the corrections to the *computed* times of the solar (Δt_s) and lunar (Δt_D and Δt_m), phenomena which would result from the use of the accelerations shown in (19) in place of the elements adopted in this work.

Epoch	Δt_s	Δt_D	Δt_m
140	-19^m	-3^m	-4^m
-140	-25	-3	-5
-250	-28	-4	-5
-500	-35	-5	-17
-750	-43	-5	-18

The adopted accelerations differ from those deduced by S. Jones [1939] and included in the elements accepted by the Nautical Almanac Offices by:

$$\begin{aligned} \Delta S_m &= -1.6'' \\ \Delta S_s &= -0.23''. \end{aligned}$$

These corrections should be multiplied by $R = T^2 + 1.33T - 0.26$ to minimize their effect on modern observations. Hence, the total corrections to the expressions for the mean longitudes of the Sun and Moon at 1900 become:

$$\begin{aligned} \Delta L_m &= +0.42'' - 2.13''T - 1.6''T^2 \\ \Delta L_s &= +0.06'' - 0.31''T - 0.23''T^2. \end{aligned}$$

Applying these to the elements used by the Nautical Almanac Offices [1961, 98, 107], expressed in terms of Universal Time,[9] we obtain for 1900.0:

$$L_m = 270;26,16.78^\circ + 1336^r306;53,36.89^\circ T + 10.76'' T^2$$
$$L_s = 279;41,49.10^\circ + 100^r0;46,10.80^\circ T + 2.09'' T^2$$
$$D = 350;44,27.68^\circ + 1236^r307;7,26.09^\circ T + 8.67'' T^2.$$

In this work, the longitudes of the Moon's perigee and node are from the expressions derived by Brown [1915] and used by the Nautical Almanac Offices [1961, 107]. For reference, these are (1900.0):

$$P_m = 334;19,46.40^\circ + 11^r109;2,2.52^\circ T - 37.12'' T^2$$
$$N_m = 259;10,59.79^\circ - 5^r134;8,31.23^\circ T + 7.48'' T^2.$$

[9] The elements stated in Nautical Almanac Offices [1961, 98, 107] are for Ephemeris Time. To obtain expressions for the elements for Universal Time the following corrections must be applied:

$$\Delta L_m = +4.65'' + 12.96'' T + 5.22'' T^2 + B$$
$$\Delta' L_s = +1.00'' + 2.97'' T + 1.23'' T^2 + 0.0747B,$$

where B is the value of the Moon's fluctuation. In the present study, B has been neglected because its magnitude at ancient epochs is unknown.

APPENDIX 2

Corrections to Earlier Elements

Previous investigations of Ptolemaic observations have been based on different lunar elements than those derived in appendix 1, as have the most convenient tables for computing the circumstances of solar and lunar phenomena in antiquity. Therefore, it seems desirable to present the corrections required to reduce these elements to those adopted in this study.

The most useful tables for computing the positions of the Moon at distant epochs or for determining the circumstances of eclipses are:

(a) P. V. Neugebauer, *Tafeln für Sonne, Planeten und Mond nebst Tafel der Mondphasen: Tafeln zur astronomische Chronologie* [1914]; with corrections based on Schoch's elements [P. V. Neugebauer 1929, i 35; ii Table E_1];

(b) P. V. Neugebauer, 'Spezieller Kanon der Mondfinsternisse für Vorderasien und Ägypten von 3450 bis 1 v. Chr'. [1934];

(c) T. R. Oppolzer, *Canon of Eclipses* [1962, first published in 1887]; and

(d) B. Tuckerman, *Planetary, Lunar and Solar Positions −600 to +1649* [1962–1964].

Of these, P. V. Neugebauer's tables and Tuckerman's computed positions are based on Schoch's corrected elements as given in P. V. Neugebauer 1929, i 35. Oppolzer's *Canon* [1962], on the other hand, is based on the elements of Hansen and Leverrier, to which Oppolzer applies an empirical correction.

