Book IX

1. {*On the order of the spheres of sun, moon and the 5 planets*}

Such, then, more or less, is the sum total of the chief topics one may mention as having to do with the fixed stars, in so far as the phenomena [observed] up to now provide the means of progress in our understanding. There remains, to [complete] our treatise, the treatment of the five planets. To avoid repetition we shall, as far as possible, explain the theory of the latter by means of an exposition common [to all five], treating each of the methods [for all planets] together.

First, then, [to discuss] the order of their spheres, which are all situated [with their poles] nearly coinciding with the poles of the inclined, ecliptic circle: we see that almost all the foremost astronomers agree that all the spheres are closer to the earth than that of the fixed stars, and farther from the earth than that of the moon, and that those of the three [outer planets] are farther from the earth than those of the other [two] and the sun, Saturn's being greatest, Jupiter's the next in order towards the earth, and Mars' below that. But concerning the H207 spheres of Venus and Mercury, we see that they are placed below the sun's by the more ancient astronomers, but by some of their successors these too are placed above [the sun's],[1] for the reason that the sun has never been obscured by them [Venus and Mercury] either. To us, however, such a criterion seems to have an element of uncertainty, since it is possible that some planets might indeed be below the sun, but nevertheless not always be in one of the planes through the sun and our viewpoint, but in another [plane], and hence might not be seen passing in front of it, just as in the case of the moon, when it passes below [the sun] at conjunction, no obscuration results in most cases.[2]

And since there is no other way, either, to make progress in our knowledge of this matter, since none of the stars[3] has a noticeable parallax (which is the only phenomenon from which the distances can be derived), the order assumed by the older [astronomers] appears the more plausible. For, by putting the sun in the middle, it is more in accordance with the nature [of the bodies] in thus

[1] There is a good deal of evidence for the identity of some of those who held the second opinion, including Plato, Eratosthenes and Archimedes. For details on this and other ancient arrangements see *HAMA* II 690-3.

[2] I.e. no transits of Venus or Mercury had been observed. Neugebauer has shown (*HAMA* 227-30) that transits are in fact predictable from Ptolemy's own theory. Ptolemy later seems to have realized this, for in the *Planetary Hypotheses* (ed. Goldstein 2,28,10–12) he says: 'if a body of such small size (as a planet) were to occult a body of such large size and with so much light (as the sun), it would necessarily be imperceptible, because of the smallness of the occulting body and the state of the parts of the sun's body which remain uncovered.' (Goldstein's translation here, p.6, is inaccurate).

[3] This includes both fixed stars and planets.

separating those which reach all possible distances from the sun and those which do not do so, but always move in its vicinity; provided only that it does not remove the latter close enough to the earth that there can result a parallax of any size.[4]

H208 2. {*On our purpose in the hypotheses of the planets*}

So much, then, for the arrangements of the spheres. Now it is our purpose to demonstrate for the five planets, just as we did for the sun and moon, that all their apparent anomalies can be represented by uniform circular motions, since these are proper to the nature of divine beings, while disorder and non-uniformity are alien [to such beings]. Then it is right that we should think success in such a purpose a great thing, and truly the proper end of the mathematical part of theoretical philosophy.[5] But, on many grounds, we must think that it is difficult, and that there is good reason why no-one before us has yet succeeded in it.[6] For, [firstly], in investigations of the periodic motions of a planet, the possible [inaccuracy] resulting from comparison of [two] observations (at each of which the observer may have committed a small observational error) will, when accumulated over a continuous period, produce a noticeable difference [from the true state] sooner when the interval [between the observations] over which the examination is made is shorter, and less soon when it is longer. But we have records of planetary observations only from a time which is recent in comparison with such a vast enterprise: this makes prediction for a time many times greater [than the interval for which observations are available] insecure. [Secondly], in investigation of the anomalies, considerable confusion stems from the fact that it is apparent that each planet exhibits two anomalies, which are moreover unequal both in their
H209 amount and in the periods of their return: one [return] is observed to be related to the sun, the other to the position in the ecliptic; but both anomalies are continuously combined, whence it is difficult to distinguish the characteristics of each individually. [It is] also [confusing] that most of the ancient [planetary] observations have been recorded in a way which is difficult to evaluate, and crude. For [1] the more continuous series of observations concern stations and phases [i.e. first and last visibilities].[7] But detection of both of these particular

[4] In his *Planetary Hypotheses* (see Goldstein's edition) Ptolemy proposes a system in which the spheres of the planets are contiguous; thus the greatest distance from the earth attained by a planet is equal to the least distance attained by the one next in order outwards. This appears to provide support for the order he adopts here, since it results in a solar distance very nearly the same as that obtained by a different method in *Almagest* V 15. Since this system also brings Mercury, at its least distance, to the moon's greatest distance (64 earth-radii), Mercury ought to exhibit a considerable parallax, contrary to what is enunciated here.

