# THE DERIVATION AND FIRST DRAFT OF COPERNICUS'S PLANETARY THEORY
# A TRANSLATION OF THE COMMENTARIOLUS WITH COMMENTARY

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## INTRODUCTION

### THE COMMENTARIOLUS AND COPERNICUS'S EARLY PLANETARY THEORY

IN THE dedicatory preface of *De revolutionibus*, Copernicus tells Pope Paul III and any interested reader of his great reluctance to publish the theory of the motion of the earth for fear of ridicule by an ignorant public. His apprehension, he explains, was so great that he was almost driven to giving up his work altogether, but at last the entreaties of his friends convinced him to publish the results of the investigations that he had concealed "not for just nine years, but already into the fourth nine-year period." Whatever this vague chronology may tell us about the period that elapsed from the time that the theory of the earth's motion first occurred to Copernicus to the time that he wrote the preface, or perhaps to the time that his friends convinced him to publish his work, it does not exclude the possibility that at some time in the past, possibly at the first moment of discovery, Copernicus had privately communicated a description of his new theories. Indeed, there is no doubt that he did so.

Copernicus certainly did nothing in his later work to advertise either the existence or authorship of such a treatise, and it would probably have vanished entirely had not Tycho Brahe received a copy in 1575 and afterwards had other copies prepared and sent to various astronomers in Germany.[1] But even Tycho's efforts nearly failed to save Copernicus's first draft of his planetary theory, for the treatise seems to have disappeared from sight in the seventeenth century and was not rediscovered until an incomplete manuscript was found in Vienna in 1877. Within three years a second, this time complete, manuscript was found in Stockholm, and only a little over ten years ago a third manuscript was located in Aberdeen. All three are probably descended from Tycho's copy, are far removed from the original, and preserve a faulty, possibly an exceedingly faulty, text.

The treatise, scarcely eight folios in length, is called in the manuscripts *Nicolai Copernici de hypothesibus motuum coelestium a se constitutis commentariolus* (A Brief Description by Nicolaus Copernicus Concerning the Models of the Motions of the Heavens That He Invented), which differs but little from Tycho's reference to what Copernicus did in *Tractatulo quodam de Hypothesibus a se constitutis*. The origin of the title is unknown. Since it refers to models that Copernicus *invented himself*, it can hardly be by Copernicus so was either placed on the manuscript given to Tycho or is by Tycho himself. That Copernicus left the treatise untitled and unsigned is by no means certain even though the only known possible reference to it during his life specifies neither author nor title. He may in fact have called it something like *De hypothesibus motuum coelestium*, but there is no evidence for any speculation. The work is today generally known as the *Commentariolus*.

In form the *Commentariolus* is simple, direct, and shows no waste of words. Indeed we can only wish that Copernicus had written more, for he says next to nothing about how he arrived at his new theories. He begins with a single principle governing planetary theory, and then raises objections to the theories of his predecessors. Next he explains that he has invented a planetary theory in conformity with his first principle, and this is followed by a set of seven postulates. These have almost nothing to do with either the principle or the objections, but instead assert the surprising theory that the earth and planets revolve around the sun and give some further consequences of this theory. He states next that he is planning a larger book to show that his theory both conforms with his first principle and is consistent with computations and observations, but, he remarks, any compe-

[1] Dreyer, 1890: p. 83.

tent astronomer will be able to figure this out for himself. Then he returns to his assertion of the motion of the earth, and defends it against criticism. This concludes his introduction. Following this he gives the order of the planets without bothering to mention that he has offered a solution to a problem that had plagued astronomy since its inception. He then describes his models for the motions of the earth, moon, and planets in longitude and latitude, and concludes by counting up the circles used in the models.

Copernicus's first principle of planetary theory is that any representation of the apparent motions of the planets must be composed exclusively of uniform circular motions. This principle is itself a deduction from the assumption that all planetary motions are controlled by the rotation of spheres. The only motion permitted to a sphere is a uniform rotation about its diameter; it can neither rotate uniformly with respect to any other line nor rotate with a non-uniform velocity. Copernicus holds that planetary models not adhering strictly to uniform circular motion are imperfect, or more precisely, are mechanically impossible since they cannot correspond to the rotation of spheres.

Now, the principal textbook of planetary theory in Copernicus's time, the *Theoricae novae planetarum* by Georg Peurbach, contains descriptions, and very elaborate descriptions they are too, of spherical representations of Ptolemy's planetary models. Peurbach gives exceedingly careful attention to the proper alignment of the eccentric sphere that carries the epicyclic sphere through its proper path, to the inclinations of the axes about which the spheres rotate, and to all the different motions of the spheres and axes required to produce the apparent planetary motions in longitude and latitude, and the precession of the apsidal and nodal lines along with the sphere of the fixed stars. This entire apparatus of spheres and axes, rotations and inclinations, is taken over by Copernicus in the *Commentariolus*, although his description is not as thorough since the reader's familiarity with such models is taken for granted. Copernicus usually describes planetary motions in terms of rotations of spheres and inclinations of axes.

The models in the *Theoricae novae planetarum*, however, do not strictly preserve uniform circular motion since they are based on Ptolemy's models which themselves violate this principle. The most important violation is the bisection of the eccentricity of the sphere carrying the planet's epicycle in the representation of the first anomaly so that the center of the epicycle remains at a constant distance from the diameter of its sphere while the sphere itself rotates uniformly with respect to a point lying on a line removed from the diameter. It was this flaw in received planetary theory that Copernicus desired above all to eliminate; indeed, he explains in the *Commentariolus* that this was the problem that initiated his investigations.

Nearly two hundred years earlier, a number of Islamic astronomers of the Marāgha school raised the same objection to Ptolemy's models, specifically to the spherical models described by Ptolemy in his *Planetary Hypotheses*. The Marāgha astronomers developed various alternatives to Ptolemy's bisected eccentricity that preserve uniform spherical rotation. The planetary theory of one in particular, Ibn ash-Shāṭir (1304–1375/6) of Damascus, contains exactly the same replacement of the bisected eccentricity by two epicycles found in the *Commentariolus*. Further, Ibn ash-Shāṭir's lunar theory is also identical to that of Copernicus, and several of the Marāgha astronomers made use of two devices for the generation of rectilinear motion from two circular motions also used by Copernicus. The work of the Marāgha astronomers, while extremely interesting in itself, is probably the most important and exciting discovery for the study of Copernicus.[2] It has, however, raised more questions than it has answered, for no intermediary showing the transmission of the Marāgha theory to the west has yet been discovered. Recently an example of the use of one of the Marāgha devices has been found in an Italian Aristotelian treatise on planetary theory that is contemporary with and independent of Copernicus.[3] The search for the

[2] The principal literature on Marāgha planetary theory is Carra de Vaux (1893); Roberts (1957); Kennedy and Roberts (1959); Abbud (1962); Roberts (1966); Kennedy (1966); Hartner (1969).

[3] Swerdlow (1972). In MS Vat. Gr. 211, f. 116r, O. Neugebauer has found figures of a model using Ṭūsī's device for generating rectilinear motion from a circle rolling on the internal circumference of a circle of twice its radius (see below, p. 504). Amico uses the other method of generating rectilinear motion (see below p. 489), and Copernicus uses both. All of this suggests that Italy, where Copernicus lived for most of the period from 1496 to 1503, is the place to look for further evidence of the transmission of the Marāgha planetary theory. I would not be surprised if there exists in Italy a Latin treatise from the late fifteenth century describing these models and cataloged, if at all, under the uninformative title *Theorica planetarum*.

transmission of this material from the Near East to, I think, Italy, perhaps by way of Byzantine sources, is a critical problem in Copernican scholarship.

Of still more importance than Copernicus's possible adaptation of medieval Arab mechanisms for preserving uniform circular motion is his introduction in the *Commentariolus* of the heliocentric theory. As mentioned earlier, he does this in a startling way, giving (correctly) as the only evidence for his assertion the equivalence of heliocentric to geocentric planetary theory and the additional sense of the heliocentric representation of the second anomaly and the order of the planetary spheres. This really has nothing to do with the principle of uniform circular motion that started Copernicus's investigations in the first place, but it seems likely that in the course of the intensive study of planetary theory undertaken to solve the problem of the first anomaly, he carried out an analysis of the second anomaly leading to his remarkable discovery.

Neither in the *Commentariolus* nor in *De revolutionibus* does Copernicus explain his analysis. However, in a page of manuscript notes containing the numerical parameters of the models in the *Commentariolus* there is very good, perhaps positive, evidence for the precise course of Copernicus's analysis of the second anomaly and derivation of the heliocentric theory. A fair amount of this paper will be devoted to this document, which I call U since it is contained in one of Copernicus's books now in the University Library of Uppsala. The evidence provided by U shows that Copernicus was investigating an alternative eccentric model of the second anomaly that was described in detail by Regiomontanus in Book XII of the *Epitome of the Almagest.* The model leads directly to the heliocentric theory, although its two forms for the superior and inferior planets lead respectively to the Tychonic and Copernican theories. This opens up the interesting question of why Copernicus chose one rather than the other, and the answer to this may lie in the intersection of the spheres of Mars and the mean sun in the Tychonic theory, which is not permissible in models composed of rotating spheres and so would provide a good reason for Copernicus's rejecting the Tychonic theory. We are thus brought back full circle to the fundamental assumption of rotating spheres.

U provides still more material for the understanding of Copernicus's original planetary theory in that it gives not only the parameters of the models in the *Commentariolus*, but also the numbers from which the finished parameters were computed. These numbers are the sines of the maximum equations of center and of the anomaly in the *Alphonsine Tables*, and thus show that Copernicus derived his parameters by simple extraction from the *Alphonsine Tables*, which, incidently, is one of the two books bound in with U. This eliminates once and for all the belief that Copernicus carried out any sort of independent derivation of the *Commentariolus* parameters from observation.

The sources of Copernicus's early planetary theory are relatively few. The derivation of the models for both first and second anomalies and almost the entire contents of the *Commentariolus* seem to depend on three certain and two possible sources. They are the following:

1. Peurbach, *Theoricae novae planetarum*. As mentioned earlier, the spherical models of Peurbach's textbook are carried over by Copernicus even though he tries to improve them in order to preserve uniform rotations. Copernicus, like everyone else in his time, probably first learned his planetary theory from the *Theoricae novae*.