For January 0, 1900 Schoch's elements [1926] are:

$$L_m = 270;26,16.65^\circ + 1336^r 307;53,39.52^\circ T + 12.22'' T^2$$
$$P_m = 334;19,45.94^\circ + 11^r 109;1,58.50^\circ T - 37.12'' T^2$$
$$N_m = 259;10,58.80^\circ - 5^r 134;8,25.90^\circ T + 7.51'' T^2$$
$$D = 350;44,28.41^\circ + 1236^r 307;7,29.95^\circ T + 9.62'' T^2$$
$$L_s = 279;41,48.24^\circ + 100^r 10;46,9.57^\circ T + 2.600'' T^2.$$

Comparing these with the elements shown in appendix 1, we obtain:

$$\Delta L_m = +0.13'' - 2.63''T - 1.46''T^2$$
$$\Delta P_m = +0.46'' + 3.02''T + 0.00''T^2$$
$$\Delta N_m = -0.01'' - 5.33''T - 0.03''T^2$$
$$\Delta D = -0.73'' - 3.86''T - 0.95''T^2$$
$$\Delta L_s = +0.86'' + 1.23''T - 0.51''T^2.$$

These corrections computed for ancient and early medieval epochs are given in Table A2.1, where Δt is the correction to the time at which the body in question would reach a given mean longitude or elongation according to Schoch's elements.

T	ΔL_m	Δt_m	ΔD	Δt_D	ΔL_s	Δt_s
−10 (900)	-120^{m}	$+3.6^{\mathrm{m}}$	-58^{m}	$+1.9^{\mathrm{m}}$	-62^{m}	$+24.8^{\mathrm{m}}$
−15 (400)	−289	+8.8	−162	+5.5	−122	+48.8
−20 (−100)	−532	+16.1	−304	+10.0	−22	+91.2
−25 (−600)	−845	+25.6	−497	+16.3	−348	+139.0

Table A2.1. Corrections to Schoch's Elements

To compute the difference in time of a given event such as an eclipse or occultation, the appropriate ΔL should be divided by the *actual* velocity.

In using tables depending on Schoch's elements, only the corrections to the mean longitude and elongation need be considered. This is because the differences between the motions of the perigee and node according to Brown and Schoch would not produce sensible effects in the position of the Moon except at very far distant epochs, and also because the difference is within the probable error assigned to these motions by Brown [1915, 514–515].

This is not true of the elements on which Oppolzer's *Canon* are based. For those, we must consider the corrections not only to the mean motions on longitude and elongation, but also to the principal arguments of latitude and anomaly. Using Newcomb's comparison [1912, 238] of Oppolzer's corrected elements and comparing them with Schoch's for 1900, we find:

$$\Delta L_m = +0.68'' - 6.36''T - 2.42''T^2$$
$$\Delta P_m = +12.56'' + 20.86''T + 9.82''T^2$$
$$\Delta N_m = -71.32'' - 71.03''T - 2.07''T^2$$
$$\Delta D = +0.84'' + 5.66''T + 0.93''T^2.$$

Applying the correction to Schoch found above, we obtain:

$$\begin{aligned}\Delta L_m &= +0.81'' + 3.73''T + 0.96''T^2\\ \Delta D &= +0.11'' + 2.50''T - 0.02''T^2\\ \Delta \bar{a} = \Delta L - \Delta P &= -11.75'' - 17.13''T - 8.86''T^2\\ \Delta F = \Delta L - \Delta N &= +72.16'' + 74.76''T + 3.03''T^2.\end{aligned}$$

These corrections are tabulated in Table A2.2.

T		ΔD	Δt_D	$\Delta \bar{a}$	ΔF
−10	(900)	−27″	+0.9^m	−727″	−373″
−15	(400)	−42	+1.4	−1750	−367
−20	(−100)	−58	+1.9	−3210	−222
−25	(−600)	−75	+2.5	−5716	+97

Table A2.2. Corrections to Oppolzer's Elements

The correction to Oppolzer's elongation is negligible, and the correction in the argument of latitude will yield an error of less than 0.2^d in computed eclipse-magnitudes at Ptolemy's time. The error in the argument of anomaly, however, can affect the time of conjunction at −100 by nearly 10 minutes.

Finally, since it is sometimes convenient to refer to Newcomb's computations [1878], which are based on Hansen's tables, the following corrections to Hansen's elements are consistent with those described above.

$$\begin{aligned}\Delta D &= -29.76'' - 31.24''T - 2.6''T^2\\ \Delta \bar{a} &= -37.10'' - 41.80''T - 0.66''T^2\\ \Delta F &= -37.47'' - 38.54''T - 0.87''T^2.\end{aligned}$$

The corrections to each of these elements for specific dates are given in Table A2.3.

T		ΔD	Δt_D	$\Delta \bar{a}$	ΔF
−17	(200)	−215″	+7.1^m	+478″	+393″
−18	(100)	−275	+9.1	+496	+406
−20	(−100)	−391	+13.1	+527	+409

Table A2.3. Corrections to Hansen's Elements

The error in the time of an eclipse due to the error in Hansen's anomaly is less than ±2 minutes for the period over which Ptolemy reports observations. The error in an eclipse-magnitude can reach nearly 0.5^d.

