Limits of observation and pseudoempirical arguments in Ptolemy's *Harmonics* and *Almagest*

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Introduction

Ptolemy's surviving writings can be divided into two categories: treatises covering a branch of scientific knowledge in a systematic and comprehensive manner, and works that address narrower problems and questions. The general treatises are the *Almagest* (astronomy), the *Tetrabiblos* (astrology), the *Harmonics* (musical pitch relations and systems), and the *Optics* (visual perception). Of these four works, the *Almagest* and *Harmonics*, and also to a large extent the *Optics*, are concerned with fields that Ptolemy regards as mathematics, that is, the study of "shape, number, size, and moreover place, time, and the like" (*Almagest* 1.1), whereas the *Tetrabiblos* concerns physics, the study of qualitative properties of matter and change.[1] This ontologically-based distinction with respect to the brances of theoretical philosophy is paralleled by a distinction in the epistemological approach of the treatises: while the *Tetrabiblos* operates primarily with an interplay between the received tradition of astrological doctrine and aprioristic reasoning leading to inexact knowledge, the "mathematical" works aim at exact knowledge by an interplay between sense perception and analytical reasoning.

In the opening chapter of the *Harmonics* Ptolemy tells us that reason and the sensory faculty of hearing are the two "criteria" (i.e. faculties for determining truth) in harmonic science, and outlines how the interplay between them is supposed to work. If we substitute vision for hearing as the sensory criterion appropriate for astronomy and optics, the demonstrative structures of all three "mathematical" treatises largely follow the strategy of *Harmonics* 1.1. The process begins with sense perception, and specifically with certain rather crude but indisputable perceptions that provide reason with the starting points for developing a theory and designing more sophisticated observational procedures and instruments for refining that theory. The back-and-forth between progressively narrower observations and progressively deeper analysis may be repeated until a theory or model is reached whose agreement with observations is within the limits of accuracy of the relevant senses. As Ptolemy asserts in an astronomical context (*Almagest* 9.2), the standard of theory verification is that one should be able to "fit [ἐφαρμόσαι] pretty well all the phenomena" to the model, that is, not only observed data that went into the model's deduction but also any other observed data that one may possess.

Hence in principle every theoretical outcome in Ptolemy's mathematical sciences ought to rest on empirical evidence, and to a great extent the "narrative" of his treatises—the linear flow of evidence and argument that one encounters by reading the works from start to finish—conforms to this expectation. As Swerdlow has pointed out, in the *Almagest* Ptolemy provides his

1 In accordance with the Aristotelian theory of "proper" and "common" sensibles, the *Optics* also deals with the perception of color, the qualitative "physical" property of bodies that is the proper sensible by means of which vision perceives the common sensibles—which more or less coincide with the properties that Ptolemy treats as mathematical.

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empirical evidence in two distinct manners.[2] Considerations that lead to conclusions about the general, as yet unquantified structures of his models tend to be presented as bald assertions of a general phenomenon relating to the apparent behaviour of the relevant heavenly bodies, without explicit reference to specific observations or observational procedures; these correspond to the crude sensory perceptions of the *Harmonics*'s epistemological strategy. But finer details and in particular quantifications of the models are justified on the basis of observations that are described in greater detail, often with specific dates and, where pertinent, indications of the instruments that were employed; these correspond in the *Harmonics*'s account to the more refined sensory perceptions that have been guided by reason. Both types of presentation of empirical evidence are present in the *Harmonics* and *Optics* too, though where in the *Almagest* the second type consists of reports of actual observations (whether expressly dated or not) that are supposed to have been made in the past by Ptolemy or his predecessors, in the *Harmonics* and *Optics* they are descriptions of demonstrations that the reader is invited to recreate, though presumably Ptolemy means us to suppose that he has also tried them out. One can also add a third type of empirical appeal: implied invitations to the reader to compare predictions derived from the quantitative models with the established phenomena. In the *Almagest*, parts of the eclipse theory in Book 6 and the sections concerning planetary stations and visibility conditions in Books 12-13 have this function of implicit model verification—implicit because Ptolemy usually does not say outright that his predictions agree with the phenomena.[3]

Data derived from the senses play a predominant role in controlling the evolution of Ptolemy's mathematical models, but do not suffice to determine them. An obvious, if usually unspoken, consideration in every decision Ptolemy makes about his models is simplicity. In discussions of astronomy in particular it was a commonplace that a multiplicity of models could be devised that were equally in agreement with the phenomena.[4] Ptolemy's response is that "generally we consider that it is appropriate to demonstrate the phenomena through simpler models, so far as this is possible, insofar as nothing significant [ἀξιόλογον] in opposition to such a proposal is apparent from the observations" (*Almagest* 3.1).

The qualification "significant" shows that in principle Ptolemy conceded that a simpler model should sometimes be preferred even if it did not fit the empirical data as closely as a more complicated model, especially if the discrepancies could be plausibly explained as arising from observational or computational errors. Yet he was generally reluctant to allow a simplicity argument to override observation, as we can see in a well known passage in *Almagest* 13.2, where he defends the staggering complexity of his models for the latitudinal motion of the planets on the grounds that the standards of simplicity for heavenly bodies cannot be learned from comparisons with the mundane constructions to which we have more direct access. His explicit invocations of simplicity in choosing between model options are rare, and typically in situations such as the question whether to explain an anomaly by means of an epicycle or an eccenter, where two model structures are kinematically identical, not just indistinguishable to the senses.

2 Swerdlow 2004, 249-250.

3 The confrontation of prediction and phenomena is explicit, however, in *Almagest* 13.8, where Ptolemy takes up the "strange" (ξενίζοντα) visibility phenomena of Venus (extreme variation of intervals of invisibility at inferior conjunction) and Mercury ("missed" appearances in the alternation of morning and evening visibility). See also 3.1 where Ptolemy adduces the accuracy of predictions of eclipse times as evidence that his solar model is correct in assuming a constant tropical year.

4 E.g. Theon of Smyrna, ed. Hiller 166-189 and the passage quoted from Geminus' digest of Posidonius's *Meteorology* quoted by Simplicius (*Commentary on Aristotle's Physics* ed. Diels, *Commentaria in Aristotelem* Graeca 9.291-292).

It is worth asking whether for Ptolemy simplicity is a "criterion" in the technical sense of that term in Hellenistic philosophy, that is, a standard guiding us towards knowledge of the reality underlying the phenomena, or whether it is merely a basis for selecting from among several hypotheses, any of which could equally be true, the one that is easiest to comprehend or most convenient. He does regard computational simplicity as a merit justifying the omission or approximation of certain theoretically necessary elements in methods of predicting phenomena, when the effects of these shortcuts are below the threshold of observational precision (*Almagest* 6.7). In the introductory section of the *Planetary Hypotheses* (1.2) he writes, rather obscurely, that in describing the models for the motions of the heavenly bodies he will use "the simpler ones among the approaches [ταῖς ἁπλουστέραις τῶν ἀγωγῶν] for the sake of convenience in the construction of mechanical models [πρὸς τὸ εὐμεθόδευτον τῆς ὀργανοποιίας], even if some discrepancy [παραλλαγή] ensues," probably referring to the models for planetary latitude which are much simpler than those of the *Almagest*.[5] However, later in the same work (2.6) he writes that one should not suppose that anything pointless and useless exists in nature, while the passage in *Almagest* 13.2 already referred to, while asserting that simplicity is not a trivial thing for human beings to appraise in heavenly bodies, nevertheless implies that it *is* an attribute of their motions. It is thus clear that simplicity is one manifestation of Ptolemy's Platonizing belief (*Harmonics* 1.2) that "the works of nature are crafted [δημιουργούμενα] with a certain reason and ordered cause, and nothing is brought about without plan or at random."

Another important class of argument that rests on the presumed orderliness of the cosmos is appeal to analogy; as Ptolemy succinctly expresses the principle in *Planetary Hypotheses* 1.2, "the most wondrous nature portions out very like things [τὰ παραπλήσια] to similar things," that is, entities that resemble each other in certain essential respects are naturally endowed with other similar characteristics. One might describe this principle as a simplicity argument applied to the cosmos as a whole. Explicit analogical argument is rare in the *Almagest*, but conspicuous in the *Harmonics* (as well as in the more speculative cosmology of the *Planetary Hypotheses*). One form it can take is the extension of attributes that have been deduced for one entity to other entities that are regarded as of the same kind; for example, the empirically deduced spherical shape of the Sun and Moon can be presumed to apply also to the invisible etherial bodies composing the heavens (*Almagest* 1.3). Another form is to infer from a numerical correspondence between two sets of entities that they can be paired off one-to-one in a meaningful way; this kind of argument comes into its own extravagantly in *Harmonics* Book 3 where the theoretical structures based on ratios of whole numbers that Ptolemy has deduced for musical pitch systems earlier in the treatise are assigned putative analogues in various aspects of the human soul and the heavens. Analogical argument can also be applied inversely to infer a kinship between entities on the basis of their having similar attributes, as when Ptolemy adduces the structural similarities among the models for certain subsets among the heavenly bodies as evidence that the models are spatially contiguous (*Almagest* 9.1 and *Planetary Hypotheses* 1B.3[6]). Like simplicity arguments, arguments

5 Aside from the planetary latitude models, the only structural difference between the *Almagest* models and those of the *Planetary Hypotheses* is Ptolemy's abandonment of the special definition of the apogee of the Moon's epicycle from *Almagest* 5.5. If Ptolemy has the latitude models in mind in *Planetary Hypotheses* 1.2, his remarks would appear to cast doubt on whether he believed that the simpler models of the *Planetary Hypotheses* were true representations of the way that the planets move, though they are in fact more accurate as geocentric transformations of the inclined heliocentric orbits of the planets than Ptolemy's earlier models; see Swerdlow 2005.

6 Goldstein 1967, 7. The two arguments are distinct: in *Almagest* 9.1 Ptolemy uses the criterion of restricted/unrestricted possible elongation from the Sun to divide the planets into two groups spatially separated by the Sun's sphere, while in *Planetary Hypotheses* 1B.3 he invokes the structural similarity between the Moon's and Mercury's

based on analogy seem to have the status of weak or probabilistic criteria in Ptolemy's epistemology. They are invoked only when empirical arguments are not available, and often with a qualification that the argument has the force of likelihood, not certainty.

Ptolemy seldom appeals to the authority of his predecessors, if we except the observation reports from the past that he necessarily accepts as givens in the *Almagest*; and when it happens, this kind of appeal is normally offered as a supplement to an empirical argument. Thus in *Almagest* 3.1 he adduces several passages from Hipparchus's works as supporting his value for the length of the tropical year, though his primary justification for that parameter comes from the comparisons of observed solstice and equinox dates in that chapter; 7.1-3 similarly draw on Hipparchus's authority as a secondary support for the elements of his precession theory. Previously, in 1.12, he has pointed out that his value for the obliquity of the ecliptic, again ostensibly derived from his own observations, is in agreement with values accepted by Eratosthenes and Hipparchus. On the other hand, in 9.1, without providing any additional rationale, he accepts the consensus of "pretty well all the leading mathematicians [σχεδὸν παρὰ πᾶσι τοῖς πρώτοις μαθηματικοῖς]" that the outermost spheres of the heavenly bodies in the cosmos are, in order of progressive proximity to the Earth, those of the fixed stars, Saturn, Jupiter, and Mars, while the Moon's sphere is closest to the Earth; but this seems to be a unique instance of Ptolemy's acquiescing in insupported authority, and the matters at issue do not come into play in the subsequent logical argument of the *Almagest*.