[5] Cf. I 1 p. 35.

[6] We cannot doubt that not only planetary theories but planetary tables had been constructed before Ptolemy: the proof is supplied by Indian astronomy, which is based on Greek theories which are largely, if not entirely, pre-Ptolemaic, and indeed by Ptolemy's own reference to the 'Aeon-tables' below (p. 422). What he means is that all previous efforts were, by his criteria, unsatisfactory

[7] Ptolemy is certainly thinking of the Babylonian planetary observations, which are characteristically of this type. They have become available to us through the 'diaries' (see Sachs[2]), but to Ptolemy were probably known only through Hipparchus' compilation (see p. 421).

phenomena is fraught with uncertainty: stations cannot be fixed at an exact moment, since the local motion of the planet for several days both before and after the actual station is too small to be observable; in the case of the phases, not only do the places [in which the planets are located] immediately become invisible together with the bodies which are undergoing their first or last visibility, but the times too can be in error, both because of atmospherical differences and because of differences in the [sharpness of] vision of the observers. [2] In general, observations [of planets] with respect to one of the fixed stars, when taken over a comparatively great distance, involve difficult computations and an element of guesswork in the quantity measured, unless one carries them out in a manner which is thoroughly competent and knowledgeable. This is not only because the lines joining the observed stars do not always form right angles with the ecliptic, but may form an angle of any size (hence one may expect considerable error in determining the position in latitude and longitude, due to H210
the varying inclination of the ecliptic [to the horizon frame of reference]); but also because the same interval [between star and planet] appears to the observer as greater near the horizon, and less near mid-heaven;[8] hence, obviously, the interval in question can be measured as at one time greater, at another less than it is in reality.

Hence it was, I think, that Hipparchus, being a great lover of truth, for all the above reasons, and especially because he did not yet have in his possession such a groundwork of resources in the form of accurate observations from earlier times as he himself has provided to us,[9] although he investigated the theories of the sun and moon, and, to the best of his ability, demonstrated with every means at his command that they are represented by uniform circular motions, did not even make a beginning in establishing theories for the five planets, not at least in his writings which have come down to us.[10] All that he did was to make a compilation of the planetary observations arranged in a more useful way,[11] and to show by means of these that the phenomena were not in agreement with the hypotheses of the astronomers of that time. For, we may presume, he thought that one must not only show that each planet has a twofold anomaly, or that each planet has retrograde arcs which are not constant, and are of such and such sizes (whereas the other astronomers had constructed their geometrical proofs on the basis of a single unvarying anomaly and retrograde arc); nor [that it was sufficient to show] that these anomalies can in fact be represented either

[8] This appears to be the only reference to the effect of refraction (if that is what it is) in the Almagest, despite its obvious relevance e.g. to the observations of Mercury's greatest elongations in IX 7. Ptolemy discusses it (as a theoretical problem) in some detail in *Optics* V 23–30 (ed. Lejeune 237–42).

[9] This seems to imply that Hipparchus recorded planetary observations of his own, which Ptolemy used to establish his theories. This may be true, but it is strange that Ptolemy cites not a single such observation by Hipparchus. Could Ptolemy mean merely that Hipparchus had not 'yet' assembled the compilation of earlier planetary observations which he mentions just below?

[10] The circulation of books in antiquity was so fortuitous that, even for one, like Ptolemy, who had access to the great resources of the libraries at Alexandria, this was a necessary *caveat*.

[11] I have little doubt that all the older planetary observations cited in the Almagest are derived from this compilation (cf. p. 452 n.66), and that part of Hipparchus' 'rearrangement' was to give their dates in the Egyptian calendar. For a similar service he rendered for the listing of lunar eclipses see *HAMA* 320–21.