APPENDIX 3

The Inequality in Eccentric Motion

The expression for the inequality in eccentric motion is frequently stated [cf. Tannery 1893, 168 f.], but its development is seldom shown. If we ignore powers of e' greater than 4, the inequality is developed most simply in this way:

$$\begin{aligned}\tan g' &= \frac{e' \sin \bar{a}'}{1 + e' \cos \bar{a}'}, e' \ll 1 \\ &= e' \sin \bar{a}'(1 - e' \cos \bar{a}' + e'^2 \cos^2 \bar{a}' - e'^3 \cos^3 \bar{a}' \ldots) \\ &= (e' + \frac{1}{4}e'^3) \sin \bar{a}' - (\frac{1}{2}e'^2 + \frac{1}{4}e'^4) \sin 2\bar{a}' + \frac{1}{4}e'^3 \sin 3\bar{a}' \\ &\quad - \frac{1}{8}e'^4 \sin 4\bar{a}' \ldots\end{aligned} \tag{1}$$

If we neglect powers of g' greater than 4, we may put,

$$g' = \tan g' - \frac{1}{3}g'^3 = \tan g' - \frac{1}{3}\tan^3 g'. \tag{2}$$

From (1), we obtain,

$$\frac{1}{3}\tan^3 g' = \frac{1}{4}e'^3 \sin \bar{a}' - \frac{1}{12}e'^3 \sin 3\bar{a}' - \frac{1}{4}e'^4 \sin 2\bar{a}' + \frac{1}{8}e'^4 \sin 4\bar{a}' + \ldots \tag{3}$$

Thus, from (1), (2), and (3), it follows that

$$g' = e' \sin \bar{a}' - \frac{1}{2}e'^2 \sin 2\bar{a}' + \frac{1}{3}e'^3 \sin 3\bar{a}' - \frac{1}{4}e'^4 \sin 4\bar{a}' \ldots \tag{4}$$

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−762	Jun 15	solar eclipse	162
−720	Mar 19	lunar eclipse	53–56, 72–73, 75, 156
−719	Mar 8	lunar eclipse	51, 53, 56–57, 73, 75, 77, 138
	Sep 1	lunar eclipse	49, 53, 57, 72–75, 77
−647	Apr 6	solar eclipse	162
−620	Sep 22	lunar eclipse	53, 58, 72–73, 75, 77
−584	May 28	solar eclipse	161
−556	May 19	solar eclipse	161
−522	Dec 16	lunar eclipse	51, 53, 58–60, 73–75, 77
−521	Jun 10	lunar eclipse	59–60
−501	Nov19/20	lunar eclipse	50–51, 53, 60, 72–75, 77, 138
−490	Apr 25	lunar eclipse	51, 53, 61, 72–73, 75, 77, 135
−431	Jun 27	summer solstice	12, 18, 78
−430	Aug 3	solar eclipse	162
−424	Oct 9	lunar eclipse	166
−382	Dec 23	lunar eclipse	38n28, 53, 61–63, 72–75, 77, 156
−381	Jun 18	lunar eclipse	38n28, 53, 63–64, 72–75
	Dec 12	lunar eclipse	38n28, 51, 53, 64–65, 72–75
−309	Aug 15	solar eclipse	161–162
−294	Dec 21	occult., β Sco	79, 80–82, 94, 97
−293	Mar 9	occult., Spica	79, 82–83, 94, 97
−282	Jan 29	occult., Pleiades	79, 82, 84–85, 94, 97
	Nov 8	occult., Spica	49, 79, 86–88, 94, 97, 166, 172
−279	Jun 26	summer solstice	12, 13n1, 18
−200	Sep 22	lunar eclipse	38n28, 53, 65, 73, 75, 156
−199	Mar 19	lunar eclipse	38n28, 53, 66, 71, 73, 75
	Sep 12	lunar eclipse	38n28, 53, 66, 71, 73, 75
−173	May 1	lunar eclipse	53, 68, 72–73, 75, 77
−161	Sep 27	fall equinox	13n1, 14n2, 18, 20–22, 40
−158	Sep 27	fall equinox	13n1, 14n4, 18, 20–22, 40
−157	Sep 27	fall equinox	13n1, 14n4, 18, 20–22, 40