2. Peurbach and Regiomontanus, The *Epitome of the Almagest.* This was begun by Peurbach, who had written the first six books at the time of his death in 1461, and completed by Regiomontanus in 1462 or 1463. The first six books follow closely the so-called *Almagestum minor*, an exposition of the first six books of the *Almagest* with additional material drawn from al-Battānī, Thābit ibn Qurra, az-Zarqāl, and the *Toledan Tables.* This unpublished work, which is found in a number of manuscripts with attributions to several unlikely authors, probably dates from the late thirteenth century. I suspect that Regiomontanus not only wrote Books VII–XIII of the *Epitome*, but also revised Peurbach's version of Books I–VI.[4] The result was the

[4] In particular, Ptolemy's preface (*Almagest* I, 1–2) and Propositions, or more accurately, *Conclusiones* 1–6 of Book I of the *Epitome* (= *Almagest* I, 3–8) are not found in the *Almagestum minor* and differ greatly in vocabulary from the Gerard of Cremona translation, appearing rather to be partially translation (especially the preface) and partially paraphrase from the Greek text. This part may well have been written by Regiomontanus. The rest of Books I–VI

finest textbook of Ptolemaic astronomy ever written, and the *Epitome* itself is the true rediscovery of ancient astronomy in the Renaissance. It was printed in Venice in 1496; the edition is very bad. This was the book that Copernicus followed, even in preference to the *Almagest*, in the writing of *De revolutionibus* which is filled with not only information and procedures, but even with close paraphrases from the *Epitome*.[5] In the *Commentariolus* the use of the *Epitome* can be seen most clearly in the section on the length of the tropical and sidereal year and the rate of precession, but, as will often be pointed out in the commentary, the *Epitome* is pertinent to many parts of the *Commentariolus*. Of greatest importance for our purpose, however, are Propositions 1 and 2 of Book XII, which contain the analysis leading to the heliocentric theory.

3. The *Alphonsine Tables*. U is bound in with the 1492 Venice edition of the *Alphonsine Tables* and the 1490 Augsburg edition of Regiomontanus's *Tabulae directionum*. The former is the source of the numerical parameters in U and the *Commentariolus*. Copernicus appears to have used the sine tables in the *Tabulae directionum* for the computation of the parameters for Mercury in the upper part of U.

4. This source is speculative. I strongly suspect that Copernicus had access to some account of the Marāgha planetary theory. Possibly he came across it in Italy, which seems a reasonable place to look for such a treatise.

5. The *Almagest*. Copernicus owned a copy of the 1515 Venice printing of the Gerard of Cremona translation of the *Almagest*. The problem is, was the *Commentariolus* written before he acquired this book? I think it was, but in his copy Copernicus wrote a value for the length of the sidereal year that may well show the precise number underlying the rough number in the *Commentariolus*. Then the maximum elongation of Venus he gives in the *Commentariolus* could follow from the table in *Almagest* XII, 9, which is not contained in the *Epitome*. This, however, is not much evidence, and there is more reason, usually depending upon errors and omissions, to believe that Copernicus was not familiar with the *Almagest* when he wrote the *Commentariolus*.

### U—THE UPPSALA NOTES

A volume with the shelf mark 34.VII.65 in the University Library of Uppsala contains Copernicus's own copies of:

1. *Tabule astronomice Alfonsi Regis* . . . Johann Hamman de Landoia, Venetiis, 1492 (Collijn, Nr. 83).
2. *Tabulae directionum profectionumque famosissimi viri Joannis Germani de Regiomonte* . . . Erhard Ratdolt, Auguste vindelicorum, 1490 (Collijn, Nr. 844).

These two books together provide the working library for an astronomer of Copernicus's time —the *Tabulae directionum* for spherical astronomy, the *Alphonsine Tables* for planetary theory and eclipses—and it is proper that he had them bound together. Bound in at the end of the volume are sixteen leaves containing tables and various notes. Most of this material was originally published by Curtze (1874: pp. 452–458; 1875: pp. 221–244), not very accurately, and reprinted by Prowe (**2**: pp. 206–244) with the addition of typographical errors. Part of it has been analyzed by L. Birkenmajer (pp. 154–210) and Neugebauer ($1968_2$).

The largest section is made up of a set of planetary latitude tables at 1° intervals which will be utilized later in the commentary on Copernicus's latitude theory. There are also tables for the computation of mean lunar conjunctions and oppositions along with the lunar longitude, anomaly, and argument of latitude, and a table of the motion of the solar apogee. These were copied by Copernicus from Peurbach's *Tabulae*

is much closer to the *Almagestum minor* and the Gerard of Cremona version, and is probably Peurbach's work, although here too I think Regiomontanus carried out revisions, possibly very extensively. Books VII–XIII contain some errors in numbers common to Gerard of Cremona, and what is more interesting, conjectural emendations and recomputations by Regiomontanus that differ from the Greek text, so it seems that here he was for the most part relying on Gerard of Cremona and doing his best to correct errors.

[5] The importance of the *Epitome* not only for the study of Copernicus, but for all sixteenth- and early seventeenth-century astronomy cannot be overemphasized, nor can its virtues be sufficiently praised. Much that is difficult in Ptolemy becomes clear in Regiomontanus's presentation. Although there are occasional errors, his understanding of Ptolemy is complete and profound to a depth not seen again until Tycho, Viète, Maestlin, and Kepler. Studying the *Epitome* makes one realize what a loss Regiomontanus's early death was to astronomy—a loss not made up for well over a century.

*eclipsium* which are entirely based on the *Alphonsine Tables.* Peurbach's tables were printed in Vienna in 1514, and since it is reasonable to assume that Copernicus would not waste his time copying from a printed book that he could buy, he probably copied from a manuscript before the printed tables were available.[6] There is also a table of the solar equation at 1° intervals, which seems to be formed from the difference column in the solar equation table in Peurbach's eclipse tables, which are at 0;10° intervals. Copernicus added a column giving $\frac{\delta}{\delta_{max}}$ to minutes where $\delta$ is a partial and $\delta_{max}$ the maximum equation.

Among the notes are records on folio 16v of two conjunctions of the moon and Saturn on January 9 and March 4 of 1500 which are of some interest for dating this material. They are as follows:

1500
die nona Januarij hora noctis fere secunda fuit ☌☽♄ in 15 42 ♉

hoc modo $\overset{*}{☽}$ bononie

Quarta Martij hora fere prima noctis fuit ☌☽♄ in 18 28 ♉ fuitque tunc ☽ in altitudine visa 35 et altius visa * quae est in ore ♈ 21 gradus $\overset{*}{☽}$ bononie

(On the ninth day of January at about the second hour of night there was a conjunction of the moon and Saturn in Taurus 15;42° in this way $\overset{*}{☽}$ at Bologna.
On the fourth of March at about the first hour of night there was a conjunction of the moon and Saturn in Taurus 18;28°, and the moon was then seen at an altitude of 35° and the star that is in the mouth of Aries was seen 21° higher $\overset{*}{☽}$ at Bologna.)

Recomputation with the *Alphonsine Tables* gives:

| | λ ☾ | λ ♄ |
|---|---|---|
| Bologna, 9 January, 1500 8:00 P.M. | 44;49° | 45;38° |
| Bologna, 4 March, 1500 7:00 P.M. | 46;44° | 48;29° |

The exact longitudes in Copernicus's notes, which were certainly computed, must refer to Saturn rather than the moon, although there is a discrepancy of 0;4° in the position for January 9. Stoeffler's *Almanach* for 1500 gives the longitude of Saturn, for 9 January Taurus 15;42° and for 4 March Taurus 18;27°, a discrepancy of only 0;1° for the second conjunction. The notes, however, certainly read like reports of observations of the two conjunctions. If Copernicus himself made the observations, then he could have recorded them here in 1500. This suggests an early date for these notes.

However, dating what Copernicus wrote on these pages is not at all so easy. On folio 15r, he wrote the following:

| | | | | |
|---|---|---|---|---|
| Saturnj apogeum | 240 21 | Anno 1527 | ♏ | 7 |
| Jovis apoge[um] | 159 0 | Anno 1529 | ♎ | 27 |
| Martis | 119 40 | Anno 1523 | ♌ | 27 |
| Veneris | 48 30 | Anno 1532 | ♊ | 16 |

The first column contains the sidereal longitudes of the "apogees" derived by Copernicus in *De revolutionibus* V: 6, 11, 16, 20, although the longitude for Venus should be 48;20°, a value Copernicus simply took over from Ptolemy. The second column contains the tropical longitudes of the northern limits of latitude of Saturn, Jupiter, and Mars (as in *De rev.* VI, 1), and the tropical longitude of the ascending node of Venus at the given dates. These were computed by Copernicus from Ptolemy's distance between apogee and northern limit for each planet and the value of precession at each date. The dates, with the exception of Venus, are those of the oppositions used by Copernicus to determine the mean motions and epochs in *De rev.* V: 7, 12, 17. In the case of Venus he used a 1529 observation to determine these parameters; what became of a 1532 observation is unknown. In any case, it is evident that these notes could not have been written before 1532, so on two adjacent folios Copernicus seems to have made notes more than thirty-two years apart.

[6] In fact a volume from Frauenburg, Uppsala University Library XVII.Ba.7945, contains the printed edition, which also includes Regiomontanus's *Tabula primi mobilis*, along with manuscripts of Bianchini's tables and John Pecham's *Perspectiva communis* copied by one Martinus de Grodzyszko in 1522–1523 (Curtze, 1878: pp. 43–44). Curtze identifies a report of a lunar eclipse on 4 July, 1525, written on the inside front cover as being in Copernicus's hand, but L. Birkenmajer (1900: p. 466) denies this identification. Curiously neither Curtze nor Birkenmajer noticed that Copernicus had copied Peurbach's tables even though they had looked at both Copernicus's manuscript and the printed tables.