Thus the narratives of Ptolemy's mathematical works present the reader with a series of rational decisions determining, refining, and quantifying the models under investigation, where each decision is based on empirical evidence, or if pertinent empirical evidence is not available, on less reliable principles reflecting the presumed orderliness of nature. This appearance, however, is partly deceptive, because Ptolemy sometimes presents ostensibly empirical evidence that, on closer examination, could not be observed. These claims, which I will call pseudoempirical, are statements concerning things that are observable, such as musical pitches and apparent positions and speeds of heavenly bodies, but what is said about them could not have been confirmed or refuted under ancient observing conditions because the claimed behaviors are smaller than the limits of observational accuracy, and one can be fairly sure that Ptolemy knew this. Pseudoempirical claims really are untestable predictions of phenomena derived from the very models for which they are supposed to provide evidence, and as such they mask gaps in the deductive completeness of Ptolemy's treatises.

My primary purpose in this paper is to show that pseudoempirical claims are present in the *Harmonics* and, with some frequency, in the *Almagest*. (I am not aware of instances in the *Optics*.) Secondarily, I wish to raise the question of why Ptolemy sometimes chooses to adduce pseudoempirical claims instead of either genuine empirical evidence or metaphysical considerations such as simplicity or analogy. I begin with a case that arises in the *Harmonics*, because in that work Ptolemy discusses in some depth the limits of observational precision and their consequences for the appropriate use of the evidence of the senses. My other examples are from the *Almagest*, and are presented in the order that they appear in that treatise; but they also represent a progression of increasing complexity, leading to cases whose status as pseudoempirical is difficult to assess with certainty.

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rapidly revolving eccenters as grounds for believing that Mercury's model is contiguous with the Moon's.

The Harmonics and the limits of auditory perception

The premise of the *Harmonics* is that the systems of intervals between the tuned pitches employed by the musicians of Ptolemy's culture were mathematical entities whose structures and properties could be described and explained by ὑποθέσεις ("hypotheses" or "models") built up out of ratios of whole numbers, just as astronomical phenomena are explained by ὑποθέσεις built up out of uniform circular revolutions. The quantitative and relational character of pitch intervals, Ptolemy argues, is inferred from the primitive observation that the pitches produced by a sounding object are dependent on quantitative elements such as the object's size and density and the distance between the point of origin of movement and the place where the air is struck and incited into motion. The effects of these quantitative elements must unite into a single, more abstract quantitative attribute called τάσις ("tenseness") that originates in the sounding body and spreads outward through the air, where it is manifested as sound. Only discrete intervals between sounds that maintain a fixed pitch are mathematically tractable, and they obviously have the character of relations, i.e. ratios, between magnitudes.

Our sense of hearing, in Ptolemy's view, is inherently inexact, but it is more reliable in certain kinds of responses and assessments than in others. At the high end of reliability are its recognition of certain pitch intervals as ὁμόφωνοι ("same-sounding") or σύμφωνοι ("together-sounding"), meaning that the pitches, though distinct, sound somehow the same or very nearly. The octave is an example of a "same-sounding" interval, and the fifth and fourth are examples of "together-sounding" intervals. Our auditory recognition of such special intervals is an elementary response to a pair of heard pitches that are approximately in a particular relation to each other, something more like a resonance than a measurement; Ptolemy gives an analogy of our visual recognition of circularity in an approximate circle, even if it is drawn freehand, which does not depend on a measurement of radii. Secondly, our hearing can reliably judge which of two heard pitches is higher so long as the interval between them is not smaller than some threshold. On the other hand, we cannot trust our hearing to correctly measure intervals or to compare the sizes of two heard intervals that are close to the same size and that do not have either their higher or their lower pitches in common.

Ptolemy's methodology for acquiring scientific knowledge involves iterative, alternating appeal to sense perception and reason to refine each other's contribution. Having arrived at a basic theoretical framework for harmonics, namely the modelling of pitch intervals as whole num-

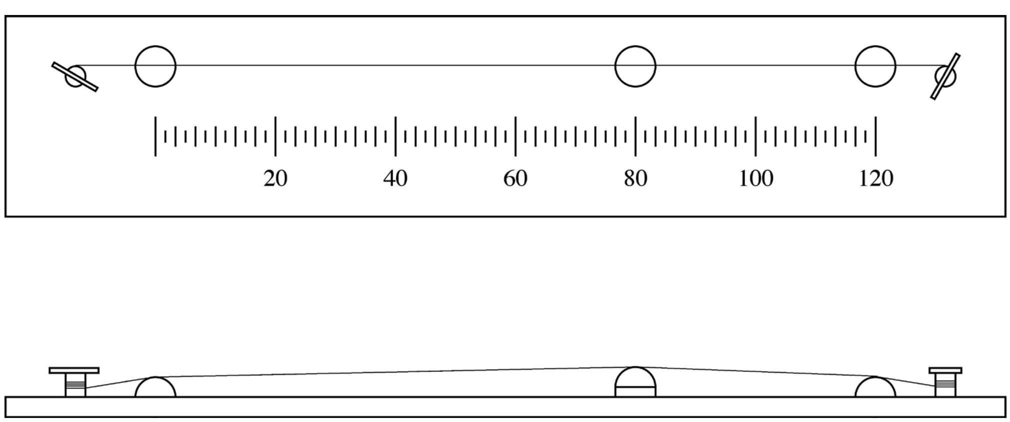

Figure 1. Ptolemy's monochord, top and side views. The bridges have spherical surfaces of equal radius. The middle bridge is movable and slightly raised relative to the fixed bridges; here it is set to produce a 2 : 1 ratio between the two parts of the string, which will be heard as an octave. The scale is divided linearly into 120 parts.

ber ratios, reason can direct the next stage of sensory investigation by devising observational techniques and instruments that exploit the model to enable one's hearing to make more exact perceptions, on the basis of which reason will take the modelling to the next stage. The instrument that Ptolemy advocates as particularly suited to harmonic research is the κανών, actually a class of instruments based on tensed strings divided by bridges into measured lengths, which produce pitches when struck or plucked. The κανών effectively reduces the sounding body to a single, easily controlled variable magnitude that has a simple and direct relation to the τάσις and hence to the perceived pitch. The simplest form of κανών is a monochord, which comprises a single string between two fixed bridges, and a movable bridge that divides the string into equally tensed parts whose lengths and ratio can be measured by a ruler running along the string's length (Fig.1).

Ptolemy employs the monochord only for the most basic demonstrations, while for more advanced work he prefers a κανών consisting of eight independent monochords. With the single monochord, we are merely to divide the string into simple whole number ratios for which we have a theoretical expectation that they correspond to "same-sounding" and "together-sounding" intervals, and verify that the pitches made by each part of the divided string produce the expected sensory response. Thus dividing the string into a 2 : 1 ratio results in a "same-sounding" octave, while dividing it into a 3 : 2 or 4 : 3 ratio results respectively in a "together-sounding" fifth or fourth.

Ptolemy also describes another demonstration that he ascribes to the followers of the fourth century B.C. Peripatetic philosopher Aristoxenos, which would most easily be carried out on a multi-string κανών. This demonstration concerns the relationship between the fourth and a smaller interval called the τόνος, which is the interval obtained by tuning up a fifth and then down a fourth from a given pitch, in other words the "difference" between a fourth and a fifth. One begins with two strings *a* and *b*, tuned by ear to sound at the interval of a fourth. String *c* is tuned (always by ear) two τόνοι above *a*, and string *d* is tuned two τόνοι below *b*. Lastly, string *e* is tuned a fourth below *c*, and string *f* is tuned a fourth above *d*. Strings *e* and *f* will ostensibly sound at the interval of a fifth, and from this it can be inferred that a fourth is equal to two and a half τόνοι.[7] A corollary of this result is that an octave consists of exactly six τόνοι.

The conclusion of the Aristoxenian demonstration is inconsistent with the equations in Ptolemy's model of the fourth with the 4 : 3 ratio and the fifth with the 3 : 2 ratio. Assuming Ptolemy's ratios, one obtains the ratio for the interval between *e* and *f* as $2^{18} : 3^{11}$ (i.e. 262144 : 177147), which is obviously not equivalent to a 3 : 2 ratio. Ptolemy has no doubt about which demonstration to trust: our hearing "all but screams out" that the monochord divided into 3 : 2 and 4 : 3 ratios produces the fifth and fourth, whereas the multiplicity of by-ear tunings in the other demonstration gives ample room for small perceptual errors that could cumulatively stretch the final interval into something heard as a fifth. Ptolemy calculates from his model that the interval between pitches a true fourth and two and a half τόνοι above a given pitch would correspond to a ratio of 129 : 128, and he remarks that even the followers of Aristoxenos would not assert that such a tiny interval could be judged by ear.

Though its appearance in a polemical context might lead one to suspect Ptolemy's statement that one cannot discriminate accurately between pitches in a 129 : 128 ratio (i.e. about 0.8%), it is in fact quite reasonable. While under ideal conditions hearing has been shown to capable of dis-

7 Half a τόνος is the interval such that if strings *i* and *j* are a half τόνος apart and strings *j* and *k* are a half τόνος apart, then *i* and *k* are a τόνος apart.

tinguishing pitches differing by as little as 0.4%,[8] 0.8% is about the average threshold of pitch discrimination for non-tone-deaf individuals comparing electronically generated sinusoidal tones of half a second's duration around 500Hz.[9] The threshold for comparisons of rapidly fading tones made by plucked strings would surely be still higher.

To make his point still more explicit, he invites the Aristoxenians to fetch the most skilled musician that they can find, and have him tune a series of seven strings at successive intervals of a τόνος, and he guarantees that the final pitch will not sound as an octave above the first. Hence either six τόνοι do not make an octave or no musician can be relied on to perform perfect tunings, and either way the Aristoxenians will be baffled. By contrast, if the strings are tuned according to calculated 9 : 8 ratios using the ruler (i.e. following the methodology of Ptolemy's simple monochord demonstrations), it will become apparent both visually and aurally that six τόνοι make an interval larger than an octave.

A pseudo-empirical claim about pitches

This statement that the ear cannot be expected to detect discrepancies as small as $^1/_{128}$ between intervals is worth remembering when one comes to *Harmonics* 2.1, which describes an exceptionally elaborate and ingenious demonstration, the capstone of the first major part of the treatise. Ptolemy has up to now been in pursuit of the mathematical rules determining the possible structures of *tetrachords*, sets of four pitches spanning an interval of a fourth, which were the building blocks out of which the various tuning systems (loosely, "scales") of Greek music were built. For our purposes it will not be necessary to review Ptolemy's investigations in detail. It is enough to know that, in accordance with the rules that he devises for the division of the tetrachord, he arrives at a very limited number of possible divisions, just six plus one that he presents as his own invention and one that he regards as a theoretically improper approximation to one of the others.

The purpose of *Harmonics* 2.1 is to show that four of the tetrachord divisions that the musicians of Ptolemy's time employed can be rigorously identified among the set that his theory has generated. The required apparatus consists of an eight-string κανών, treated as two sets of four strings, and a musician capable of making accurate tunings by ear. We are repeatedly asked to have one or the other of the two sets of four strings tuned to a particular tetrachord division from among those familiar to musicians; after the first time this is done, the additional condition is imposed that one of the pitches in the new tetrachord is tuned to match one of the pitches in the tetrachord that is already tuned on the other half of the κανών. Following each tuning operation, the observer compares specific pairs of pitches from among the eight strings, in most instances merely judging which pitch is higher, and inferences are made that ultimately lead to the identification of the four musician's tetrachords among the theoretically prescribed repertoire.