H211 by means of eccentric circles or by circles concentric with the ecliptic, and carrying epicycles, or even by combining both, the ecliptic anomaly being of such and such a size, and the synodic anomaly of such and such (for these representations have been employed by almost all those who tried to exhibit the uniform circular motion by means of the so-called 'Aeon-tables',[12] but their attempts were faulty and at the same time lacked proofs: some of them did not achieve their object at all, the others only to a limited extent); but, [we may presume], he reckoned that one who has reached such a pitch of accuracy and love of truth throughout the mathematical sciences will not be content to stop at the above point, like the others who did not care [about the imperfections]; rather, that anyone who was to convince himself and his future audience must demonstrate the size and the period of each of the two anomalies by means of well-attested phenomena which everyone agrees on, must then combine both anomalies, and discover the position and order of the circles by which they are brought about, and the type of their motion; and finally must make practically all the phenomena fit the particular character of the arrangement of circles in his hypothesis. And this, I suspect, appeared difficult even to him.

The point of the above remarks was not to boast [of our own achievement]. Rather, if we are at any point compelled by the nature of our subject to use a procedure not in strict accordance with theory (for instance, when we carry out proofs using without further qualification the circles[13] described in the
H212 planetary spheres by the movement [of the body, i.e.] assuming that these circles lie in the plane of the ecliptic,[14] to simplify the course of the proof); or [if we are compelled] to make some basic assumptions which we arrived at not from some readily apparent principle, but from a long period of trial and application,[15] or to assume a type of motion or inclination of the circles which is not the same and unchanged for all planets;[16] we may [be allowed to] accede [to this compulsion], since we know that this kind of inexact procedure will not affect the end desired, provided that it is not going to result in any noticeable error; and we know too that assumptions made without proof, provided only that they are found to be in agreement with the phenomena, could not have been found without some careful methodological procedure, even if it is difficult

[12] διὰ τῆς καλουμένης αἰωνίου κανονοποιίας. In my opinion, Ptolemy is referring to a type of work in which the mean motions of the planets were represented by integer numbers of revolutions in some huge period, in which they all return to the beginning of the zodiac, and the planetary equations were calculated by a combination of epicycles or of eccentre and epicycle which was not reducible to a geometrically consistent kinematic model, i.e. to a class of Greek works which were the ancestors of the Indian siddhāntas. In this I am in agreement with van der Waerden, 'Ewige Tafeln', except that I believe that the αἰών implied by the title of these tables does not mean 'eternity' (cf. the conventional translation, 'Eternal Tables', which is philologically possible, but not necessary), but refers to the immense common period in which the planets return (cf. the Greek inscription of Keskinto, *HAMA* 698–705, and the Indian Mahāyuga). The other two references to these tables in antiquity (P. Lond. 130, see Neugebauer-van Hoesen, *Greek Horoscopes* p. 21, I 12–13, and Vettius Valens VI 1, ed. Kroll 243,8) are consistent with, but do not require, this interpretation.

[13] Literally 'as if the circles were bare [circles]'.

[14] Ptolemy in fact carries out all the proofs involving the longitudinal motions of the planets (in Bks. IX–XII) as if the motions lay in the plane of the ecliptic.

[15] The paradigm case of this is the introduction of the equant.

[16] E.g. the special model for the longitudinal motions of Mercury, or the special inclinations attributed to the inner planets for their latitudinal motions.

to explain how one came to conceive them (for, in general, the cause of first principles is, by nature, either non-existent or hard to describe); we know, finally, that some variety in the type of hypotheses associated with the circles [of the planets] cannot plausibly be considered strange or contrary to reason (especially since the phenomena exhibited by the actual planets are not alike [for all]); for, when uniform circular motion is preserved for all without exception, the individual phenomena are demonstrated in accordance with a principle which is more basic and more generally applicable than that of similarity of the hypotheses [for all planets].

The observations which we use for the various demonstrations are those H213
which are most likely to be reliable, namely [1] those in which there is observed actual contact or very close approach to a star or the moon, and especially [2] those made by means of the astrolabe instruments. [In these] the observer's line of vision is directed, as it were, by means of the sighting-holes on opposite sides of the rings, thus observing equal distances as equal arcs in all directions, and can accurately determine the position of the planet in question in latitude and longitude with respect to the ecliptic, by moving the ecliptic ring on the astrolabe, and the diametrically opposite sighting-holes on the rings[17] through the poles of the ecliptic, into alignment with the object observed.