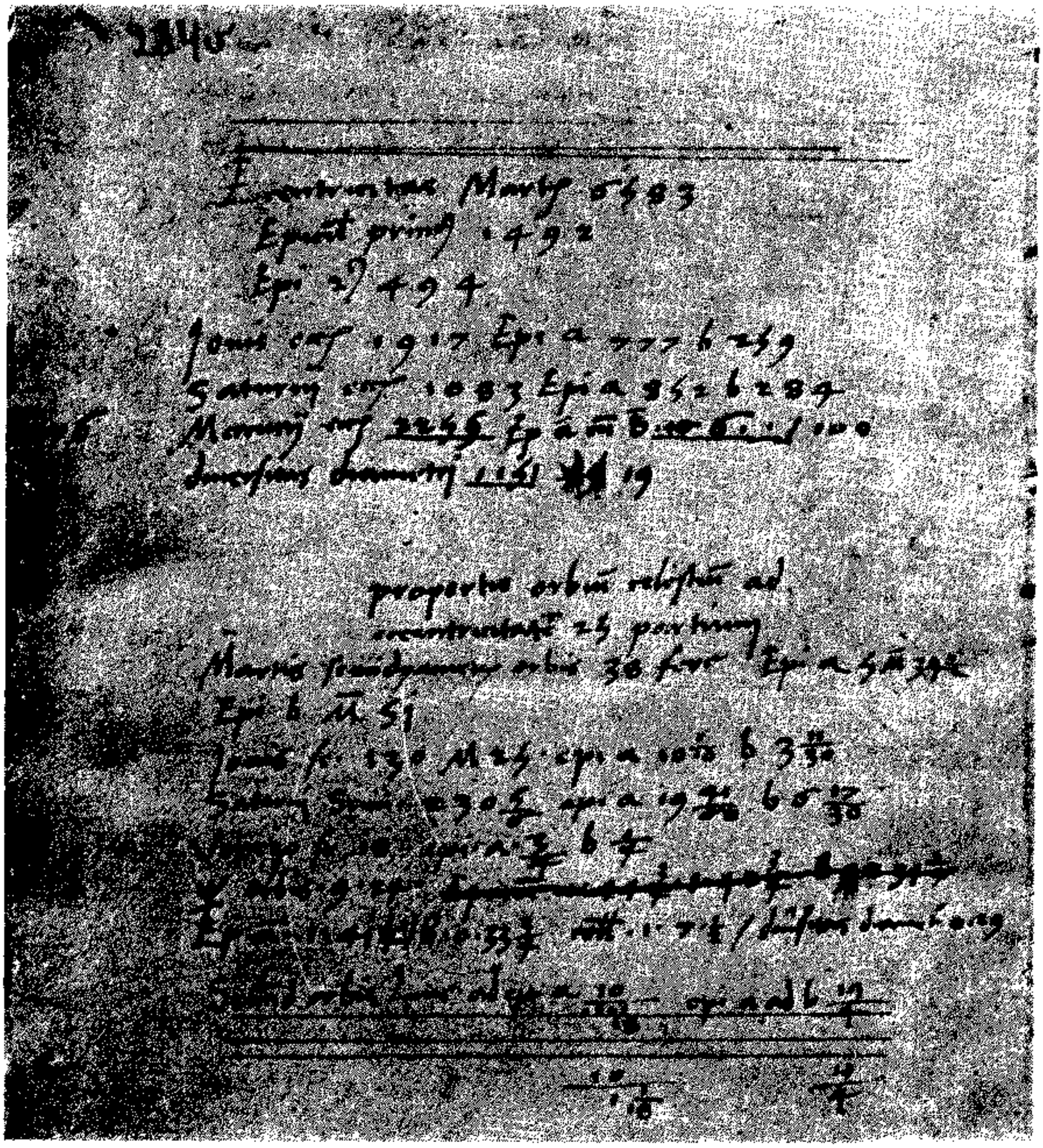

Plate 1

The notes that interest us are on folio 15v, that is, on the verso of the page listing the apogees and northern limits. Since 15r was written after 1532, and 16v could have been written in 1500, it is certainly difficult to determine when these notes on 15v were written. A facsimile is given in plate 1, and a transcription given below.[7] Crossed out material is in italics. Explanation of the contents of U will be taken up in the commentary.

[7] I would like to thank the University Library of Uppsala for providing this photograph and permission to reproduce it here.

Eccentricitas Martis 6583
Epiciclus primus 1492
Epi[cyclus] secundus 494
Jovis ecce[ntricitas] 1917 Epi[cyclus] a 777 b 259
Saturnij ecce[ntricitas] 1083 Epi[cyclus] a 852 b 284
376 Mercurij ecce[ntricitas] 2256 [2259?] Ep[icyclus] a cum b ·*10*·6· ·/ 100
diversitas diametrj 1151 *59* 19

proportio orbium celestium ad
eccentricitatem 25 partium

Martis semidyameter orbis 38 fere Epi[cyclus] a 5 $\bar{M}$ 34
Epi[cyclus] b $\bar{M}$ 51
Jovis se[midiameter] 130 M 25·epi[cyclus] a $10\frac{1}{10}$ b $3\frac{11}{30}$
Saturnij Semi[diameter] $230\frac{5}{6}$ epi[cyclus] a $19\frac{41}{60}$ b $6\frac{17}{30}$
Veneris se[midiameter] 18·epi[cyclus]·a·$\frac{3}{4}$ b $\frac{1}{4}$
☿ orbis·9·24·*Epi[cyclus] a 1·44*$\frac{3}{4}$*·1·42*$\frac{3}{4}$*·b 0 34*$\frac{1}{4}$
Ep[icyclus] a·1·41$\frac{1}{4}$/b·0·33$\frac{3}{4}$ coll[igunt?]·1·7·$\frac{1}{2}$/ diversitas diametri 0·29
Semid[iameter] orbis Lune ad ep[icyclum] a $\frac{10}{1\frac{1}{18}}$ epi[cyclus] a ad b $\frac{19}{4}$

$\frac{10}{1\frac{1}{18}}$ $\frac{19}{4}$

## THE DATE OF THE COMMENTARIOLUS

Dating the *Commentariolus* and dating U probably mean dating Copernicus's derivation of the heliocentric theory. The parameters in the *Commentariolus* are taken from the lower part of U; not *vice versa* because the radii of Mercury's epicycles in the *Commentariolus* are rounded from the values in U. Further, the numbers in the lower part of U are derived from the numbers in the upper part. U is therefore prior to the *Commentariolus*. Further, U may even be prior to the completed heliocentric theory of the *Commentariolus* since it shows that the center of the planetary orbits is the mean sun but does not show whether the earth revolves around the mean sun or the mean sun around the earth. This will be taken up in the commentary. The *Commentariolus* also displays a good deal of carelessness and incomplete understanding on Copernicus's part. Examples of this are the precession, the lunar latitude theory, the latitude theory of Venus and Mercury, and the description of the variation of the radius of Mercury's orbit, all of which will be taken up in the commentary. These problems suggest that the *Commentariolus* was written in haste, possibly very shortly after Copernicus developed his new theories and before he correctly understood all the necessary details of his models.[8], Thus the derivation of the heliocentric theory, the writing of U, and the writing of the *Commentariolus* probably came very close together, possibly within a few months or even weeks. If we can date one, we can date all three. But how do we do this?

We have seen that the contents of the manuscript containing U are useless because they may run all the way from 1500 to after 1532. The watermark of these pages, an ox head surmounted

[8] These are real errors on Copernicus's part, and cannot be explained away by appealing to his statement that he is deferring mathematical demonstrations for a larger book (see below, p. 438). They show not only that he may have written the *Commentariolus* very rapidly before he understood a number of details of his theory, but also, in the case of his false description of the apparent motion of Mercury, that he may well have copied the Mercury model from some other treatise without fully understanding its relation to Mercury's apparent motion (see below, p. 504).

by a cross-staff with an entwined serpent (illustrated by L. Birkenmajer, p. 155), is of a very common type used in Germany from *ca.* 1475 to 1530 and in northern Italy during at least the last quarter of the fifteenth century. But if the observations on 16v were recorded in 1500, which is already earlier than anyone of reason imagines Copernicus developed his new theories, the paper must be earlier still, so a positive date of the paper would probably offer no help.

Therefore, we must try to date the *Commentariolus*, and we must look for an earliest possible date and a latest possible date. Observations are of no use in establishing an earliest date since nothing in the *Commentariolus* depends on any observation made by Copernicus with the possible exception of one showing that Ptolemy's lunar parallax at quadrature is too large. There is such an observation made by Copernicus in Bologna in 1497 and cited in *De rev.* IV, 27. But this has nothing to do with the heliocentric theory so it is useless for our purpose.

A latest possible date might be derived from the sidereally fixed apsidal line of the earth in the *Commentariolus.* In connection with observations made in 1515 and 1516 (*De rev.* III, 13) Copernicus deduced that the apsidal line moves non-uniformly with respect to the fixed stars (III: 20, 22). He also found a corresponding variation in the eccentricity (III: 16, 21) which is not mentioned in the *Commentariolus* where the eccentricity is merely rounded from the sine of the maximum equation of center in the *Alphonsine Tables.* This might lead one to suggest that the *Commentariolus* was written before 1515/16. However, making observations is one thing, and reducing and using them for the derivation of parameters is something else. The distinction of the irregular motion of the apsidal line from the precession depends not only upon the 1515/16 observations, but also upon Copernicus's fully developed precession theory. But Copernicus's precession theory, which is connected to his theory of the variation of the obliquity of the ecliptic, depends specifically upon observational records in Johann Werner's *De motu octavae sphaerae.*[9] Werner's book was published in 1522 and Copernicus's criticisms of it are from 1524. Therefore, Copernicus could not have derived the motion of the earth's apsidal line as presented in *De revolutionibus* until after 1522 or 1524. Further, (1) his theory of the motion of the apsidal line depends more upon the longitudes of the solar apogee given in *Epitome* III, 13 than upon his own derivation from the observations of 1515/16 (see below p. 443 n. 2), and (2) his period for the irregular motion and change of eccentricity is merely the period of the irregular precession and change of obliquity. There is no observational evidence whatsoever for transferring this period from the precession and obliquity to the solar model. Copernicus was doing little more than guessing, and hoping that it would work. All this means that the 1515/16 observations are useless for assigning a latest date to the *Commentariolus*, and further, Copernicus may have made no use of them for eight or ten years. Incidently, if he was doing his job properly, he should have been making observations of equinoxes every year without fail, so the choice of the 1515/16 observations in *De revolutionibus* could be fortuitous anyway.