The procedure has more in common procedurally with the Aristoxenian demonstration than with those that Ptolemy has advocated hitherto, since the tunings are to be made by ear, not by calculation and the ruler. We are not asked to carry out long series of cumulative tunings between observations, however, so the effects of sensory imprecision can be expected to be less deleterious. Still, there is a problematic comparison where Ptolemy tells us that the second-lowest

8 Harris 1952.

9 Loui, Alsop, & Schlaug 2009, 10216; for details of the tests see Loui, Guenther, Mathys, & Schlaug 2008, supplementary data. A similar study (with similar results) is reported in Tervaniemi *et al.* 2005, 2-3.

pitch of one tetrachord "will be found to be a little sharper" than the corresponding pitch of the second tetrachord, an observation that provides a necessary step in his deduction of the identity of the latter tetrachord. According to Ptolemy's theoretical model for the tetrachord divisions that he has identified with the two sets tuned on his κανών at this stage, the interval between the two strings under comparison is 5120 : 5103, which is approximately 301 : 300. So if Ptolemy's model is correct, the difference between the pitches, if exactly tuned, would be much smaller than the 129 : 128 difference that, he previously claimed, could not be accurately judged by ear, and in fact it would be below the normal threshold of pitch discrimination. In other words, even if a good musician was doing the tuning, it would be a matter of chance which of the strings would turn out sharper than the other, and in any case they would likely be so close that the observer would have difficulty telling the pitches apart.

Ptolemy was certainly aware of the numbers that his theory predicted for this pair of pitches, though he does not state them in the chapter in question, and he can hardly have failed to realize that they were so close to equality as to make them indistinguishable under experimental conditions.[10] This realization need not have shaken his confidence in his model, but it would have shown him that the elaborate demonstration he devised in *Harmonics* 2.1 was faulty. If he actually performed the demonstration before including it in his treatise, and his musician consistently tuned the two strings in such a way that the first one always sounded slightly but unambiguously sharper than the second, he should have concluded that either the musician was systematically mistuning, rendering this stage of the demonstration worthless, or the model was false. If, on the other hand, in repeated trials the relation of the two pitches was inconsistent or indeterminate, he should have concluded that the model was confirmed up to this point but the demonstration could not proceed making use of this relation as an observed inequality.

The proper way to regard Ptolemy's statement about the perceived sharpness of one pitch relative to the other, I believe, is as a pseudo-empirical fact, a kind of ideal observation that Ptolemy is *sure* would be made if it were not prevented by the limitations of human sense perception and the physical conditions we work within. It plugs a rather small hole in the didactic structure of the *Harmonics*, which is all about the interplay of observation and reasoning, and Ptolemy likely felt that such stopgaps were unavoidable and pardonable if one was proposing to construct large scale mathematical deductions about the perceptible world.

The sphericity of the visible heavenly bodies

Ptolemy establishes the broad cosmological framework of the *Almagest* in a series of chapters, 1.3-8; the theses of these chapters, none of which would have surprised Plato or Aristotle, are founded on arguments that are predominently empirical though some aprioristic considerations also come into play. 1.3 addresses a twofold thesis, that "the heavens are spherical and move spherically [σφαιροειδῶς]."[11] Most of the empirical arguments adduce phenomena from which it can be inferred that, broadly speaking, the visible heavenly bodies all revolve daily on circular paths that are centered on a single axis and lie in planes perpendicular to it, which is effectively what Ptolemy means by "moving spherically." This conclusion is consistent with but does not prove the hypothesis that the heavens are, taken as a whole, spherical. To establish that, Ptole-

10 Proportional string lengths derived from the model are written, as sexagesimal approximations, in the accompanying diagram, though it is not certain whether this is a feature that goes back to Ptolemy or a medieval supplement.

11 The phrase appears at the end of 1.2.

my turns to a mathematical-metaphysical argument (that the heavens, being the largest of all bodies, should have the three-dimensional form that has the greatest volume in proportion to its surface) and two arguments that he calls physical, since they depend on assumptions about the properties of the etherial matter that he assumes the heavens to be composed of. The second physical argument is as follows:

> Nature has formed all mundane and perishable bodies generally from curved but nonhomeomeric shapes, and all those that are in the ether and divine again from (shapes that are) homeomeric and *spherical*—since if they were planar or disk-shaped, a circular shape would not be apparent to all who see (them) from various places of the Earth at the same time—and because of this, it is plausible that the ether surrounding them, being of similar nature, is spherical and travels circularly and uniformly on account of its being homeomeric.

("Homeomeric" here means that all parts of the surface or circumference are geometrically congruent, which is a property of circles, cylinders, and spheres.)

There are two empirical claims here, capped by the argument from analogy that we have already cited in passing. First, we see around us that naturally formed bodies are more or less all rounded, but those in our terrestrial environment are geometrically irregular while the ones in the heavens, by which Ptolemy certainly means the Sun and Moon and perhaps also the planets and stars, are geometrically regular.[12] Specifically, they are seen as having circular outlines, which would be compatible with their being circles or disks seen head-on or spheres seen from any direction. Secondly, their outlines appear circular no matter where we observe them from on the Earth's surface, which rules out their being actual circles or disks since, by a well known optical theorem, a circle is normally seen as oval when seen obliquely.[13]

Just three chapters later, however, Ptolemy demonstrates that the Earth has, to the senses, the relation of a point to the heavens (1.6). In other words, so far as observations are concerned, all points on the Earth's surface can be treated as the same point; hence the empirical argument that the visible heavenly bodies are not circular or disk-shaped has no force. Granted, Ptolemy's arguments pertain only to the distances of the fixed stars and the Sun, and in Book 5 he shows that the Moon is near enough to the Earth to exhibit a significant parallax. But even at its minimum distance according to Ptolemy's lunar model, 33 Earth-radii, the Moon would not be near enough for its outline to be seen from any point on the Earth as noticeably oval, supposing it had a planar circular face—especially since the minimum distance coincides with half-Moon phase.[14] Thus the statement that the heavenly bodies are always seen as circular from every terrestrial vantage point, while a legitimate empirical claim in its own right, becomes pseudoempirical in the context of the argument, since the reader is led to suppose that the theoretically predicted appearance of noncircularity of a flat-faced heavenly body would be perceived.

12 Ptolemy believed that the planets and stars have small but discernible apparent disks, and in *Planetary Hypotheses* 1B.5 (Goldstein 1967, 8) he offers estimates of their diameters.

13 Euclid, *Optics* 34-35 (in the first recension in Heiberg's edition) = 34-36 (in the so-called "Theonine" recension). According to these propositions, the circle's diameters will also be seen as equal if the eye lies at a distance from the circle's center equal to the circle's radius, but this obviously does not apply to the heavenly bodies.

14 Ptolemy *could have* argued for the Moon's sphericity on the basis of the appearance of its phases, just as in 1.4 he could have argued for the Earth's sphericity on the basis of the outline of its shadow on the Moon during lunar eclipses. Why Ptolemy does not mention these well-known arguments is a mystery.

The choice of solar model

Ptolemy introduces the eccenter and epicycle models as general approaches to modelling anomaly in *Almagest* 3.3. The next chapter turns to the specific solar model. Ptolemy writes:

> Now that these things have been set out in advance, there should be a preliminary assumption also about the anomaly apparent with respect to the Sun for the sake of its being single and causing the time from the least motion to the mean to be always greater than that from the mean to the greatest; for we find that this too is concordant with the phenomena. It can be accomplished by means of either of the models under consideration, making the proviso that (it is accomplished) by means of the epicyclic (model) such that the Sun's shifting towards the leading (signs) takes place on the arc of (the epicycle) that is farther from the Earth. But it would be more reasonable [εὐλογώτερον] to apply the eccentric model since it is simpler and accomplished not by two motions but by one.

Unpacking this paragraph, we see that Ptolemy starts out not with two candidate models but with three: the eccenter model, and two epicyclic models differentiated by the direction that the Sun revolves around its epicycle (Figs. 2-3). The process of deciding which one to adopt involves two stages. First, the version of the epicyclic model in which the Sun revolves around its epicycle in the opposite sense to that of the epicycle's revolution around the Earth is chosen over the other version in which the two motions have the same sense. This choice is made on the basis of an empirical claim, that the "phenomena" are concordant with having the time from slowest to mean apparent speed greater than that from mean to greatest, which was shown in 3.3 to be characteristic of the opposite-sense epicyclic model but not of the same-sense model. Secondly, the eccenter model is chosen over the opposite-sense epicycle model, not on empirical grounds, but according to a simplicity argument. An empirical discriminant would not in fact be possible at this stage, because the eccenter model and the opposite-sense epicycle model are (with appropriate choice of parameters) kinematically equivalent, that is, each would place the Sun at exactly the same point in space at any chosen time.

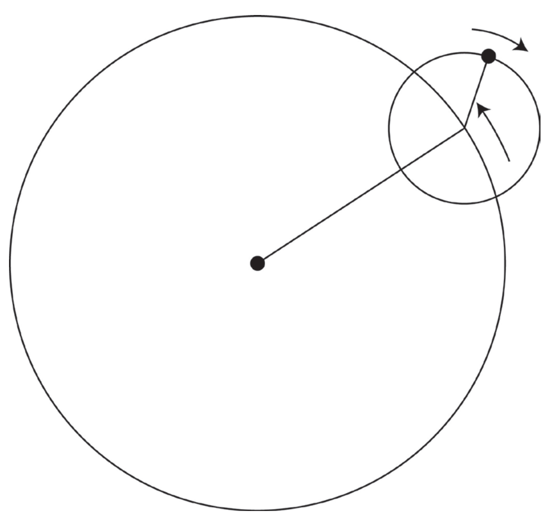

Figure 2. Opposite-sense epicycle model. The revolution of the body on the epicycle is considered in a geocentric frame of reference, i.e. relative to the revolving radius from the Earth to the center of the epicycle. If the two periods of revolution are equal, the radius from the epicycle's center to the body will maintain a fixed direction in a Cartesian frame of reference and the body will trace out an eccentric circular path at uniform actual speed.

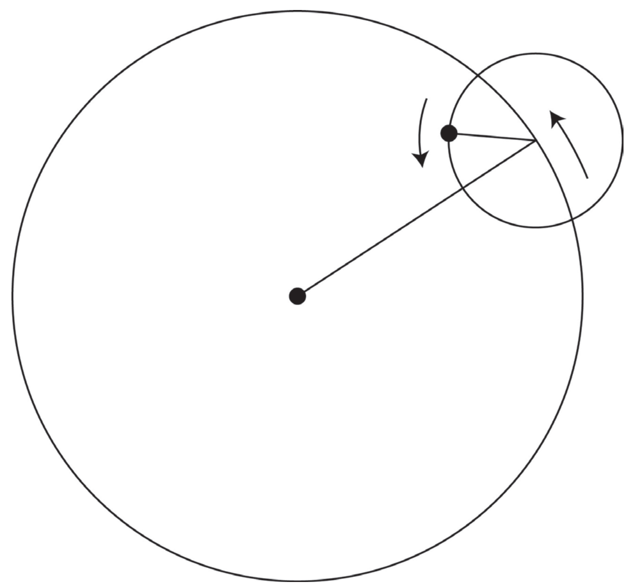

Figure 3. Same-sense epicycle model. If the periods of revolution in a geocentric frame of reference are equal, the body will trace out a closed epitrochoid that, for a small epicycle, closely approximates an eccentric circle with slight flattening around the perigee. The body's actual speed, as well as its apparent speed as seen from the Earth, are slowest at perigee and fastest at apogee.