3. {*On the periodic returns of the five planets*}[18]

Now that we have completed the above discussion, we will first set out, for each of the 5 planets, the smallest period in which it makes an approximate return in both anomalies, as computed by Hipparchus.[19] These [periods] have been corrected by us, on the basis of the comparison of their positions which became possible after we had demonstrated their anomalies, as we shall explain at that point.[20] However, we anticipate and put them here, so as to have the individual mean motions in longitude and anomaly set out in a convenient form for the H214
calculations of the anomalies. But it would in fact make no noticeable difference in those calculations[21] even if one used more roughly computed mean positions.

[17] It is not clear why the plural ('rings') is used (contrast the singular at V 1, H354,13). Although the sights are attached only to ring 1 in Fig. F (p. 218). Ptolemy is presumably referring to both ring 1 and ring 2, since ring 2 has first to be moved to the correct sighting position on the ecliptic ring (no. 3).

[18] See *HAMA* 150–2, Pedersen (270) has fallen into some confusion about Ptolemy's procedure: see Toomer[3] 144–5.

[19] If Ptolemy means, as we may presume, that the periods 'computed by Hipparchus' are the relationships in integers, '57 returns in anomaly correspond to 59 years and 2 revolutions in longitude', etc., then he seems ignorant of the fact that these are well-known (to us) Babylonian period relationships (for details see *HAMA* 151).

[20] This is a reference to the chapters on the 'corrections of the mean motions', IX 10, X 4, X9, XI 3 and XI7. The 'comparison' refers to the use in these chapters of *two* positions, separated by a long time-interval, to derive the mean motions. On the problem of the actual derivation of the corrections given here, and of the mean motions, see Appendix C.

[21] Ptolemy means that where he uses the mean motions in determining the eccentricity (e.g. X 7 p. 484) over the short periods involved (a few years) one could use quite crude parameters (e.g. the mean motions given by the uncorrected Babylonian periods) without seriously affecting the final result. He is right (see p. 484 n.33). The corrected mean motions are given here merely for convenience. Cf. the procedure for the lunar mean motion table, p. 179.

There are, as we said,[35] two types of motion which are simplest and at the same time sufficient for our purpose, [namely] that produced by circles eccentric to [the centre of] the ecliptic, and that produced by circles concentric with the ecliptic but carrying epicycles around. There are likewise two apparent anomalies for each planet: [1] that anomaly which varies according to its position in the ecliptic, and [2] that which varies according to its position relative to the sun.

For [2] we find, from a series of different [sun-planet] configurations observed round about the same part of the ecliptic,[36] that in the case of the five planets[37] the time from greatest speed to mean is always greater than the time from mean speed to least. Now this feature cannot be a consequence of the eccentric hypothesis, in which exactly the opposite occurs, since the greatest speed takes
H251 place at the perigee in the eccentric hypothesis, while the arc from the perigee to the point of mean speed is less than the arc from the latter to the apogee in both [eccentric and epicyclic] hypotheses. But it can occur as a consequence of the epicyclic hypothesis, however only when the greatest speed occurs, not at the perigee, as in the case of the moon, but at the apogee; that is to say, when the planet, starting from the apogee, moves, not as the moon does, in advance [with respect to the motion] of the universe, but instead towards the rear. Hence we use the epicyclic hypothesis to represent this kind of anomaly.[38]

But for [1], the anomaly which varies according to the position in the ecliptic, we find from [observations of] the arcs of the ecliptic between [successive] phases or [sun-planet] configurations of the same kind[39] that the opposite is true: the time from least speed to mean is always greater than the time from mean speed to greatest. This feature can indeed be a consequence of either of the two hypotheses (in the way we described in our discussion of the equivalence of the hypotheses at the beginning of our treatise on the sun [III 3]). But it is more appropriate to the eccentric hypothesis,[40] and that is the hypothesis which we actually use to represent this kind of anomaly, since, moreover, the other anomaly was found to be peculiar, so to speak, to the epicyclic hypothesis.