−146	Sep 26/27	fall equinox	18, 20–21, 22n16, 33, 36, 39–40
−145	Mar 24	spring equinox	14, 16, 18, 20, 25n20, 33, 36, 40
		(Rhodes)	18, 20, 25n20, 33, 36, 40
		(Alexandria)	18, 20n12, 25n20
	Apr 21	lunar eclipse	23n18
	Sep 27	fall equinox	13n1, 18, 20, 33, 39, 40
−144	Mar 23	spring equinox	18, 20
−143	Mar 23	spring equinox	18, 20
−142	Mar 23	spring equinox	18, 20
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−132	Mar 23	spring equinox	18, 20
−131	Mar 23	spring equinox	18, 20
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−129	Mar 24	spring equinox	18, 20
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−127	Mar 23	spring equinox	13n1, 18, 20, 40, 163
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−126	May 2	lunar elongation	38, 103, 119, 121
	Jul 7	lunar elongation	38, 103–104, 110, 121
+92	Nov 29	occult., Pleiades	79, 88–89, 94, 97
+98	Jan 11	occult., Spica	79, 90–1, 94, 97
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+127	Mar 26	Saturn, elong.	37n27, 39, 51n7
+130	Dec 15	Mars, elong.	37n27
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	Sep 25	fall equinox	13, 18, 20, 31, 35–36, 40
+133	May 6	lunar eclipse	53, 70, 73, 75
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+139 Sep 26	fall equinox	13, 18, 20, 31, 35–36, 40
+140 Mar 22	spring equinox	13, 18, 20, 31, 35–36, 40
Jun 25	summer solstice	13, 18, 40
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2. DATED OBSERVATIONS BY TYPE AND DATE

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−158 Sep 27	fall	13n1, 14n4, 18, 20–22, 40
−157 Sep 27	fall	13n1, 14n4, 18, 20–22, 40
−146 Sep 26/27	fall	18, 20–21, 22n16, 33, 36, 39–40
−145 Mar 24	spring	14, 16, 18, 20, 25n20, 33, 36, 40
	(Rhodes)	18, 20, 25n20, 33, 36, 40
	(Alexandria)	18, 20n12, 25n20
Sep 27	fall	13n1, 18, 20, 33, 39, 40
−144 Mar 23	spring	18, 20
−143 Mar 23	spring	18, 20
−142 Mar 23	spring	18, 20
Sep 26	fall	13n1, 14n2, 18, 20–22, 40
−141 Mar 24	spring	18, 20
−134 Mar 23/24	spring	13n1, 18, 20, 40, 164
−133 Mar 24	spring	18, 20
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+139 Sep 26	fall	13, 18, 20, 31, 35–36, 40
+140 Mar 22	spring	13, 18, 20, 31, 35–36, 40

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+140 Jun 25	summer	13, 18, 40

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−762 Jun 15	(Nineveh)	162

−647 Apr 6	(Archilochus)	162
−584 May 28	(Thales)	161
−556 May 19	(Larissa)	161
−430 Aug 3	(Thucydides)	162
−309 Aug 15	(Agathocles)	161–162
−128 Nov 20	(Hipparchus)	165–166
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−719 Mar 8	Babylon	51, 53, 56–57, 73, 75, 77, 138
Sep 1	Babylon	49, 53, 57, 72–75, 77
−620 Sep 22	Babylon	53, 58, 72–73, 75, 77
−522 Dec 16	Babylon	51, 53, 58–60, 73–75, 77
−521 Jun 10	Babylon	59–60
−501 Nov19/20	Babylon	50–51, 53, 60, 72–75, 77, 138
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−382 Dec 23	Babylon	38n28, 53, 61–63, 72–75, 77, 156
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Dec 12	Babylon	38n28, 51, 53, 64–65, 72–75
−200 Sep 22	Alexandria	38n28, 53, 65, 73, 75, 156
−199 Mar 19	Alexandria	38n28, 53, 66, 71, 73, 75
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−145 Apr 21	Rhodes	23n18
−140 Jan 27	Rhodes	53, 68–69, 72–75, 77
−134 Mar 21	Rhodes	23n18
+125 Apr 5	Alexandria	51, 53
+133 May 6	Alexandria	53, 70, 73, 75
+134 Oct 20	Alexandria	53, 70–71, 73, 75, 77
+136 Mar 6	Alexandria	53, 71–73, 75, 77

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+138 Dec 16	104–108, 119
+138 Dec 22	109–111, 119, 121
+139 Feb 9	109, 112, 119, 121
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−293 Mar 9	Spica	79, 82–83, 94, 97
−282 Jan 29	Pleiades	79, 82, 84–85, 94, 97
Nov 8	Spica	49, 79, 86–88, 94, 97, 166, 172
+92 Nov 29	Pleiades	79, 88–89, 94, 97
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