Since observations do not help to date the *Commentariolus* one way or the other, we must look elsewhere. The books used by Copernicus offer no help. The *Alphonsine Tables*, *Tabulae directionum*, and the *Epitome* were all printed before the end of the fifteenth century so are too early to be of significance. Zacuto's *Almanach perpetuum* with the additions by Alfonso de Cordoba Hispalensis was printed in 1502, but I do not think Copernicus made any use of this work in the *Commentariolus* (see below, p. 452). The Latin *Almagest* was printed in 1515; although it seems unlikely that Copernicus had studied it when he wrote the *Commentariolus*, it is possible that he used it in some small way (see pp. 453, 493), so there are difficulties in using its publication date as evidence. If indeed he knew a treatise explaining the Marāgha planetary theory, I think he would have found it during his stay in Italy from 1496 to 1503, but one cannot argue for a date on the basis of a treatise that may not exist, and the Marāgha theory is, in any case, relevant only to the first anomaly, not to the heliocentric theory.

So far I have been discussing and, I hope, doing away with the plausible arguments for dating the *Commentariolus.* Some were hypothetical; some have been proposed before. I will not bother with a fair number of implausible, highly circumstantial arguments, usually from omission, found elsewhere.

[9] An analysis of this will appear in a collection of papers on Copernicus to be published by the University of California Press in 1974 under the editorship of Robert S. Westman.

There is, however, one piece of admittedly circumstantial evidence for dating the *Commentariolus*, discovered by L. Birkenmajer and discussed briefly by A. Birkenmajer (pp. 95–96), Zinner (1943: pp. 185–186), Rossmann (p. 31), and Rosen (1971: pp. 67n., 343) that appears to be of great value. It is a brief entry in a catalog of books and manuscripts in the library of the Cracow physician and professor of medicine Matthew of Miechów dated 1 May, 1514, mentioning "*Item sexternus Theorice asserentis Terram moveri, Solem vero quiescere*" (next a six-folio [gathering? manuscript?] of a *Theorica* maintaining that the earth moves while the sun is at rest). It is hard to believe that this could refer to anything other than the *Commentariolus* for then we would be faced with the remarkable possibility that there was someone other than Copernicus who wrote a description of the heliocentric theory that found its way to Cracow by 1514. This is so implausible that we can be virtually certain that the catalog entry does in fact describe the *Commentariolus*, which must therefore have been written in early 1514 at the very latest, and thus obviously before the 1515/16 observations. Incidently, the entry also suggests the possibility that Copernicus called his treatise something like *Theorica planetarum*.

As for an earliest possible date, there is nothing but empty speculation since the only evidence is the absence of any evidence, and this does not count for much. This at last brings us to Copernicus's remark in *De revolutionibus* that he had concealed his work "into the fourth nine-year period." If we take this literally, it means more than twenty-seven but less than thirty-six years. But from when do we count backwards? Copernicus wrote the dedication in 1542 (Rosen, p. 401), but his statement is that Tiedemann Giese frequently encouraged him to publish what he had concealed into the fourth nine-year period. When or for how long did Giese encourage him? The latest date indicated would be 1515, but Miechów's catalog gives a date a year before this, and the statement is too vague to give an earliest date. I think there is insufficient evidence to determine how long before 1514 Copernicus developed his new planetary theory.

## TEXTS, TRANSLATIONS—PRINCIPLES OF THIS TRANSLATION AND COMMENTARY

The first manuscript of the *Commentariolus* was discovered in Vienna in Österreichische Nationalbibliothek MS. 10530, and published by Curtze in 1878. It is lacking a folio containing most of the lunar theory. A second, fortunately complete, manuscript was shortly found in Stockholm in the Library of the Royal Swedish Academy of Sciences bound in with a copy of the 1566 Basel edition of *De revolutionibus* that belonged to Johann Hevelius. This text was published by Lindhagen in 1881. A collation of the Stockholm and Vienna manuscripts was published by Curtze in 1882, and an edition based on the three previous publications was published by Prowe (**2**: pp. 184–202) in 1884 without textual variants from the two manuscripts. A new edition, ostensibly from the manuscripts, was published by Rossmann in 1948 (reprinted 1966) along with his translation. Rossmann's text differs but little from Prowe's (and this often not wisely), and there is no collation of the textual variants. In 1962 W. P. Wightman published an account of a manuscript in the Library of the University of Aberdeen containing the beginning of the *Commentariolus* bound in with a copy of the Basel edition of *De revolutionibus* (Rosen, p. 310), and in 1965 J. Dobrzycki identified it as a complete copy (Rosen, p. 282). The Aberdeen manuscript has not yet been published, but a new edition of the *Commentariolus* based on all three manuscripts is in preparation and will be published before long in Poland. It is to be hoped that this new edition will straighten out the textual problems once and for all.

The Vienna manuscript contains a dedication by Christian Longomontanus to his friend Johann Ericksen dated 18 July, 1600, when Longomontanus left Prague (Curtze, 1878: p. 1). It is evidently a copy of Tycho's manuscript, and the Stockholm manuscript agrees with it in a sufficient number of erroneous readings to show that it too must be descended from the same source. The Aberdeen manuscript is apparently related closely to the Stockholm text although it is not a copy (Rosen, p. 282). Thus all three manuscripts descend from the copy acquired by Tycho in 1575, and their many false readings, particularly in numbers, show that they are far removed from Copernicus's original, although one bad intermediary could explain many of the errors.

The first translation of the *Commentariolus* was published by A. Müller in 1899. A second was published by E. Rosen, first in 1937 in *Osiris* **3**, then in 1939 in *Three Copernican Treatises*, which has been often reprinted, most recently in a third edition (1971). F. Rossmann's text and translation were published in 1948 and reprinted in 1966. I do not think that any of these translations can be recommended for its accuracy, but far be it from me to take to task anyone who has fought with this difficult and corrupt text. The accompanying notes are in all cases superficial, non-technical, and frequently erroneous so that I wonder how any reader has been able to understand much of the treatise through the use of these translations.

My translation is based on Prowe's text continuously compared with Curtze (1878), Lindhagen (1881), and Curtze (1882). Everyone who has dealt with the text—Curtze, Lindhagen, Prowe, Müller, Rosen, Rossmann—has proposed emendations. I have considered all these, and put forth a good many, probably too many, of my own. The textual notes to the translation presented here are given to page and line numbers of Prowe's edition. I have put the emended reading first, usually with more text than just the corrected word or number. The correction itself is in italics. Where a phrase is quoted with no italics, the emendation is to punctuation, and I have recorded these only when the corrected punctuation affects the sense. Following the emended reading I have generally put the readings of the Stockholm manuscript, designated S, and the Vienna manuscript, designated V, in parentheses. This seems more informative than merely giving Prowe's own choice or emendation. In some cases I have taken a manuscript reading rejected by Prowe, and then merely give the manuscript letter following the emendation. In corrections of punctuation I have not given the uncorrected text since only an inspection of at least a whole sentence can make sense out of them, and I assume anyone who is interested can make the comparison himself. In a few cases where I suspect severe textual corruption, like a missing line or a meaningless phrase, I have mentioned this in the notes with a question mark, but have not incorporated any emendation in the translation. These are discussed in the commentary.

Many false numbers can be corrected by U, which is of the highest authority since it is in Copernicus's own hand. Other numbers can often be corrected from internal comparison or computation, but the most difficult to control are the longitudes of the apsidal and nodal lines, some of which may contain textual errors. They have been left as they stand in the manuscripts, and are discussed in the commentary.

The sections of the text have been numbered from one to nine, and I have broken these sections into smaller units, often giving titles to the subdivisions. The commentary follows each subdivision. In the translation I have tried to give my understanding of the text while remaining as literal as possible in keeping with intelligible English. Additions and filled-out ellipses have been placed in square brackets unless the addition is just the substitution of a noun for a pronoun or an understood subject or object. The most difficult passage in this respect is the description of the second anomaly of the superior planets. The one word in the text that has caused translators and writers the most trouble is *orbis* for there seems to be some embarrassment about admitting that Copernicus envisioned all celestial motion as the rotation of spheres. In the *Commentariolus orbis* always means sphere; spheres have axes, circles do not. The translation of *orbis* by *circle* frequently makes nonsense out of the text. When Copernicus refers to circles instead of spheres, he always means great circles on the surface of a sphere, and this is shown clearly by his occasional use of the phrase "circle of the sphere." When he refers to the plane of a sphere, an ellipsis used frequently in the *Epitome*, he always means the plane of the great circle perpendicular to the axis.[10]

The list of references, which includes only the writings consulted in the preparation of this study, is in no sense a bibliography of literature on Copernicus. For one with no taste for hagiography and no head for metaphysics, but only an interest in Copernicus's astronomy, Alpheus would be a welcome aid in the indelicate labor of examining the excessive quantity of scholarly productions that end up having nothing, or at least nothing of competence to say about his planetary theory. There are, however, some valuable studies worthy of mention. An excellent and frequently penetrating description of Copernicus's planetary theory was written by Michael Maestlin, originally as a letter to Kepler in order to help him with the planetary distances he needed when working on the *Mysterium Cosmographi-*

[10] The translation is done strictly. In the commentary I have freely used circle, sphere, and even orbit. The word *orbis* can, of course, also mean circle, but in the *Commentariolus* Copernicus always and in *De revolutionibus* he frequently uses it to mean sphere.

*cum*, and then in a somewhat revised version as an appendix to the *Mysterium*.[11] Kepler says much of interest about Copernicus throughout his writings, especially in Part I of the *Astronomia Nova*.[12] The study of François Viète's unfinished and unpublished *Ad Harmonicon Coeleste* has been, for me at least, a revelation of what the planetary theory of Ptolemaei Paraphrastes is really all about.[13] The best modern technical expositions are still those of Delambre (1821) and Herz (1887–1894). Since Herz's work, the only worth-while contributions have been Neugebauer ($1968_1$), Toomer (1969), and the various articles on the Marāgha astronomers. The writings of L. Birkenmajer, although mostly non-technical, are historically valuable, and I regret that my ignorance of Polish leaves me able to do no more than mine his work for quotations from documents and primary sources. Some useful and interesting work has been done in Poland in the last few years, especially by J. Dobrzycki, and I likewise regret that I cannot take full advantage of it.