Now from prolonged application and comparison of observations of individual [planetary] positions with the results computed from the combin-
H252 ation of both [the above] hypotheses, we find that it will not work simply to assume[41] [as one has hitherto] that the plane in which we draw the eccentric

[35] III 3 p. 141.

[36] This eliminates the effect of the ecliptic anomaly. Examples would be observations of Mars at opposition, station and (by interpolation) conjunction all near the same point in the ecliptic.

[37] Excising καὶ before ἐπὶ τῶν πέντε πλανωμένων at H250,17. (καὶ was apparently omitted in the text translated by al-Ḥajjāj). One would have to translate Heiberg's text 'in the case of the five planets too' (as well as the sun and moon). But the situation is precisely the opposite for the sun and moon (see e.g. III 4 p. 153). Perhaps the whole phrase καὶ . . . πλανωμένων is an ancient interpolation.

[38] See Ptolemy's discussion of this point at III 3 p. 144–5. However, as Neugebauer points out (*HAMA* 149–50) it is perfectly possible for an eccentric model to represent the planetary motions, provided the apsidal line is allowed to move, and precisely this kind of eccentric model is described at XII 1, though even there Ptolemy restricts its applicability to the outer planets.

[39] This eliminates the effect of the synodic anomaly. Examples would be observations of oppositions of Mars in different parts of the ecliptic (as in X 7).

[40] Cf. III 4 p. 153, where Ptolemy prefers it on the ground that it is 'simpler'.

[41] Literally 'that the assumption that . . . cannot progress so simply'.

circles is stationary, and that the straight line through both centres (the centre of the [planet's] eccentre and the centre of the ecliptic), which defines apogee and perigee, remains at a constant distance from the solstitial and equinoctial points; nor [to assume] that the eccentre on which the epicycle centre is carried is identical with the eccentre with respect to the centre of which the epicycle makes its uniform revolution towards the rear, cutting off equal angles in equal times at [that centre]. Rather, we find that the apogee of the eccentre performs a slow motion towards the rear with respect to the solstices, which is uniform about the centre of the ecliptic, and comes to about the same for each planet as the amount determined for the sphere of the fixed stars, i.e. 1° in 100 years (at least, as far as can be estimated on the basis of available evidence). We find, too, that the epicycle centre is carried on an eccentre which, though equal in size to the eccentre which produces the anomaly, is not described about the same centre as the latter. For all planets except Mercury the centre [of the actual deferent] is the point bisecting the line joining the centre of the eccentre producing the anomaly to the centre of the ecliptic. For Mercury alone, [the centre of the deferent] is a point whose distance from the centre of the circle about which it rotates is equal to the distance of the latter point towards the apogee from the centre of the eccentre producing the anomaly, which in turn is the same distance towards the apogee from the point representing the observer; for also, in the case of this planet alone, we find that, just as for the moon, the H253 eccentre is rotated by [the movement of] the above-mentioned centre in the opposite sense to the epicycle, [i.e.] in the advance direction, one rotation per year. [This must be so] because the planet itself appears twice in the perigee in the course of one revolution, just as the moon appears twice in the perigee in one [synodic] month.

6. {*On the type of and difference between the hypotheses*}

One may more easily grasp the type of the hypotheses which we infer on the basis of the preceding [phenomena] from the following description.

First for that of the [four planets] other [than Mercury], imagine [Fig. 9.1] the eccentre ABG about centre D, with ADG as the diameter through D and the centre of the ecliptic; on this let E be taken as the centre of the ecliptic, i.e. the viewpoint of the observer, making A the apogee and G the perigee. Let DE be bisected at Z, and with centre Z and radius DA draw a circle HΘK, which must, clearly, be equal to ABG. Then on centre Θ draw the epicycle LM, and H254 join LΘMD.

First, then, although we assume that the plane of the eccentric circles is inclined to the plane of the ecliptic, and also that the plane of the epicycle is inclined to the plane of the eccentres, to account for the latitudinal motion of the planets, in accordance with what we shall demonstrate concerning that topic, nevertheless, for the motions in longitude, for the sake of convenience, let us imagine that all [those planes] lie in a single [plane], that of the ecliptic, since there will be no noticeable longitudinal difference, not at least when the inclinations are as small as those which will be brought to light for each of the