Before examining Ptolemy's claim of establishing on empirical grounds that, considerations of simplicity aside, the opposite-sense epicycle model can be shown to be viable—and by implication, the same-sense model can be shown not to be viable—it is worthwhile to look at the treatment of this question by Ptolemy's older contemporary, Theon of Smyrna in his *Mathematics Useful for Reading Plato*. Theon motivates the application of eccenter and epicycle models to explaining the Sun's apparent motion in a way similar to *Almagest* 3.4, by asserting that the time intervals between the solstices and equinoxes are not equal whereas the angles separating the Sun's longitudes at these events are all 90°. He gives exactly the same figures as Ptolemy for the time intervals between vernal equinox and summer solstice (94½ days) and between summer solstice and autumnal equinox (92½ days), but he also gives the remaining two intervals between autumnal equinox and winter solstice (88⅛ days) and between winter solstice and vernal equinox (90⅛ days), which Ptolemy does not give. This is a significant difference. Ptolemy sets out to derive the parameters of his eccenter model from the given time intervals, a calculation that calls for observed dates of just three of the four events. Theon, however, simply posits—without even proving that this is geometrically possible—a position of the eccenter relative to the Earth and ecliptic such that the eccenter is divided by the solstitial and equinoctial lines in unequal quadrants proportional to the four given intervals, and then he asserts as bald facts the same parameters that Ptolemy derives trigonometrically, namely that the center of the eccenter is displaced from the center of the Earth and cosmos by $^{1}/_{24}$ of its radius in the direction of Gemini 5½°.

Theon now turns to the same-sense epicycle model, using the diagram shown in Fig. 4. This diagram shows the Earth as point Θ, the ecliptic as circle ΑΒΓΔ (with the direction of increasing longitude being counterclockwise), the deferent as circle ΜΟΝΞ, and the epicycle in four successive positions as circles ΕΖΗΚ, ΛΠ, ΦΥ, and ΧΨ. From a geocentric radial perspective, the Sun travels counterclockwise on the epicycle, from Ε to Ζ to Η to Κ and back to Ε, in the same time that the epicycle takes to revolve around Θ, so that its actual locations in relation to the whole

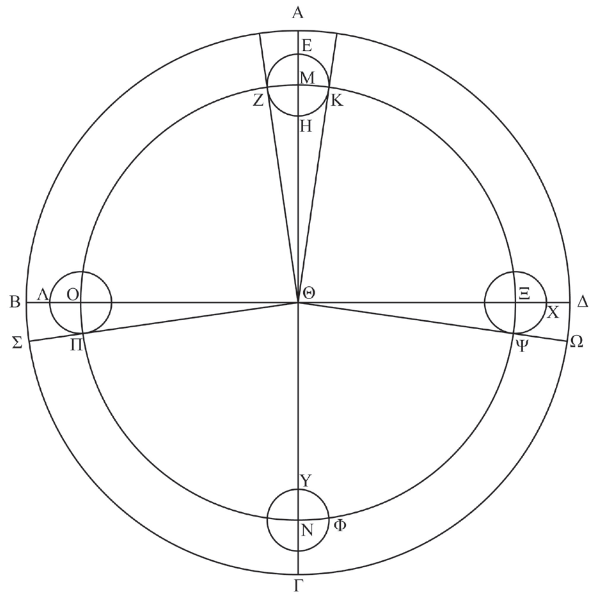

Figure 4. Theon's diagram for the same-sense epicycle model.

system are successively E, Π, Y, and Ψ. For the observer at Θ these points are projected on the ecliptic as A, Σ, Γ, and Ω. Noting that E represents the Sun's furthest distance from the Earth, Theon therefore equates this position with the apogee of Gemini 5½° that he previously gave for the eccenter model. From the diagram he has no difficulty in showing that the Sun sweeps out the larger arc ΩAΣ in the same time interval (half a year) as it sweeps out the smaller arc ΣΓΩ, which means that the Sun appears to be moving faster around Gemini 5½° than around the diametrically opposite point, which contradicts the apparent speeds implied by the given time intervals between the solstices and equinoxes.

The diagram for the opposite-sense model (Fig. 5) is similar, but now the Sun revolves clockwise around the epicycle going from E to K to H to Z and back to E in the same time as the epi-

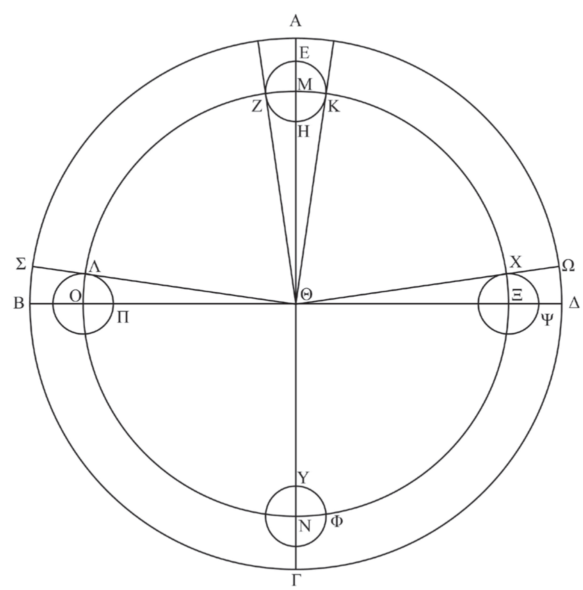

Figure 5. Theon's diagram for the opposite-sense epicycle model.

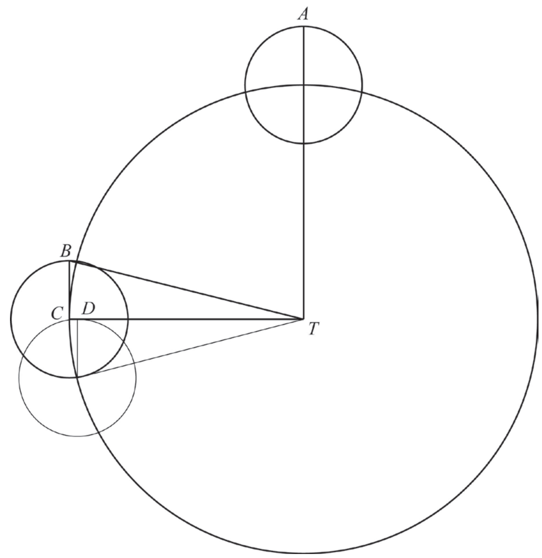

Figure 6. Behavior of the opposite-sense epicycle model. When the Sun is at its apogee, A, the apparent speed is slowest. One quarter of a year later, it is at B, but the moment when the apparent speed equals the mean is slightly later, when the Sun, at D, is at the point where TD is tangent to the epicycle. At this point the true anomaly, angle ATD, is 90°.

cycle revolves counterclockwise around Θ. The four positions of the Sun and their projections on the ecliptic are lettered as before, but now arc ΩΑΣ is smaller than arc ΣΓΩ, so that the Sun's apparent motion around Gemini 5½° is slower than around the diametrically opposite point, in agreement with the given time intervals. For Theon's didactic purposes, this is a sufficient demonstration that the opposite-sense epicycle model is viable for the Sun.

Theon's treatment of the same-sense model is obviously fallacious, because he has no right to assume that the apogee of the path traced by the Sun in this model must coincide with the apogee found for the eccenter model. In fact the opposite is the case: if one were to hypothesize a same sense model and then use trigonometric methods as in *Almagest* 3.4 to derive the direction of the apogee, it would turn out to be almost exactly 180° from Gemini 5½°. Hence in Fig. 4 the region around Γ would correspond to the part of the ecliptic around Gemini 5½°, so that the model predicts slower motion just where it ought to.

Ptolemy does not make this mistake. His criterion is whether the moments when the Sun appears to move at mean speed are closer to the moment of slowest apparent motion or that of fastest apparent motion, which is a valid discriminant between the two varieties of epicyclic model. In the opposite-sense model (Fig. 6), one quarter of a year's motion brings the Sun from its point of least apparent speed at apogee, *A*, to a position *B* such that its longitude less than the mean longitude, but its equation, angle *BTC*, has not yet reached its (subtractive) maximum, at which point the apparent speed equals the mean speed. This occurs a little later, when the Sun is at *D*. In the same-sense model (Fig. 7), when the Sun has travelled for a quarter of a year starting from its point of least apparent speed at perigee, *A*, it will be at exactly the same position *B* as in the opposite-sense model, but it will *already* have passed the point of maximum equation, *D*, when the apparent speed equalled the mean speed.

But how could one test this discriminant empirically? Ptolemy just writes vaguely that it is "concordant with the phenomena." He would have had absolutely no way of directly measuring the apparent speed of the Sun to a precision of even a minute per day, and around the dates of maximum equation, when the rate of change of the speed is greatest, it is accelerating or decel-

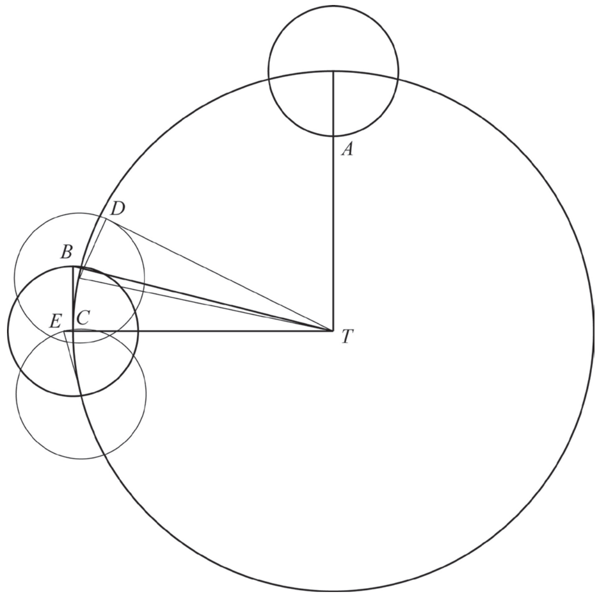

Figure 7. Behavior of the same-sense epicycle model. The apparent speed is lowest when the Sun is at its perigee, A. One quarter of a year later, it is at B, but the moment when the apparent speed equals the mean is slightly earlier, when the Sun is at D such that TD is tangent to the epicycle. The position of the epicycle when the true anomaly of the Sun, at E, is 90° is very close to the corresponding position in the opposite-sense model.

erating by less than three *seconds* per day. The observations on which the *Almagest*'s solar theory is based are solstices and equinoxes, which yield dated longitudes only at four points 90° apart. In principle Ptolemy could have tried to determine solar longitudes at other dates from the observed solar declinations, using a meridian instrument or armillary, but these would certainly not have been accurate enough to determine the date when the Sun's apparent speed was equal to the mean speed.