I have tried in this study to examine Copernicus's work in two ways. The first is that of a historian, inquiring into what Copernicus's work is about, how he arrived at his planetary theory, and what sources and principles guided his investigations. The second is that of an astronomer, not a modern astronomer, but a contemporary of Copernicus's, or better, one like Brahe or Viète or Kepler, who was critically interested in knowing whether Copernicus's contributions are of value, whether he properly understood the Ptolemaic theory he was adapting, whether he properly understood his own theory, and whether the theory in fact does what he claims for it. With all due respect, I hope my occasional (or frequent?) impatience with Copernicus's efforts will be understood in this way.

In conclusion I would like to thank O. Neugebauer for placing at my disposal his notes on Copernicus and for his advice on this paper, and G. J. Toomer for checking over the translation; neither should be held responsible for my idiosyncrasies and errors.

[11] Maestlin's letter is printed in Kepler, *Werke* **13**: pp. 54–65. The volume also contains the surviving correspondence between Maestlin and Kepler during the period of the writing and printing of the *Mysterium* which, incidently, shows that Kepler was not always giving his teacher's explanations the serious attention he should have. Maestlin's appendix, *De dimensionibus orbium et sphaerarum coelestium iuxta Tabulas Prutenicas, ex sententia Nicolai Copernici*, is printed in Kepler, *Werke* 1: pp. 132–145. A translation by A. Grafton appears in this issue of the *Proceedings of the American Philosophical Society*, pp. 523–550.

[12] Kepler, *Werke* **3**.

[13] See Manuscripts under List of References.

## TRANSLATION AND COMMENTARY

# A BRIEF DESCRIPTION BY NICOLAUS COPERNICUS CONCERNING THE MODELS OF THE MOTIONS OF THE HEAVENS THAT HE INVENTED

### 1. [INTRODUCTION]

I understand that our predecessors assumed a large number of celestial spheres principally in order to account for the apparent motion of the planets through uniform motion, for it seemed highly unreasonable that a heavenly body should not always move uniformly in a perfectly circular figure. They had discovered that by the arrangement and combination of uniform motions in different ways it could be brought about that any body would appear to move to any position.

Calippus and Eudoxus, attempting to carry this out by means of concentric circles, could not by the use of these[a] give an account of everything in the planetary motion, that is, not only those motions that appear in connection with the revolutions of the planets, but also that the planets appear to us at times to ascend and at times to descend in altitude, which concentric circles in no way permit. And for this reason a preferable theory, in which the majority of experts finally concurred, seemed to be that it is done by means of eccentrics and epicycles.

Nevertheless, the theories concerning these matters that have been put forth far and wide by Ptolemy and most others, although they correspond numerically [with the apparent motions], also seemed quite doubtful, for these theories were inadequate unless they also envisioned certain *equant* circles, on account of which it appeared that the planet never moves with uniform velocity either in its *deferent* sphere or with respect to its proper center. Therefore a theory of this kind seemed neither perfect enough nor sufficiently in accordance with reason.

[a] 185:7 *ex* his (SV et)

The purpose of Copernicus's brief review of earlier planetary theory is to enunciate the principles of this science and raise objections to the work of his predecessors. He states first that the method of planetary theory is to resolve the apparent, irregular motion of a planet into separate components that are (1) uniform in velocity and (2) circular in figure. The homocentric "circles" of Eudoxus and Calippus fulfill both these requirements, but produce an unsatisfactory representation of the planet's motion in longitude and in no way account for its apparent change of distance. How much Copernicus understood or thought he understood of the Eudoxan model is not clear. The later treatises of Girolamo Fracastoro and Giovanni Battista Amico show that there was some interest at this time in the (limited) possibilities of homocentric sphere models, but neither of these authors demonstrate more than a superficial knowledge of the Eudoxan model. What is of interest to note about Copernicus's remark is that he objects to the result, but not to the principle of homocentric spheres.

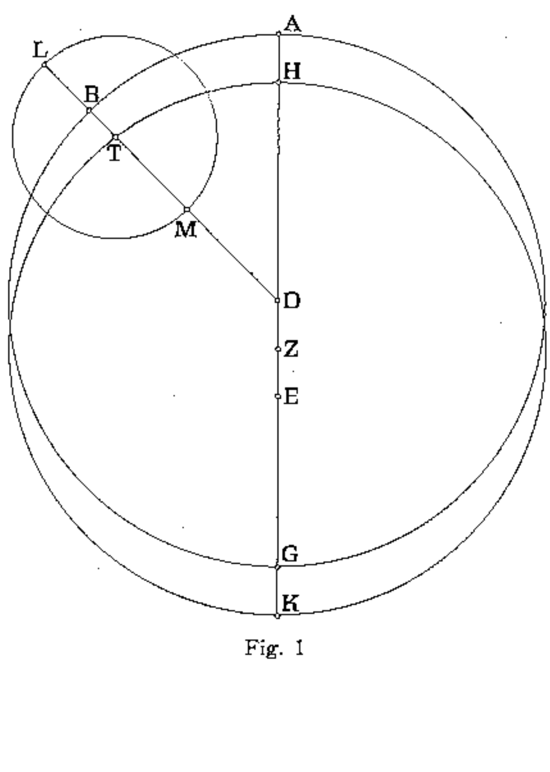

Fig. 1

In his comment on the Ptolemaic model, on the other hand, he concedes that the representation of planetary motion is accurate for purposes of computation, but objects on principle to the violation of uniform circular motion. This objection is directed against Ptolemy's bisected eccentricity in the representation of the first or zodiacal anomaly. Copernicus's own understanding of the model is seen most clearly in Regiomontanus's description in the *Epitome of the Almagest* IX, 6. It is translated here with the points Copernicus finds objectionable in bold-face type.[1] The figure for the proposition is figure 1.

6. *To assign suitable causes for the irregular motions of the three superior planets and Venus.*

[1] Translated from Venezia, Bibl. Naz. Mar., MS. Lat. fondo antico 328. This manuscript was prepared for Bessarion under Regiomontanus's supervision.

The same representation will be of use for the motion in longitude of the three superior planets and Venus which you will understand in the figure as follows:

Let *ABG* be an eccentric circle about center *D*, let its diameter passing through the center of the sphere of the signs be *ADG* in which the center of the sphere of the signs will be point *E*, and thus point *A* will be its greatest distance and point *G* its least distance. And after bisecting line *DE* in point *Z*, with radius equal to *AD* I describe about *Z* circle *HTK* equal to circle *ADG*, and about center *T* I describe the circle of the sphere of revolution which will be circle *LM*. Draw line *LTMD*. Let us imagine that the plane of all these circles is in the plane of the sphere of the signs so that what follows will be simpler.

Now, first it must be understood that line *EA* passing through the greatest and least distance of the eccentric moves with the motion of the sphere of the fixed stars, carrying with it the two points *Z* and *D*. Then it must be understood that the plane of the eccentric sphere *HTK*, which carries the sphere of revolution *LM*, always moves in the order of the signs about its center *Z*, **uniformly, however, not about *Z*, but about point *D*.** After that it must be understood that the epicycle moves about its center, carrying the body of the planet in the order of the signs in its higher half and opposite to the order of the signs in its lower half. **But the uniformity of this motion takes place in relation to the point in the highest part of the epicycle that lies on the line passing through point *D* and the center of the epicycle.** And in this way [an irregularity] will appear to take place similar to the irregularity that is perceived by observation.

Copernicus takes exception to the uniform motion of the center of the epicycle taking place on the imaginary circle *ABG* about center *D*, the "equant circle," while its center remains on circle *HTK* about center *Z*, the "deferent sphere." The phrase that the planet does not move uniformly "with respect to its proper center" refers to the motion on the epicycle. This is uniform with respect to the mean apogee *L* lying on the line directed to center *D*. Copernicus points out in *De rev.* V, 2, that the motion of the planet should be uniform with respect to the apogee lying on a line directed to center *Z*. But the uniform motion of the planet with respect to *L* is, of course, a necessary consequence of the uniform motion of *T* with respect to *D*, for were the motion on the epicycle referred to an apogee directed to a different center, say *Z*, the motion of anomaly would be out of phase with the sun and the planet would not be at the mean apogee of the epicycle at the time of mean conjunction.

Copernicus's only real objection to Ptolemy's model, therefore, is that the motion of the center of the epicycle is not uniform with respect to the center from which it maintains a constant distance. The reason for this objection is that Copernicus considers the motion of a planet to be directed by the revolution of a material sphere or spheres in which the planet is fixed. The only motion permitted to the sphere is a simple uniform rotation about its diameter; it cannot move uniformly with respect to any other straight line passing through it. In figure 1, the diameter of the sphere carrying the epicycle passes through point *Z*, and therefore its motion cannot possibly be uniform with respect to a line passing through *D*. If sphere *HTK* were made to rotate about an axis passing through *D*, it would be displaced. The principle of uniform circular motion as Copernicus uses it is a mechanical principle about the rotation of a sphere and nothing more. It should not be understood as a philosophical, much less metaphysical, principle about the motion proper to the substance of the heavens. Speculations about such things do not belong to the domain of mathematical astronomy.

Therefore, when I noticed these [difficulties], I often pondered whether perhaps a more reasonable model composed of circles could be found from which every apparent irregularity would follow while everything in itself moved uniformly, just as the principle of perfect motion requires. After I had attacked this exceedingly difficult and nearly insoluble problem, it at last occurred to me how[a] it could be done with fewer and far more suitable devices than had formerly been put forth if some postulates, called axioms, are granted to us, which follow in this order:

First Postulate

There is no one center of all the celestial spheres (*orbium*) or spheres (*sphaerarum*).

Second Postulate

The center of the earth is not the center of the universe, but only the center towards which heavy things move and the center of the lunar sphere.

Third Postulate

All spheres surround the sun as though it were in the middle of all of them, and therefore the center of the universe is near the sun.

Fourth Postulate

The ratio of the distance[b] between the sun and earth to the height of the sphere of the fixed stars is so much smaller than the ratio of the semidiameter of the earth to the distance of the sun that the distance between the sun and earth is imperceptible compared to the great height of the sphere of the fixed stars.

Fifth Postulate

Whatever motion appears in the sphere of the fixed stars belongs not to it but to the earth. Thus the entire earth along with the nearby elements rotates with a daily motion on its fixed poles while the sphere of the fixed stars remains immovable and the outermost heaven.

Sixth Postulate

Whatever motions appear to us to belong to the sun are not due to [motion] of the sun but [to the motion] of the earth and our sphere with which we revolve around the sun just as any other planet. And thus the earth is carried by more than one motion.

Seventh Postulate

The retrograde and direct motion that appears in the planets belongs not to them but to the [motion] of the earth. Thus, the motion of the earth by itself accounts for a considerable number of apparently irregular motions in the heavens.

[a] 186:5 inexplicabilem, obtulit se tandem quomodo
[b] 186:19 comparationem *distantiae* (SV distantiarum)

The problem Copernicus sets himself is the development of planetary models that will (1) accurately represent the apparent motions of the planets and (2) preserve uniform circular motion. Since he raises no objection to the accuracy of the Ptolemaic models, he has no intention of deriving more accurate numerical parameters, and thus the simple extraction of parameters from the *Alphonsine Tables* in the *Commentariolus* is readily understandable. One wishes that Copernicus would say something more in this paragraph about how he "attacked this exceedingly difficult and nearly insoluble problem." Are the models entirely his own invention or did he know of the identical models developed by Ibn ash-Shāṭir? We cannot decide on the basis of his own statements. Copernicus remains far too reticent about acknowledging the sources of his information even in *De revolutionibus* where he hardly admits the existence of Regiomontanus even though he relies upon the *Epitome of the Almagest* constantly, in preference to the *Almagest* itself, for numerical parameters, geometrical demonstrations, methods of solving problems, and observational records. Likewise, he does not acknowledge in *De revolutionibus* the use of Regiomontanus's *Tabulae directionum* for some of the spherical astronomy in Book II, nor of Georgio Valla's *De expetendis et fugiendis rebus* for the star catalog and scattered information in Books I and V, nor of Johann Werner's *De motu octavae sphaerae* for records of the obliquity of the ecliptic and the very value of the obliquity that he himself uses for reducing his observations of Spica. We should, therefore, hardly be surprised that in the *Commentariolus* he acknowledges no debt to the *Alphonsine Tables*, the *Epitome*, and perhaps to a presently unknown treatise explaining the planetary models of the Marāgha astronomers.

I do not understand what Copernicus means by "fewer" devices. Perhaps it is a comparison with the planetary models in Peurbach's *Theoricae novae planetarum*. Peurbach's excellent descriptions of solid sphere models are done so meticulously that the number of spheres seems exceedingly great.

The seven postulates, incorrectly called *axioms* since they are hardly self-evident, take the place of the general description of the universe in the opening chapters of the *Almagest*, the *Epitome*, and later, *De revolutionibus*. Whether Copernicus considers them absolutely true or merely working hypotheses is not made clear. While I have no desire to become involved in the debate over Osiander's preface, preferring to leave that to those who never read beyond the opening chapters of *De revolutionibus*, it seems clear that Copernicus considers the equivalent statements in *De revolutionibus* to be true. There is no reason to doubt that he also believes these postulates to be true.[2]

He has, however, made more assumptions than were necessary. Since his adoption of heliocentric theory followed from his study of possible rearrangements of planetary models, only postulates 3 and 6 are temporally prior to his planetary theory. In fact only postulates 3 and 6 are logically prior while postulates 2, 4, 5, and 7 are *consequences* of 3 and 6, and postulate 1 stands by itself. Thus, if 3 and 6 are true, then 2, 4, 5, and 7 can be proved. Postulates 3 and 6 cannot be proved, and the evidence for their truth is the sense of the heliocentric theory itself, that is, the fixing of the order and distances of the planets, and the explanation of the second anomaly, gratuitously given here as the seventh postulate.

It is also worth noting that the postulates, with the exception of the first, have no connection with the objections to Ptolemy's representation of the first anomaly stated earlier nor with Copernicus's own model for the first anomaly, but are concerned only with the heliocentric theory. Since Copernicus has raised no objection to Ptolemy's representation of the second anomaly, the introduction of these postulates at this point appears unmotivated. Perhaps this flaw and the logical error of stating postulates 2, 4, 5, and 7 as postulates rather than deductions from postulates 3 and 6 are intelligible if one considers that the *Commentariolus* may well have been written in haste with no revision.

The following remarks may be made on each postulate:

1. This may be nothing more than a vague distinction of the centers of the lunar and planetary orbits, clarified in postulates 2 and 3, but I think it means something more. It excludes the possibility of homocentric spheres without epicycles no matter where their common center may be, and therefore applies to the first anomaly since the second anomaly alone could be

[2] It could, however, be intelligently argued that because Copernicus calls these statements postulates (*petitiones*), he is therefore not asserting that they are necessarily true. Yet, if he had any doubts about the truth of the heliocentric theory, he probably would not have advanced it in the first place.

represented perfectly by a series of homocentric spheres carrying the planets about the center of the earth's orbit. In my reading of the text I take *sphaerarum* to be a synonym clarifying the meaning of *orbium*.

2. That the earth is not the center of the universe follows necessarily from postulates 3 and 6, or even from 6 alone. As an assumption without proof it seems highly doubtful, flatly contradicted by the first principle of spherical astronomy. Postulate 4 removes this difficulty. "Center towards which heavy things move" is a rendering of Copernicus's words *centrum gravitatis*.

3. The common center of the planetary spheres is the center of the earth's orbit, the mean sun, which is indeed near (*circa*) the sun. That the mean sun may be considered the center of the spheres of the planets is the fundamental assumption of the heliocentric theory, made by Copernicus in the course of his investigation of the eccentric representation of the second anomaly described by Regiomontanus in *Epitome* XII, 1 and 2.

4. If the earth moves around the sun, that is, if postulate 6 is true, the sphere of the fixed stars must be very distant for there to be no detectable stellar parallax. Copernicus's manner of stating this is interesting. It follows from *Epitome* I, 4 (= *Almagest* I, 6), where it is shown that the earth is but a point in comparison to the distance of the sphere of the fixed stars. Copernicus must apply this comparison to the entire radius of the earth's orbit. Now it is also pointed out in *Epitome* I, 4 that for all practical purposes the earth is also a point compared to the distance of the sun, and so Copernicus begins with this comparison and then extends it *a fortiori* to the ratio between the radius of the earth's orbit and the distance of the fixed stars. The consequence for the distance of the sphere of the fixed stars is impressive. If we take al-Battānī's solar parallax of 0;3° and solar distance of 1146 terrestrial radii (from *Epitome* V, 25, and essentially followed in *De rev.* IV, 24), then a stellar parallax of 0;3° would place the fixed stars at a distance of about 1,313,200 terrestrial radii, and a parallax of 0;1° at about 3,939,600 terrestrial radii. Compared to the distance of about 20,000 terrestrial radii following from Ptolemy's theory of planetary distances, commonly accepted in Copernicus's time, these distances are extraordinarily large, and if Copernicus is suggesting a stellar parallax of less than 0;1°, the distance to the fixed stars must be larger still.

5. The diurnal rotation of the earth should be deduced from postulate 6 since a rotation of the entire universe, including the sun, about an earth that moves around the sun would be unthinkable. The sphere of the fixed stars is the outermost heaven because a sphere of diurnal rotation is no longer necessary. Possibly Copernicus also means that additional spheres of precession and trepidation are eliminated, but I think such an interpretation unlikely in this postulate about the diurnal rotation.

6. This is the second fundamental assumption of Copernican theory. Postulate 3 by itself might still permit the mean sun, the center of the planetary spheres, to revolve around the earth. This postulate removes all ambiguity. Copernicus uses the plural *motions*, as he does in the nearly identical title of the section on the three motions of the earth, because he is also including the apparent diurnal motion of the sun, which is really unnecessary here and should have been attached to postulate 5.

7. This is Copernicus's explanation of the second anomaly. Along with the fixing of the order and heliocentric distances of the planets, it is the principal argument for his planetary theory. Nevertheless, since it can be *proved* from postulates 3 and 6, it should not be stated as a postulate.

Now that these postulates have been set down, I shall attempt briefly to show how carefully the uniformity of the motions may be preserved. I have decided, however, for the sake of brevity to leave the mathematical demonstrations out of this treatise as they are intended for a larger book. Nevertheless, the lengths of the semidiameters of the spheres will be set down here in the explanation of their circles, from which anyone not ignorant of mathematics will easily

> understand how very precisely such an arrangement of circles agrees with computations and observations.
>
> In the same way, in case anyone believes that we have asserted the movement of the earth for no good reason along with the Pythagoreans, he will also receive considerable evidence [for this] in the explanation of the circles. And in fact, [the evidence] by which natural philosophers attempt so very hard to confirm the immobility of the earth depends for the most part upon appearances. All [their evidence] falls apart here in the first place since we overthrow the immobility of the earth also by means of an appearance.

Again, Copernicus returns to his original object of preserving uniform motion. What is the "larger book" he refers to? There is no reason to believe it was to be anything like *De revolutionibus.* Consider that he says his models agree "with computations and observations." Since no mean motions or positions at epoch are given in the *Commentariolus*, I cannot imagine how Copernicus expects his reader to compute a planetary position and compare it with an observation, unless he expects him to use mean motions from, say, the *Alphonsine Tables.* All he can claim with truth is that his models will produce equations in agreement with the correction tables in the *Alphonsine Tables*, and this is true precisely because the parameters are extracted from the Alphonsine correction tables. The derivation of new parameters, however, is exactly what Copernicus at least makes a pretense (and it is mostly pretense) of doing at length in *De revolutionibus.* I believe that the sort of book Copernicus was contemplating when he wrote the *Commentariolus* would have consisted of geometrical demonstrations of the equivalence of Ptolemy's and his own models for both the first and second anomalies showing how, given the same parameters, they will produce the same apparent motions. These demonstrations would show "how carefully uniform motion may be preserved" in numerical agreement with, I suppose, the *Alphonsine Tables.* If he intended to derive new parameters as in *De revolutionibus*, or was using new parameters here, he could hardly commend his work on the ground of its correspondence with any kind of computation his reader could carry out. He notes that anyone skilled in mathematics can figure all this out from the elements of the orbits in the *Commentariolus*, and this would not be possible if the elements were new. I assume, in fact, that he expected a skilled mathematician to recognize that the parameters will produce the familiar Alphonsine equations, and then demonstrate for himself the equivalence of the planetary models. In short, one cannot use this statement about a "larger book" to show that Copernicus was even contemplating, let alone working on, *De revolutionibus* at this time.