Even considering the apparent speeds cumulatively, that is, comparing observed longitudes over longer intervals with the predictions of the models, one would not be able to demonstrate empirically that one model is more successful than the other. If one computes solar longitudes for every day over an entire year according to Ptolemy's eccenter model (kinematically equivalent to the opposite-sense epicycle model) and according to a same-sense epicycle model having equivalent parameters and aligned with its perigee matching the eccenter model's apogee, the difference between the models has a maximum of just 6 minutes when the mean Sun is around 45° and 135° on either side of the apsidal line, well below the precision of longitudes derived from observed declinations. The effects on phenomena involving other heavenly bodies of discrepancies of 6 minutes in solar longitudes would also be too small to isolate; for example the times of true syzygies would be affected by about ten minutes at most.[15]

The most pronounced difference between the predictions of the two models from a geocentric point of view is in the distances of the Sun from the Earth. The opposite-sense model makes the Sun trace, with uniform true speed, an eccentric circular path whose apogee is at Gemini

15 Swerdlow 2004, 250 suggests that one could confirm Ptolemy's claim by observing that the time from when the Sun is at apogee (meaning Gemini 5½° or thereabouts) to when it is observed at 90° elongation from apogee is about five days greater than the time from 90° elongation to perigee. This is, however, practically the same as Theon's attempted demonstration; the phenomenon would be consistent with a same-sense epicycle model having its perigee near Gemini 5½°, as one can see by comparing the situations of the Sun and its epicycle at the moment of 90° elongation according to the two models in Figs. 6 (Sun at *D*) and 7 (Sun at *E*).

5½° according to Ptolemy's parameters. The same-sense model fitted to the same initial equinox and solstice observations makes the Sun trace, not quite uniformly, an epitrochoid that closely approximates the eccentric circle of the other model in shape and size, but with its *perigee* at Gemini 5½°. According to either model, the apparent diameter of the Sun's disk should be about $^1/_{12}$ greater at perigee than at apogee, so one might hope to use measurements of the diameter to determine which end of the apsidal line is the apogee. Whether an ancient observer could have detected a variation of this order of magnitude in the apparent solar diameter is an open question, but we know what Ptolemy thought: he writes in *Almagest* 5.14 that "we find that the diameter of the Sun is always subtended by approximately the same angle in every situation, with no significant variation arising from its distances."

Ptolemy's claim that the phenomena determine the required sense of the Sun's revolution in an epicycle model is thus pseudoempirical, in a stronger sense than the claim about the apparent disks of the heavenly bodies in 1.3, since here he is directly asserting that one can observe an unobservable effect.

Planetary epicycles and eccenters

In *Almagest* 9.5 Ptolemy presents a rationale for the structure of his models for the five planets. The key points are as follows:

> (1) The planet revolves uniformly around an epicycle, with the sense of revolution such that the planet travels in the direction of increasing longitude when it is on the part of the epicycle furthest from the Earth.
>
> (2) The center of the epicycle revolves around an eccentric deferent circle.
>
> (3) The apsidal line of the model shifts uniformly in the direction of decreasing longitude at the rate of precession.
>
> (4) The angular motion of the epicycle's center around the eccenter is such that its angular motion is uniform with respect to an equant point distinct from the center of the deferent.

Only the first two points are justified in this chapter on the basis of empirical claims similar to the one we have just examined concerning the Sun's motion.

Ptolemy begins by pointing out that there are two fundamental model types available, the eccenter model and the epicycle model, and that there are "similarly" (ὁμοίως) two anomalies in each planet's motion, the synodic anomaly correlated with the planet's elongation from the Sun, and the zodiacal anomaly correlated with the planet's longitude. Although he does not yet explicitly draw an inference from this conjunction of two pairs, the linkage by ὁμοίως clearly hints at the plausibility of a one-to-one correspondence: if one anomaly is caused by an epicycle, the other would be caused by an eccenter. This sets up a weak, aprioristic bias against a double epicycle model even before we have looked at the details of the two anomalies.

Ptolemy then offers a phenomenon that ostensibly proves that the synodic anomaly is produced by a same-sense epicycle:

> We find in the case of the five planets from various configurations observed successively and around the same parts of the zodiac that the time from the greatest speed to the mean is always greater than that from the mean to the least.

This appears to mean a procedure along the following lines.[16] For some selected region of the ecliptic, one determines from observation two dates when the planet was at its maximum elongation on either side of its mean longitude. If these belonged to different synodic cycles, one subtracts whole mean synodic cycles from the interval separating them, and what remains will be an estimate of either the interval from mean apparent speed through maximum and back to mean or that from mean through minimum and back to mean, on the assumption that the zodiacal anomaly has not significantly changed throughout. What Ptolemy asserts, then, is that the intervals from mean to greatest to mean speed are consistently more than half a mean synodic period, and those from mean to least to mean are consistently less. This is a test that could have been performed for all five planets; Saturn would be the most difficult case because of its small synodic anomaly, but careful observations and interpolation ought to have made it possible to detect the inequality of the intervals if not their precise length.[17] Ptolemy's inference that the synodic anomaly must be produced by a same-sense epicycle is true to the extent that an opposite-sense model can be ruled out, though he fails to mention that there exists an eccenter model with advancing apsidal line that is kinematically equivalent to the same-sense epicycle model.[18] The empirical claim itself, however, is sound.

Conversely, Ptolemy has an empirical argument to prove that the zodiacal anomaly is produced by an eccenter or an opposite-sense epicycle:

> In the case of the anomaly that is observed [θεωρουμένης] in relation to the parts of the zodiac, we find contrarily from the arcs of the zodiac taken up at the same phases or the same configurations that the time from the least speed to the mean is always greater than that from the mean to the greatest.

Ptolemy's highly compressed statement can be expanded as follows. We have deduced that the synodic anomaly results from the planet's revolution around an epicycle. We now wish to investigate the motion of the epicycle's center around the Earth, to see whether the time from this center's fastest apparent motion to the moment when its apparent motion equals its mean motion is greater or less than the time from that moment to the slowest apparent motion. Since the center cannot be observed directly, one uses multiple observations of the planet at a particular stage of its synodic cycle ("phases" or "configurations"), when it is approximately at the same point on the epicycle so that the center's longitude can be approximated from the observed longitude of the planet. On the basis of such observations, Ptolemy asserts that the time from least to mean speed is consistently greater than the time from mean to greatest speed.

16 This is a slight simplification and generalization of the second method outlined by Swerdlow 2004, 252. For an inferior planet, the mean longitude is the same as the Sun's mean longitude; for a superior planet, it is obtained from the relevant period relation. In either case the exact alignment of the mean longitude is not required, only a reasonably accurate rate of mean motion.

17 Swerdlow 2004, 251 asserts that a demonstration of this kind is only practicable for Venus. In the case of Mercury, it is true that the planet's day-to-day longitudes could only be observed adequately in certain portions of the ecliptic. However, his statement that the point of tangency on the epicycle cannot be observed for the superior planets is mistaken; one merely has to find the date when the difference between observed longitudes of the planet on successive days and the planet's calculated mean longitudes is at a maximum.

18 Ptolemy mentions the possibility of eccenter alternatives at *Almagest* 12.1, though only for the superior planets. See *POxy astron.* 4173 (Jones 1999a, 1.166-167 and 2.152-155) for a fourth century AD fragment of a set of mean motion tables, based on Ptolemy's but apparently pertaining to a system in which the inferior planets had epicycle models and the superior planets eccenter models.

Swerdlow remarks that the most direct way to carry out such a demonstration would be using oppositions of a superior planet, since this phase should coincide with the moment when the planet's observed longitude coincides with the longitude of the epicycle's center.[19] In fact oppositions (or accurately interpolated conjunctions) are the *only* synodic phenomena that have a hope of yielding meaningful results, since stations and first and last visibilities involve factors that make the elongation of the planet from its mean longitude significantly variable. A sufficiently dense collection of observed oppositions (which requires a sustained program of observations over several years or even decades, depending on the planet) would allow one to show by interpolation that the time from the point of slowest apparent speed to a longitude 90° greater is longer than the time from this point to the point of greatest apparent speed.[20] But this is a situation closely paralleling our examination of the Sun's anomaly. If the epicycle's center travels on an eccenter or an opposite-sense epicycle, the point when it has a longitude 90° away from the point of slowest apparent speed will be the point of mean apparent speed; but the nature of the model is precisely what we are trying to determine, so it would be a *petitio principii* to claim that we have demonstrated Ptolemy's empirical claim. A same-sense epicycle model with the perigee at the point of least apparent speed would be equally compatible with the observations, though it would result in the points of mean speed being closer to the point of slowest speed. As was the case with the Sun, there is no way that Ptolemy could have obtained a set of observations dense enough or precise enough to discriminate between an optimally-fitted same-sense epicycle model and the eccenter model that he adopted. It is noteworthy that, after ruling out the same-sense model, he chooses the eccenter over the kinematically equivalent opposite-sense epicycle by invoking not just the simplicity argument used in *Almagest* 3.4 but also the correspondence principle, that two distinct anomalies call for two distinct model types.

The precessional motion of the planetary apsidal lines

Ptolemy's justifications of the two remaining points about the planets' models that he asserted in *Almagest* 9.5 are more complex. In each case he provides a detailed demonstration based on dated observation reports for one or two planets, but for the remaining planets he gives only a brief general empirical claim. Thus he shows in 9.7 by an analysis of observations made in his own time and four centuries earlier that Mercury's apsidal line has shifted eastward by 4°, the amount corresponding to his rate of precession, but there is no corresponding demonstration for the other four planets, only a terse remark at the end of 9.7 that we find the hypothesis that the apsidal line has precessional motion to be "concordant" (σύμφωνον) "from the part-by-part fitting [ἐκ τῆς... κατὰ μέρος ἐφαρμογῆς] of the phenomena relating to the other planets."[21]

At this point it will be useful to have a sense of how well Ptolemy's models and their parameters are fitted to the motions of the planets. For this purpose we have used Ptolemy's *Almagest* models and the JPL Horizons ephemeris[22] to compute long runs of longitudes (comprising a

19 Swerdlow 2004, 253.

20 Swerdlow 2004, 253-254, illustrated for Jupiter.

21 The extension to the other planets is thus not simply an instance of analogical argument as stated in Jones 2005, 30 (cf. Swerdlow 2004, 254), though analogy is a latent, secondary consideration, since the reader is likely to infer that Ptolemy would not have postulated moving apsidal lines for the other planets purely on the basis of the "fitting of the phenomena" without the ostensibly secure example of Mercury.

22 http://ssd.jpl.nasa.gov .

Planet	*Date range*	*Mean* $\Delta\lambda$	*Standard Deviation* $\Delta\lambda$	*Maxima*
Mercury	AD 110-155 (46y)	−1.26°	2.86°	−9.30°/+7.07°
Venus	AD 100-163 (64y)	−1.24°	1.07°	−2.65°/+4.68°
Mars	AD 110-156 (47y)	−1.17°	0.38°	−2.11°/+0.03°
Jupiter	AD 140-151 (12y)	−0.95°	0.12°	−1.23°/+0.60°
Saturn	AD 120-149 (30y)	−1.15°	0.24°	−1.58°/+0.56°

Table 1. Fit of Ptolemy's models to runs of longitudes computed from the JPL ephemeris.

rough return to the initial Sun-planet configuration) for each planet at 5-day intervals,[23] and in Table 1 we display (1) the mean value of the difference $\Delta\lambda = \lambda_{Ptolemy} - \lambda_{JPL}$, which is the systematic longitudinal offset, incorporating the approximately −1° error in Ptolemy's tropical frame of reference for his own time, (2) the standard deviation of $\Delta\lambda$, and (3) the maximum positive and negative values of $\Delta\lambda$.

Next, using the same runs of JPL longitudes, we determined the optimum values for the three dimensional parameters of Ptolemy's models, namely the longitude of apogee λ_A, the eccentricity *e* of the deferent (scaled such that the deferent's radius is 60), and the epicycle radius *r* (according to the same scale), so that the standard deviation of $\Delta\lambda$ is minimized.[24] In Table 2 we give Ptolemy's parameters, the optimized parameters, and the measures of fit for the models with the optimized parameters.

One is struck by how close Ptolemy's parameters for the superior planets are to their optimum values. Among them, the parameter that makes the largest contribution to error in predicted longitudes is Saturn's epicycle radius.[25] Venus's epicycle radius is also very accurate,[26] its apogee at least decent, but the eccentricity is significantly too large. Mercury has a good epicycle radius, too small an eccentricity, and a disastrously inaccurate apogee. In addition, the

Planet	*Ptolemy* λ_A	*e*	*r*	*Optimized* λ_A	*e*	*r*	*Mean* $\Delta\lambda$	*Standard Dev.* $\Delta\lambda$	*Maxima*
Mercury	190°	3	22.5	218.6°	4.57	22.14	−1.26°	2.05°	−6.69°/ +4.03°
Venus	55°	1.25	43 1/6	59.2°	0.83	43.35	−1.23°	0.73°	−1.94°/ +1.79°
Mars	115°	6	39.5	116.3°	5.89	39.48	−1.17°	0.31°	−2.40°/ +0.24°
Jupiter	161°	2.75	11.5	160.7°	2.69	11.54	−0.95°	0.09°	−1.12°/ 0.74°
Saturn	233°	3 25/60	6.5	234.1°	3.53	6.30	−1.15°	0.07°	−1.37°/ +1.02°

Table 2. Ptolemy's planetary parameters compared with optimally fitted parameters.

23 The *Almagest* longitudes were computed for mean noon, Alexandria, and the JPL longitudes for 14:00 UT.

24 For these optimizations we have retained Ptolemy's mean motions. The error contributed by Ptolemy's inaccurate tropical frame of reference is small over the time intervals used here.

25 Interestingly, in the earlier *Canobic Inscription* Ptolemy gave 6 1/4 for the epicycle radius, much closer to the optimum. One wonders what led him change it.

26 The precise value adopted by Ptolemy almost exactly the radius required in a simple epicycle model such that the greatest elongation is 46°, which is one of the traditional estimates reported in ancient sources (Pliny, *Naturalis Historia* 2.6.38 with attribution to Timaeus, see Duke 2002, 57).

Planet	*Ptolemy*		*Optimized*			
	2nd cent. AD	*3rd cent. BC*	*2nd cent. AD*	*3rd cent. BC*	*Shift*	*Adjusted Shift*
Mercury	190°	186°	218.6°	213.2°	5.4°	3.8°
Venus	55°	51°	59.2°	52.3°	6.7°	5.1°
Mars	115°	111°	116.3°	110.5°	5.8°	4.2°
Jupiter	161°	157°	160.7°	155.4°	5.1°	3.5°
Saturn	233°	229°	234.1°	230.0°	4.1°	2.5°

Table 3. Parameters for Ptolemy's planetary models optimized for his own time and 400 years earlier.

peculiar model Ptolemy assumes for Mercury, with its rapidly revolving eccenter and resulting double perigee, makes a large contribution to the error in predicted longitude; an optimally fitted model having the same structure as for the other planets would bring the standard deviation of $\Delta\lambda$ down to 1.67°.[27]

Thirdly, we determine the optimized parameters to fit runs of longitudes from the JPL ephemeris of the same length and density as those used above, but exactly 400 Julian years earlier. The apogees for Ptolemy’s time and for four centuries earlier are compared in Table 3. The shift in longitude according to Ptolemy’s theory is of course 4° for all planets. For the optimized apogees, we give both the shift in true tropical longitude and the shift reduced by 1.6° to correct for the accumulated error in Ptolemy’s tropical frame of reference over four centuries.[28]

Now if Ptolemy’s resources for locating Mercury’s apogee in his own time were so defective that the result was nearly 30° off, it should be obvious that he could not have detected a shift on the order of 4°, as he claims, by comparing his result with observations from the third century BC, the oldest period from which he appears to have had planetary observation reports. The reasons behind the large errors in Ptolemy’s parameters for Mercury as well as those behind the unnecessary special structure of his model cannot be recovered in detail, but the chief cause was undoubtedly the highly restricted conditions under which Mercury could be observed in proximity to fixed stars. An approximate idea of these conditions can be obtained from the numerous preserved Babylonian records from the last four centuries BC of observed positions of Mercury’s position relative to the so-called Normal Stars. Figure 8 shows the planet’s actual positions in longitude and latitude (according to modern theory) at the dates of these observations. The parts of the zodiac within which Babylonian observers were able to see Mercury together with a nearby Normal Star turn out to have been limited to two intervals of roughly half the zodiac, one of them applying to evening observations, the other to morning observations.[29] An observer in Alexandria or generally in Egypt would have had slightly better observing conditions for Mercury because of the lower terrestrial latitude, but there would still have been large “blackout” areas within which it would have been difficult or impossible to obtain accurate observed positions of the planet. This would apply both to Ptolemy’s own observations and to any that were available to him from past centuries (e.g. by Timocharis or the unknown observers who recorded planetary observations with dates “according to Dionysius”).

27 The parameters of the optimized conventional model would be λ_A = 218.6°, e = 2.75, and r = 22.74.

28 In any planetary observations used by Ptolemy, he has determined the planet’s longitude relative to one or more fixed stars, whose positions for the date are found from his star catalogue adjusted according to his 1° per century precession rate.

29 The absence of reported observations of Mercury around 15°-30°, 240°-270°, and 300°-350° results from large gaps between stars in the regularly used Normal Stars; see Jones 2004, 481-491 with Figures 1-2 on pp. 493-494.

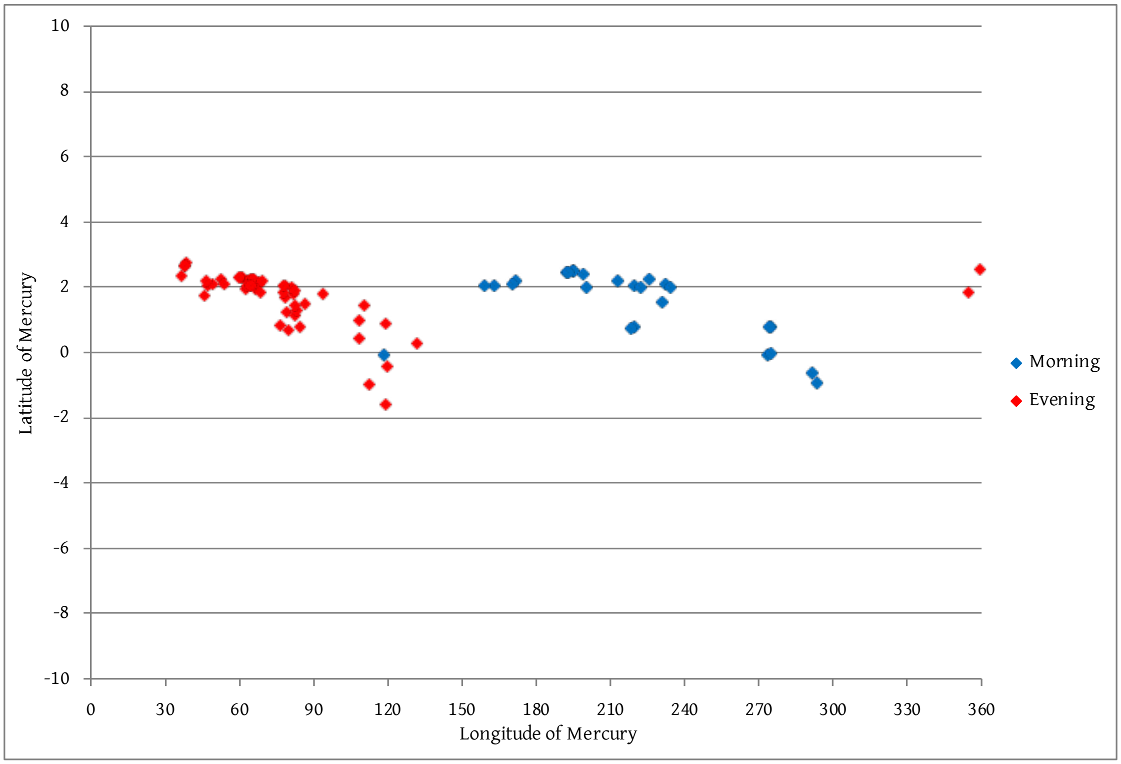

Figure 8. Locations of Mercury at the dates of preserved Babylonian observations of the planet near Normal Stars.

That Ptolemy nevertheless gives the appearance of demonstrating empirically that the longitude of Mercury's apogee and perigee were almost exactly 4° lower in the first half of the third century BC than in his own time is a tribute to his skill in manipulating an analysis of observation reports to yield a preordained result. The method, which involves finding a line of symmetry between one selected observation and an interpolation between two others, is very sensitive to small variations in the data, as Ptolemy has to have known by experience, and the agreement of the apogee that he obtains with his theoretical expectation depends on several small imprecisions in the calculations.[30] It is hard to avoid the conclusion that his analysis of the third century BC observations constitutes a pseudoempirical claim of a more elaborate kind than the general statements about solar and planetary speeds that we have considered up to now.

This places a greater burden on Ptolemy's brief and frustratingly imprecise statement that the phenomena for the remaining planets "fit" a precessional motion of their apogees. Read strictly, Ptolemy's wording could signify only that the phenomena are *consistent* with shifting apogees, but the context implies a stronger connotation, that the phenomena *require* them; the word translated above as "fitting," ἐφαρμογή, is used, for example, to signify coincidence of geometrical objects. The planets' apogees really do shift in a tropical frame of reference at rates close to that of precession as shown in Table 3 (in other words, in a sidereal frame of reference they move more slowly than the solstitial and equinoctial points). Could Ptolemy have detected and measured these motions for the planets other than Mercury by locating their apogees at a period several centuries before his time from old observation reports?

30 See Jones 2005, 27-30 for details. Ptolemy's inclusion of two Babylonian reports of Mercury's passage by Normal Stars suggests that the selection of observations as well as their analysis was motivated by the desire to obtain a particular result, since such reports are very imprecise indicators of the planet's longitude.

In the case of Venus, the answer is almost certainly not. Ptolemy's value for the longitude of Venus's apogee in his own time, 55°, is ostensibly obtained as one of the midpoints of two pairs of observed equal but opposite greatest elongations of the planet from the mean Sun. But these observations do not stand up to closer scrutiny. In the first place, their reported dates all differ by from 12 to 20 days from those of the actual greatest elongations; these discrepancies far exceed the plausible range of error in determining these phenomena, and they are large enough so that the planet's elongation on the reported dates would have been significantly less than a greatest elongation occurring on that date would have been.[31] Two of the observations (AD 127 October 12, morning, and 132 March 8, evening) are ascribed to Theon the mathematician, and as reports of the location of Venus relative to nearby stars they appear to be reasonably accurate.[32] The other two (136 December 25, evening, and 140 July 30, morning), which Ptolemy says that he observed himself, both state that the planet was a small fraction of a degree from a nearby star when in fact its distance was well over a degree away; these are obviously fabricated positions chosen so that the elongations are exactly equal and opposite to the elongations of the observations by Theon with which they are paired off.[33]

As Swerdlow has convincingly argued, genuine observations of greatest elongations would only have allowed Ptolemy to determine the location of the apogee very roughly, at best to within a broad region of a zodiacal sign, and he likely chose the specific 55° longitude because it is approximately 90° from the mean solar longitudes for a pair of greatest elongations that he uses to locate the center of uniform revolution of Venus's epicycle.[34] Supposing that he had observations from, say, four centuries earlier of sufficient quality and density to allow him to determine a satisfactory collection of greatest elongations, he would have been in no better position to locate a precise apogee for the earlier date than for his own time.[35]

The situation with respect to the superior planets is different. Ptolemy's apogees, adjusted for the error in the tropical frame of reference, are correct to within half a degree for Mars and Saturn, and within a degree and a half for Jupiter. There is little reason to doubt that his apogees were determined by essentially the method of analysis of three observed oppositions to the mean Sun that he uses to demonstrate them in *Almagest* 10.7, and 11.1, and 11.5, although the ostensibly empirical data in these chapters have been adjusted so that the calculations yield more or less exactly the round number eccentricities that he adopts in his models.[36] If he was

31 See Swerdlow 1987, 36-43 and especially Table 1, p. 37.

32 Theon's distances are expressed in terms of the "length of the Pleiades" in these reports, adding a subjective element to the reduction of Venus's location to a precise longitude. The AD 127 report states that Venus appeared to be passing β Vir "one Moon" to the north, which must be a mistake since their latitudes were almost equal.