The insulting reference to the "Pythagoreans" is amusing and informative. Far from being in any way influenced by these old sages, Copernicus, who arrived at the theory of the earth's motion through his own analysis, accuses them, justly I think, of not knowing what they were talking about. In light of this remark, the citations of Philolaus, Heracleides, *et al.* in the Dedication and I, 5 of *De revolutionibus* are nothing more than a few references to venerable antiquity, set down by Copernicus in order to give his new theory some respectability in the eyes of people who are impressed most of all by such things, not the least vice of Copernicus's time. One has only to remember the technicalities of planetary theory to understand that the venerable old sages had nothing to do with Copernicus's work.

In contrast to the gratuitous assumption by the Pythagoreans of the motion of the earth, Copernicus will give his reader "considerable evidence in the explanation of the circles." Is this promise kept? Not explicitly, for nowhere does Copernicus point out that any particular apparent motion or any particular model is evidence or proof of the earth's motion. If, however, one considers that Copernicus arrived at the heliocentric theory by an analysis of the geometry of planetary models, then the very explanation of the models, which are both equivalent to geocentric models and possess a striking logical sense of their own, is the most convincing evidence of their truth. Indeed, it is the evidence that convinced Copernicus himself.

The following reference to the natural philosophers (*Physiologi*), by which I suppose Copernicus means Aristotelians, shows something of his attitude toward natural philosophy. Note that he does not argue with them on their own ground, that is, physical causes, the nature of matter, motions appropriate to different kinds of matter, and such, but perhaps unfairly shifts the issue to his own domain by stating that their arguments for the immobility of the earth depend mostly upon appearances while he will show that the immobility of the earth is only an appearance. In the last analysis he is right since, if the earth did not appear to be so completely at rest, the physical arguments would never have been invented, but it still seems an evasion of a confrontation with the Aristotelians to remark, as if it were a full refutation, that the earth's immobility is only an appearance and say nothing more.

Copernicus, however, is under no obligation to dispute with Aristotelians. He has already removed the only astronomical objection to his theory with the fourth postulate. Being an astronomer, not a natural philosopher, he knows, as Ptolemy knew before him, that astronomy is so much more sophisticated in its methods and precise in its results than the primitive state of physical science, that the astronomer is free to ignore the crude and, in Copernicus's time, nearly incomprehensible chatter of natural philosophers with regard to anything above the lunar sphere. This is the reason that in *De rev.* I, 8, Copernicus considers physical arguments about the rotation of the earth, but nowhere bothers to present any physical justification for his much more astounding theory that the earth moves about the sun. To understand Copernicus's work properly, as he understood it, one must completely remove it from natural philosophy. Copernicus does precisely this when he subtly shifts the argument to purely astronomical considerations of appearances since the convincing evidence for the movement of the earth lies in the planetary theory itself and this sort of evidence is completely outside of natural philosophy. Far from bringing about a union of astronomy and terrestrial physics, Copernicus is at pains to maintain their distinction.

So in one sense Osiander was right about astronomical hypotheses because they need not be in accordance with natural philosophy. In a more important sense, however, he was wrong because the astronomer is perfectly justified in claiming that his planetary theory, which accurately represents the apparent motions of the planets, shows the real motions of the planets and the arrangement of the heavens. This is exactly what Copernicus, a pure astronomer who wrote (applied) "mathematics for mathematicians," did in putting forth his heliocentric theory. When astronomy says one thing and physics another, the problem is not always with astronomy.

## 2. THE ORDER OF THE SPHERES

The heavenly spheres surround each other in the following order: The highest is the immovable sphere of the fixed stars which encloses and fixes the positions of all [the others]. Under it is the sphere of Saturn[a] which the sphere of Jupiter[b] follows, then the sphere of Mars, under which is the sphere in which we are carried around, then the sphere of Venus, last the sphere of Mercury. The sphere of the moon revolves around the center of the earth, and is carried with it like an epicycle. Also, one [planet] exceeds[c] another in rapidity of revolution in the same order in which they traverse the larger or smaller perimeters of [their] circles. Thus, Saturn returns to the same position in the thirtieth year, Jupiter in the twelfth, Mars in the second,[d] the earth in the annual revolution; Venus completes a revolution in the ninth month, Mercury in the third.

[a] 188:7 *Saturnius* (SV Saturnus)
[b] 188:7 *Jovianus* (S Jovius, above in second hand; V omit)
[c] 188:11 superat secundum
[d] 188:13 *Mars secundo* (SV omit)

The immediate benefit of heliocentric theory is that the order of the planets, which corresponds to the periods of their revolutions about the sun, is fixed without question. It then follows that the quantity of their maximum equations shows the relative sizes of their orbits compared to the earth's orbit and allows their relative heliocentric distances to be determined. This is the second benefit of heliocentric theory.

When Copernicus says that the sphere of the fixed stars "fixes the position (*locans*)" of everything, he means either that the sphere of the fixed stars is the place in which the whole universe is located or that apparent motions of planets are measured with respect to the fixed stars. The use of the word *locans* makes me believe that his meaning is the former, but the similar statement in *De rev.* I, 10 will support both meanings.

The periods of Mars and Venus require comment. That of Mars is missing in both manuscripts, but later, in 195: 1 (see below, p. 465) Mars is said to complete a revolution in the 29th month. This is such a gross error that I have emended the text to the 23rd month since the longitudinal period of Mars is about 687 days $= 22 \cdot 30$ days $+ 27$ days, which falls in the second year. It is, however, worth noting that the period of 2 years and 5 months, that is, 29 months for Mars is given by Cleomedes (*De motu circ.* I, 3, ed. Ziegler, 18:23–26).[1] Now Georgio Valla's translation of Cleomedes was printed in Venice in 1498, and it is not impossible that Copernicus simply took over this worthless number without checking it against Mars's mean daily motion in either the *Alphonsine Tables* or the *Epitome* from which he could immediately compute the correct period. Any reader who believe the error belongs to Copernicus is free to restore the erroneous numbers. Venus completes a heliocentric revolution in about 225 days $= 7 \cdot 30$ days $+ 15$ days, that is, in the eighth month, not in the ninth month as given here and in 198: 10. This error, however, must be due to Copernicus since the same incorrect period is given in *De rev.* I, 10. It is surprising that he made such a mistake and then persisted in it since the computation of the approximate period is trivial and he evidently did it correctly for Mercury.

[1] The same in *Scholia in Aratum* (in Firmicus Maternus, ed. Aldus, Venetiis, 1499), cf. E. Maass, (1958), p. 427 n.28,9.

## 3. THE MOTIONS THAT APPEAR TO BELONG TO THE SUN

The earth is carried around by a motion of three components. By the first [it is carried around] in the *great sphere* which,[a] surrounding the sun, completes a revolution in the order of the signs in one year, always describing equal arcs in equal times. The center of the great sphere is distant 1/25 part of its semidiameter from the center of the sun. Now since it is assumed that the semidiameter of this sphere has an imperceptible quantity compared to the height of the sphere of the fixed stars, it follows that the sun will appear to be carried around by this motion just as if the earth were located in the center of the universe. Since, however,[b] this results from the motion not of the sun, but rather of the earth, so for example, when the earth is in Capricorn the sun is seen in a straight line along the diameter in Cancer, and so on in the same way. Further, the sun will appear to move non-uniformly in this motion on account of its distance from the center of the sphere, as has just been explained, the maximum equation due to which reaches $2\frac{1}{6}°$. The sun is invariably removed from the center [of the great sphere] in the direction of a point of the sphere of the fixed stars which is distant about 10° to the west of the bright star which is the more brilliant [of the two] in the head of Gemini. Thus, the sun is seen at its greatest distance when the earth is in the position opposite to this and the center of the sphere lies between them. Moreover, not only the earth, but also whatever is included within the lunar sphere is carried around by this sphere.

[a] 188:17 *qui* Solem (SV quo)
[b] 189:7 subiaceat. Cum autem (S)

The first motion of the earth, the annual revolution, is brought about by the rotation of what Copernicus calls here and occasionally in *De revolutionibus*, the Great Sphere (*orbis magnus*). The reason for using this expression is never given. Kepler, who usually understands Copernicus's motivation better than Copernicus himself, clearly does not know why when he guesses, both in the *Mysterium Cosmographicum* and in the *Epitome Astronomiae Copernicanae*, that the earth's orbit is called the Great Sphere because it has so many uses.[1] Later in this commentary, when examining Copernicus's derivation of the heliocentric theory, I shall consider the possibility that it is a relic of a stage of Copernicus's analysis in which he had adopted a Tychonic system so that the Great Sphere was the sphere carrying the common center of all the planetary spheres around the earth. In *De revolutionibus* he usually calls the earth's orbit the annual sphere (*orbis annuus*) which does not grant it quite so conspicuous a position.