33 In the AD 136 report Ptolemy states that Venus was ⅔ of a "Moon" west of φ Aqr and that a near-occultation seemed to be about to occur, when in fact the planet was approximately 1¼° west of the star in longitude and almost a degree north of it in latitude. The AD 140 report has Venus half a "Moon" northeast of ζ Gem, but Venus was actually almost 1½° east of the star and at nearly the same latitude.

34 Swerdlow 1989, 41-43. Rawlins 2002 and Thurston 2002 show that the parameters for a model for Venus could be derived from analysis of an arbitrary set of three observed greatest elongations; there is no evidence, however that this method was used in antiquity, and Ptolemy clearly was not aware of it.

35 In any case, because of the small eccentricity of Venus's orbit and the imperfect fit of Ptolemy's model with any parameters to the planet's actual geocentric longitudes, the optimal apogee for the model fluctuates over a range of several degrees depending on the selection of date-longitude pairs to which the model is fitted.

36 Thurston 1994. The calculations in the *Almagest* have also been manipulated so that the long iterative procedure appears to converge faster than it really should (Duke 2005). I would guess that the *Almagest* parameters were

able to determine the apogees with comparable success from third century BC observations, a shift of their longitudes of the same order of magnitude as precessional motion would have been obvious.

For this, Ptolemy would have needed a sufficient number of reliable dated observations of each planet near each of several oppositions so that he could accurately estimate the moments when the planet was diametrically opposite the mean Sun according to his theory. There were certainly a few reports of near-oppositions among the early planetary observations available to Ptolemy. In *Almagest* 11.7 he cites a Babylonian report from 229 BC of Saturn near a Normal Star that, by his calculations, was four days before mean opposition. A report from 241 BC of Jupiter's location relative to fixed stars was cited in an early second century AD astronomical treatise that was probably known to Ptolemy; he would have determined that this was about two days before mean opposition.[37] But we have Ptolemy's own testimony in *Almagest* 9.2 that most of the old planetary observations preserved in his time were unsuitable for theoretical analysis, being mostly first visibilities, last visibilities, stations, and observations of positions such that the reported distances from fixed stars were too large to be considered reliable. His contention in this chapter is that satisfactory observational resources for working out proper planetary models did not exist much before his own time.

In any case, if Ptolemy had meant to indicate in 9.7 that the precessional shift of all the planets' apogees could be demonstrated by comparing apogees independently determined at widely separated periods, he would have written this more explicitly. His expression, invoking the ἐφαρμογή ("fitting") of phenomena to the hypothesis, actually harks back to a passage in 9.2 in which he warns the reader of certain methodological compromises that he will have to make in his planetary theory, including "hypothesizing certain primary matters not on the basis of an observed starting point [μὴ ἀπὸ φαινομένης ἀρχῆς], but having grasped them in accordance with continued trial and fitting [ἐφαρμογή]."[38] Such a process would mean, in the present instance, that Ptolemy tried out various possibilities for the behavior of the planets' apogees, and found that sidereally fixed apogees yielded the best agreement with observations at dates remote from his time.

The problem with this "better fit" account is that small changes in the assumed longitude of apogee do not have a very pronounced effect on the predicted longitudes of the planets, and in individual observations the effect would be obscured by noise. For example, if we compare our 12 years' worth of longitudes of Jupiter computed according to Ptolemy's parameters with longitudes computed according to the same model but with a 4° change in the apogee, we find that the differences never exceed ±0.5°, with a standard deviation of 0.26°. But the differences between Ptolemy's model and modern theory have a roughly ±0.3° range with standard deviation 0.12°, while even carefully selected observation reports would have been subject to errors on the order of, say, a sixth of a degree. The effect of apogee shift on longitude is still smaller in relation to the other errors for Venus and Saturn. Mars offers the best prospect for discerning it; for this planet, a 4° shift can lead to differences in predicted longitude as large as 2.5°, with standard deviation 0.70°, though 89% of the absolute differences are less than 1, and the larger

derived by selection or averaging of results from several sets of oppositions.

37 *P. Oxy. astron.* 4133 in Jones 1999a, 1.69-80 and 2.2-5; for the probable relation to Ptolemy see Jones 1999b. The observation was probably made by the same person whose report of Jupiter's position earlier in the same year is in *Almagest* 11.3.

38 "ὑποτίθεσθαί τινα πρῶτα μὴ ἀπὸ φαινομένης ἀρχῆς, ἀλλὰ κατὰ τὴν συνεχῆ διάπειραν καὶ ἐφαρμογὴν εἰληφότα τὴν κατάληψιν."

discrepancies occur around the planet's intervals of invisibility. Ptolemy would have been lucky to find suitable observations among his sources of early planetary observation records.

It seems likely, therefore, that Ptolemy's entire treatment of the motion of the apsidal lines, not just the demonstration for Mercury, is pseudoempirical. He presents this feature of his models in 9.5 as an additional complication to the basic epicycle-on-eccenter model, because his longitudinal frame of reference is tropical, but he is probably taking over the assumption that the planets' apsidal lines are tropically fixed from earlier planetary models that had this assumption by default since their frame of reference was sidereal.[39] Fortuitously, his shifting apsidal lines turn out to be a much better approximation to reality than tropically fixed lines would have been.

The eccentricities of Venus

Ptolemy's fourth point concerning the models for the planets is that the centers of uniform angular motion (i.e. equants) for their epicycles are distinct from the centers of the eccentric deferents. The special model for Mercury, described in 9.6, has the deferent's center revolving rapidly on a small circle whose center lies twice as far from the Earth in the direction of the apogee as the equant. We will not discuss Mercury's model further here. For the remaining four planets, the deferent's center lies at the exact midpoint of the Earth and the equant, a condition often called the "bisection of the eccentricity."

Ptolemy justifies the bisection of the eccentricity in a similar manner to his treatment of the motion of the apsidal lines, that is, he provides a detailed observation-based deduction of the two eccentricities for one planet, Venus (*Almagest* 10.2-3), but the extension of the hypothesis to the remaining three planets rests only on a general empirical claim (10.6). This claim is, however, more specific than the one provided in 9.7 for the motion of the apsidal lines of the planets other than Mercury:

> In the case of each of these (planets), following a general approach [κατὰ τὸ ὁλοσχερέστερον τῆς ἐπιβολῆς], the (eccentricity) found by means of the greatest difference in the anomaly dependent on (the position in) the zodiac is found to be approximately double the eccentricity arising from the quantity of the retrogradations around the greatest and least distances of the epicycle [ἐκ τῆς πηλικότητος τῶν περὶ τὰς μεγίστας καὶ ἐλαχίστας ἀποστάσεις τοῦ ἐπικύκλου προηγήσεων].

This passage has been much discussed in recent scholarship, and we can afford to be brief with it.[40] Ptolemy clearly means this argument to be a rough empirical indication of the bisection, not a summary of a rigorous deduction, in contrast to what he has previously shown for Venus. Whether or not this is what Ptolemy intended, the reader would likely interpret it as saying that there are two *independent* ways of nonrigorously estimating the eccentricity causing a planet's zodiacal anomaly, assuming an epicycle and eccenter model without yet differentiating between the center of the deferent and the center of the epicycle's uniform motion. One way, based on empirical information relating to the planet's retrogradations, indicates the variation in distance of the epicycle's center from the Earth and thus the deferent's eccentricity; the other,

39 Jones 2005, 29-30.

40 Evans 1984, 1088; Swerdlow 2004, 262-263; Jones 2004, 376-380.

based on equations of center derived from observations, indicates the location of the equant.[41] Read thus, the statement is simply false with respect to Jupiter and Saturn, while for Mars it is correct so far as it goes, but omits the crucial fact that the apogee derivable from Mars's retrogradations according to an equantless model is diametrically opposite that derivable from the equations of center. If Ptolemy had written this passage so as to give a more or less valid statement of the phenomena, it would have been something like this:[42]

> The eccentricity arising from the quantity of the retrogradations around the greatest and least distances of the epicycle is, in the case of Mars, about half that found by means of the greatest difference in the anomaly dependent on (the position in) the zodiac, with the apogee in the opposite part of the zodiac, while in the case of Saturn (the eccentricity derived from retrogradations) is about a third (of the eccentricity derived from equations of center), with the apogee in the same part of the zodiac, and in the case of Jupiter it is too small to determine.

Needless to say, a statement along these lines, while showing that the simple model is inadequate, would not make the necessity of bisecting the eccentricity appear obvious.

On the other hand, Ptolemy's statement could be understood as a radical compression of the following:

> The eccentricity derived from equations of center, if applied to a simple epicycle and eccenter model, does not yield retrogradations that agree with the phenomena. If, however, we assume that this is the eccentricity of the center of uniform motion, but that the eccentricity of the eccenter is half that, the predicted retrogradations agree with the phenomena.

This would be a valid description of an effect of the bisection, which is accurate within the range of precision of ancient observations and detectable for all three superior planets. It is likely what Ptolemy meant,[43] but one may reasonably suspect that he deliberately expressed the idea so as to convey a deceptive impression that a 1 : 2 ratio of eccentricities is apparent already from a "naïve" consideration of each planet's observed behavior. Despite the elliptical and misleading wording, I would *not* characterize this as a pseudoempirical claim.

The demonstration for Venus is a different matter. Here Ptolemy has a procedure that appears to truly separate the measurement of the epicycle's distance at apogee and perigee from the determination of the center of uniform motion. Let us first consider the latter.

Ptolemy has begun his treatment of Venus in 10.1 by locating its apsidal line as passing through 55° and 235° using the symmetry of pairs of equal and opposite greatest elongations; we have already noted that the reported observations are not genuine greatest elongations and are partly doctored, but here we can accept the result as a given. Comparing greatest elongations where the mean Sun was near either apsidal longitude, he establishes that 55° is the apogee and that the epicycle's radius is 43⅙ such that the deferent's radius is 60 (10.2); we will return pres-

41 The order of phrases in Ptolemy's Greek reinforces the impression that the method of deriving an eccentricity from retrogradations is to be thought of as independent of, and not subordinate to, the derivation from equations of center. The phrasing cannot be reproduced in literal translation, but this slight rewording conveys the effect: "… the eccentricity arising from the quantity of the retrogradations around the greatest and least distances of the epicycle is found to be approximately half that found by means of the greatest difference in the anomaly dependent on (the position in) the zodiac."

42 Jones 2004, 377-379.

43 This is how Swerdlow 2004 interprets the passage.

Ptolemy date	*mean Sun*	$\lambda_{♀}$	*elongation*	*Modern date*	*mean Sun*	$\lambda_{♀}$	*elongation*
134 Feb 18, AM	325½°	281 11/12°	43 7/12°	134 Feb 15, 8 UT	323.55°	279.24°	44.31°
140 Feb 18, PM	325½°	13⅚°	48⅓°	140 Feb 19, 20 UT	327.52°	15.77°	48.25°

Table 4. Two greatest elongations of Venus according to Ptolemy's data and modern theory.

ently to this part. Now he selects a pair of observed greatest elongations, one in either direction from the mean Sun, with the mean Sun being approximately 90° from the apogee at both dates; by hypothesis, this means that a perpendicular dropped from the epicycle's center to the apsidal line will intersect it at the center of uniform motion. Hence by a simple trigonometrical calculation involving the sum and difference of the elongations and the epicycle radius, he finds that the center of uniform motion is almost exactly 2½ units from the Earth such that the deferent's radius is 60.

The calculation is legitimate in principle, and moreover Ptolemy was able to find a pair of greatest elongations that met the criterion of having the mean Sun close to 90° from the apsidal line, which is not something that can be taken for granted.[44] In Table 4 we give the data for these events according to Ptolemy's report as well as according to the JPL ephemeris.[45]

The first thing that is apparent from this table is that Ptolemy's dates are very close indeed to the true dates of greatest elongation, close enough that the longitudes of Venus on Ptolemy's dates can be treated as longitudes at greatest elongation without introducing any significant error in the ensuing calculations. (Incidentally we can also see that Ptolemy was capable of determining dates of Venus's greatest elongation within a margin of very few days.) The second thing we notice is that Ptolemy's observed longitudes of Venus cannot both be accurate, since the sum of his elongations (which should be independent of the mean Sun) is approximately 91.92° whereas according to modern theory it is 92.56, a discrepancy of close to two-thirds of a degree. In fact, taking into account the systematic error of approximately −1° in Ptolemy's tropical longitudes for this range of years, it turns out that the longitude reported by Ptolemy for the earlier greatest elongation is about ⅔ ° too low, while the reported longitude for the later one is approximately correct.

Of course a measurement error of ⅔ ° is implausibly large, especially for an event that by its nature had to be determined by repeated observations on successive nights. It is also enough to make a significant difference in the calculated eccentricity of the center of uniform motion. If we repeat Ptolemy's computations using 44.31° and 48.25° as the given elongations, the eccentricity becomes 2.1 units instead of 2.5.[46] We may recall that our optimized eccentricity of deferent for an equant model of Venus was 0.83 (Table 2), so that the optimized eccentricity of the equant would be about 1.6. It is reasonable to suspect that 2.5 was a predetermined quantity (likely to match Ptolemy's solar eccentricity), and that Ptolemy has adjusted the longitude of one of his greatest elongations to get this result.

44 Since Venus performs five synodic cycles in almost exactly eight years, any particular synodic phenomenon (such as a greatest morning elongation) will only occur within a span of several decades with the mean Sun in five narrow and more or less equally spaced intervals of the ecliptic.

45 To estimate the mean Sun for the modern theory calculations, we use the Sun's mean longitude according to the Almagest solar theory plus 1° to compensate for the error in tropical frame of reference.

46 Shifting the assumed longitudes of the mean Sun by 0.1°, which affects only the difference between the elongations, changes the resulting eccentricity by about 0.1 units.

Ptolemy's method of finding the eccentricity of the deferent requires two observed greatest elongations, one with the mean Sun at Venus's apogee and the other with the mean Sun at the perigee; a trigonometrical calculation yields both the eccentricity and the epicycle's radius. Again the procedure is theoretically sound, but the observations that it requires could not have been made in the interval between AD 120 and the mid 140s that encompasses the observations that Ptolemy uses for his planetary theories. The observations that Ptolemy claims to be greatest elongations with the mean Sun at 55⅖° and 235½°, i.e. within a degree of the two ends of his apsidal line, are respectively 14 and 25 days distant from the true greatest elongations, and his reported longitudes have probably both been doctored, in the case of the latter one (AD 136 November 18) by more than a degree, to enlarge the elongations.[47] The elongations that he assigns can hardly be better than guesses, and while the elongation for his claimed observation with the mean Sun at perigee, 47⅓°, would be about right for an actual greatest elongation in this situation, the elongation for the claimed observation at apogee, 44⅘°, is about ¾° too small. If Ptolemy somehow had access to well-observed greatest elongations with the mean Sun truly close to the apogee and perigee—the most recent candidates would have been in the mid first century AD and the late first century BC respectively—he would have had about 45.6° and 47.3° for the elongations, leading to an eccentricity of deferent of approximately 0.85 units, in pretty good agreement with the eccentricity in our optimized model. The elongations that he gives in 10.2 must, however, have been chosen to yield predetermined values for both the eccentricity and epicycle radius.[48]

To sum up the situation, Ptolemy's approach to demonstrating the bisection of Venus's eccentricity depends on the availability of observations of Venus at its greatest elongations from the mean Sun when the mean Sun is in highly particular locations: at apogee, perigee, and 90° from the apsidal line. Only for the last of these conditions did suitable greatest elongations take place in Ptolemy's own time, whereas for the others he would have needed reports from as long as a century and a half earlier. But it is clear that he did not have these; otherwise why did he not use them explicitly in the *Almagest*, or at least make the simulated observations that he does cite agree with them? More generally, if Ptolemy's belief that Venus had an equant model with a bisected eccentricity was based on empirical evidence, how could it have come about that both his eccentricities, ostensibly found by independent analyses, are about 50% too large? I conclude that, although a viable procedure in the abstract, Ptolemy's deduction of the eccentricities in 10.2-3 is *for his circumstances* pseudoempirical. His observations may have sufficed to indicate that the eccentricity of the deferent is smaller than that of the center of uniform motion, but the choice of specific ratio must really have depended on analogy with the superior planets.

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47 The report for AD 129 May 20, which is attributed to Theon, is a rather accurate description of the location of Venus relative to two stars, but Ptolemy's reduction of the information to a longitude involves inaccuracies that deducted nearly half a degree.

48 Swerdlow 1989, 43 suggests that Ptolemy could have estimated the deferent's eccentricity by using the actual greatest elongations that occurred nearest to the false dates of greatest elongations used in 10.2, treating them as if they were really in the apsidal line; such a calculation does result in an eccentricity of about 1.3 units, close to Ptolemy's 1.25. But when the mean Sun is away from the apsidal line, one must use the sum of a pair of oppositely oriented greatest elongations, not just a single greatest elongation, to determine the distance and radius of the epicycle because the epicycle's center is assumed to revolve uniformly around the equant, not the Earth. Doing the calculation with the two actual greatest elongations nearest perigee and the two nearest apogee in Ptolemy's time, one would again get an elongation of about 0.8 units.

General remarks

Before turning to the general question of why Ptolemy incorporated pseudoempirical claims in the two treatises considered in this article, we may note in passing that all the examples we have discussed from the *Almagest* lead Ptolemy to conclusions that are, in a qualified sense, correct. The Sun, Moon, and planets really are spherical; the Sun really is furthest from the Earth when its apparent motion is slowest; when a planet's heliocentric revolution is optimally approximated in a geocentric frame of reference by an epicycle and eccenter model, the center of the epicycle really is farthest from the Earth too when its apparent motion is slowest; the apparent apsidal lines of these geocentric approximations really do precess in a tropical frame of reference at rates on the order of magnitude of the precession of the fixed stars; and bisecting the eccentricity yields a good fit for an equant model of Venus. On the other hand, Ptolemy's broad theoretical hypothesis for the *Harmonics*, that all pitches in the tuning systems of Greek music should correspond exactly to whole number ratios subject to certain constraints, would not be considered viable today, so we would not accept the conclusions that he draws from any of the comparisons of heard pitches described in *Harmonics* 2.1, including the problematic one we have discussed.

If we ask what were the hidden nonempirical reasons behind the choices that Ptolemy ostensibly justifies by pseudoempirical claims as well as why Ptolemy leaves these reasons hidden, the most plausible answers differ from case to case. His avoidance of same-sense epicycles to model the anomaly of the Sun and the zodiacal anomaly of the planets seems to be a case of preferring a simpler model. In comparison with the eccenter or even the opposite-sense epicycle model, the same-sense model produces the effect of anomaly in a rather perverse way, by slowing down the actual speed precisely where its proximity to the Earth should give an appearance of swiftest motion and *vice versa*. I suggest that Ptolemy did not offer such a simplicity argument in the *Almagest* because the models, though to the senses indistinguishable, are nevertheless kinematically distinct. It is an extreme case of the priority that Ptolemy attributes to empirical evidence over aprioristic reasoning, which he invokes elsewhere (*Almagest* 13.2) to justify adopting *complex* models. Here, where a choice of model suggested by considerations of simplicity could notionally be confirmed by observation if our means of observing were only sensitive enough, and is definitely not refuted by the observations we can make in reality, Ptolemy offers the reader the kind of positive evidence that *ought* to be available.

In other cases, analogy seems to have been the primary form of reasoning. Thus Ptolemy had a sound empirical basis for the bisection of the eccentricity of Mars, Jupiter, and Saturn, even if he was unable to present it as a fact deduced straightforwardly from observation reports so that he had to justify it as a case of "fitting" the phenomena to an otherwise incompletely motivated model; it would have appeared plausible to assume that the bisection applied also to Venus, for which the empirical evidence pertaining to the eccentricities was murky. However, it would have seemed desirable to offer the reader at least one clear demonstration of the bisection from observations, and Venus was a good candidate for this not just because it came before the superior planets in the order of presentation in *Almagest* Books 10-11, but because it was comparatively easy to design a procedure for notionally isolating the two kinds of eccentricity based on greatest elongations. As for the shapes of the visible heavenly bodies, the Moon's sphericity was obvious from the appearance of its phases, so one would expect the Sun, planets, and stars to be spherical too; but it might have seemed too bold to apply analogical reasoning twice in a row, to go from the Moon alone to all the visible bodies, and then from the visible bodies to the invisible etherial bodies composing the bulk of the heavens. Finally, the decision to have the planets' apsidal lines sidereally fixed looks like an essentially cautious move; from Ptolemy's point of view (with his commitment to a tropical frame of reference as the best description of reality), precessing apsidal lines were not the choice recommended by considerations of simplicity, but he may have been reluctant to posit a long-term behavior for them that was different from the consensus of earlier planetary models and tables in the absence of decisive empirical proof one way or the other.

Overlaying the particular considerations that may have led Ptolemy to introduce pseudoempirical arguments is the privileged status of mathematical science as he delineates it in *Almagest* 1.1. The two defining characteristics of mathematics, according to Ptolemy's account, are that its objects are intrinsically knowable as exact things (unlike the objects of physics) and, crucially, that through our senses we are able to grasp these objects (unlike those of theology). Ptolemy knows that there are limitations to our ability to know mathematical realities through our senses, and sometimes he admits it. But one senses that the severe standard set by his conception of what we would call "mathematical sciences" as "mathematics" *tout court*, together with the principle that all knowledge comes ultimately from the senses, drove him from time to time to push the boundary between secure reasoning based on the senses and plausible reasoning based on metaphysical considerations or consensus further in the direction of the senses than his circumstances allowed.

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