The model for the motion of the earth, a simple eccentric, is shown in figure 2. The earth $O$ moves uniformly in a circle about center $\bar{S}$ which we shall call the mean sun. The true sun $S$ is removed from $\bar{S}$ in the direction of a point $\lambda_A{}^*$

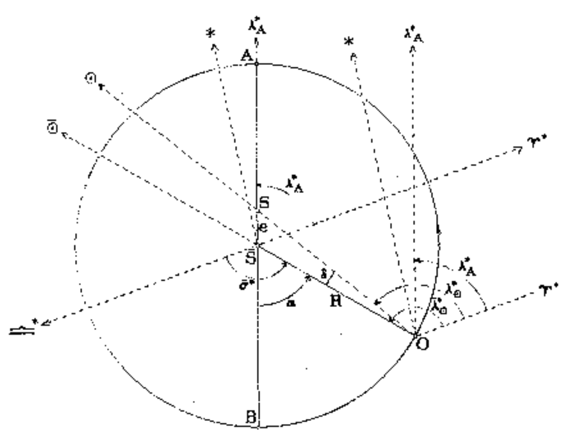


Fig. 2

[1] Kepler, *Gesammelte Werke* **1**: pp. 19, 20 (= **8**: pp. 38, 36); **6**: p. 403.

in the sphere of the fixed stars that Copernicus designates as being about 10° west of the more brilliant star, indicated by $*$, in the head of Gemini. Although Copernicus has not yet stated whether ecliptic longitude is tropical or sidereal, we shall measure it sidereally and indicate the zero point of sidereal longitude by $\Upsilon^*$. Since the sphere of the fixed stars is sufficiently distant to preclude annual parallax, directions to the same point on the sphere of the fixed stars from any point in the model are parallel. Now, the earth moves uniformly through angle $\bar{\sigma}^*$, completing a revolution in a sidereal year, and thus the direction $O\bar{\odot}$ passing through $\bar{S}$ will move uniformly through angle $\bar{\lambda}^*{}_\odot$ = angle $\bar{\sigma}^*$ while the direction $O\odot$ passing through $S$ will move non-uniformly through angle $\lambda^*{}_\odot$. The signs $\bar{\odot}$ and $\odot$ indicate the directions of the mean and true sun respectively. The difference between $\bar{\lambda}^*{}_\odot$ and $\lambda^*{}_\odot$ is angle $\delta$, and the maximum value of $\delta$ according to Copernicus is $2\frac{1}{6}°$ which is the maximum solar equation in the *Alphonsine Tables*. Where the radius $\bar{S}O = R = 1$, the eccentricity $e = \bar{S}S$ is

$$e = R \sin \delta_{\max} = \sin 2;10° = .0378 = \frac{1}{26.46}$$

or where $R = 60$, $e = 2;16 = \frac{1}{26;28}$. Copernicus's value is rounded to $e = 1/25$ so that in computing the parameters of the planetary models he can let $R = 25$ rather than 26;28 and measure all distances and radii in units in which $e = 1$.

Evidently, it had not yet occurred to Copernicus to measure sidereal longitudes from $\gamma$ Arietis, and so the directions of apsidal and nodal lines are specified in relation to zodiacal stars. These directions are all considered sidereally fixed, as in the *Alphonsine Tables*, and further, the apsidal lines of the earth (sun) and Venus coincide, again as in the *Alphonsine Tables*. Unfortunately, unlike the radii of the spheres and epicycles of the planetary models which, with the exception of Mercury, are computed from the sines of the maximum equations in the *Alphonsine Tables* with full precision, the directions of the apsidal lines show slight variations and one gross error (for Saturn). All of this will be taken up in detail in commenting on the superior planets; here it is merely introductory to considering the apsidal line of the earth. Now, Copernicus says that it is "about 10° to the west of the bright star which is the more brilliant [of the two] in the head of Gemini," and this imprecise statement refers to either Castor or Pollux.

The longitude of the apsidal line may be found as follows: The Alphonsine star catalog gives tropical longitudes of stars, and although it was originally derived for the year 1256 by applying al-Battānī's uniform precession of 1° in 66 years to the longitudes in as-Sūfī's catalog for the year 964, in some way not yet explained it was later referred unchanged to the Era of Alfonso, June 0, 1252, and assigned a precession from the Era of the Incarnation, January 0, 1 A.D., according to the theory of precession and trepidation found in the *Alphonsine Tables*. This precession is also given to the apogees and nodes of the planets, so if we compute their tropical positions for the Era of Alfonso and compare them with the tropical positions of stars in the catalog, we can find their differences, and these differences will be the distances of the apogees and nodes from fixed stars just as Copernicus specifies them in the *Commentariolus*. These distances will be the same at any epoch since the apogees and nodes move with the fixed stars, that is, they are sidereally fixed. We let the mean longitude of the apogee at epoch be $\lambda_{A0}$, which moves with the mean precession $\bar{\pi}$, and we let the equation of trepidation be $\delta$; then the true longitude of the apogee $\lambda_A$ is given by

$$\lambda_A = \lambda_{A0} + \bar{\pi} \pm \delta.$$

At the Era of Alfonso, the longitude of the solar apogee

$$\lambda_{A\odot} = 80;37° + 0;0° + 8;4° = 88;41°.$$

Compared with Castor and Pollux, we have

$$\lambda \text{ Castor} - \lambda_{A\odot} = 100;28° - 88;41° = 11;47°$$

$$\lambda \text{ Pollux} - \lambda_{A\odot} = 103;48° - 88;41° = 15;7°$$

while Copernicus says $\lambda_{A\odot}$ is about 10° to the west. Pollux is clearly out of the question, and Castor leaves a difference of 1;47°. I shall postpone discussion of this discrepancy to the later examination of the apsidal lines of the superior planets.

On the other hand, the apsidal longitude derived by Copernicus in *De revolutionibus* is slightly over 10° west of Pollux. I believe, however, that this better agreement is only a coincidence since it creates many more problems than it solves. In III, 16 Copernicus finds for September 14, 1515, a tropical longitude of the apsidal line of 96;40°, and for this date, he says in III, 18 that the sidereal longitude of the autumnal equinox is 152;45°. The true precession $\pi$ is thus

$$\pi = 180° - 152;45° = 27;15°$$

in agreement with recomputation from the tables in III, 6 and 8 which give

$$\pi = \bar{\pi} + \delta = 26;41,30° + 0;34° = 27;15,30°.$$

Taking the sidereal longitudes of Castor and Pollux from Copernicus's star catalog, we find tropical longitudes

$$\lambda \text{ Castor} = 76;40° + 27;15° = 103;55°$$

$$\lambda \text{ Pollux} = 79;50° + 27;15° = 107;5°$$

so that

$$\lambda \text{ Castor} - \lambda_{A\odot} = 103;55° - 96;40° = 7;15°$$

$$\lambda \text{ Pollux} - \lambda_{A\odot} = 107;5° - 96;40° = 10;25°$$

The latter is in excellent agreement with the *Commentariolus*. I am afraid, however, that we must forego this agreement as coincidence for several reasons:

1. The apsidal longitude cannot be separated from Copernicus's derivation of an eccentricity of 323 where $R = 10000$, or about $1/31$ where $R = 1$, very far from $e = 1/25$ in the *Commentariolus*.

2. The maximum equation for $e = .0323$ is about 1;51° instead of the 2;10° of the *Commentariolus* and the *Alphonsine Tables*.

3. The apsidal line in the *Commentariolus* is sidereally fixed while the apsidal line in *De revolutionibus* possesses its own, irregular motion with respect to the fixed stars. This important discovery would hardly be omitted in the *Commentariolus* if Copernicus had already made it.[2]

[2] Copernicus's solar model in *De rev.* III, 20 is identical, except for numerical parameters, to az-Zarqāl's model which also varies the eccentricity and the direction of the apsidal line by means of a second, rotating eccentricity. Toomer, 1969, is an invaluable study of az-Zarqāl's and Copernicus's solar theories. Copernicus's source for az-Zarqāl's theory was a strangely garbled passage in *Epitome* III, 13 in which az-Zarqāl's sidereal longitude of the solar

4. The precession of 27;15° for the year 1515 is according to the theory of precession worked out in *De revolutionibus*, and the sidereal longitude of the apsidal line depends upon this precession, while in the *Commentariolus* Copernicus appears to have only a vague, qualitative model of precession with no numerical parameters at all. Further, the trepidation period of 1717 years in *De revolutionibus* was, in fact, derived by Copernicus, not from the apparent precession of the fixed stars, but from the change in the obliquity of the ecliptic between Ptolemy's value of 23;51,20° for 139/140 and Johann Werner's value of 23;28,30° for the end of 1515, which Copernicus himself employed for the reduction of his observations of Spica made in 1515 and 1525. (His own value of 23;28,24° for 1525 is merely computed from the completed theory.) Werner's *De motu octavae sphaerae* was published in 1522, and Copernicus's critical observations on it are in a letter dated June 3, 1524. This seems much too late for the date of composition of the *Commentariolus*.

apogee was misunderstood to be tropical, with the result that the apogee seemed to have regressed between al-Battānī and az-Zarqāl. This misunderstanding is already present in the *Almagestum minor* III, 11, the probable source of the error in the *Epitome*. The longitude of the apogee attributed to az-Zarqāl, 77;50°, is from the *Toledan Tables* where it is in fact for the Epoch of the Hijra, not az-Zarqāl's own time. Copernicus perpetuates the error in *De rev.* III, 20, and, after expressing great puzzlement at this insoluble problem, ends up dropping az-Zarqāl's value from further consideration. It is strange that it never occurred to him that az-Zarqāl might be following his own practice of measuring longitudes sidereally. It is even stranger that he at the same time managed to read into the bad account in the *Epitome* a correct description of az-Zarqāl's model identical to his own, unless this indicates that he had some further source of information about az-Zarqāl, which, however, is unlikely. See also below p. 458, n. 2. In any case, at the time he wrote the *Commentariolus*, he had either not given serious attention to the motion of the solar apogee in *Epitome* III, 13 or was simply content to use the sidereally fixed apogee in the *Alphonsine Tables*.

Owing to these problems of inconsistency with other parameters and difficulties in chronology, I think that the 10;25° difference between the longitude of Pollux and the direction of the earth's apsidal line cannot be taken as the source of the 10° difference in the *Commentariolus*.

In computing longitudes from Copernicus's model, we find angle $\alpha$ of the mean anomaly, the distance of the earth from the aphelion $B$, which follows from

$$\alpha = \bar{\sigma}^* - \lambda^*_A = \bar{\lambda}^*_\odot - \lambda^*_A,$$

and then angle $\delta$ from

$$\tan\delta = \frac{|e\sin\alpha|}{R + e\cos\alpha}.$$

(We shall frequently use tangent formulas for simplicity even though Copernicus does not employ them.) Then the apparent longitude of the sun is given by

$$\lambda^*_\odot = \bar{\lambda}^*_\odot \pm \delta. \quad \begin{array}{l} - \text{ for } 0° \leq \alpha \leq 180° \\ + \text{ for } 180° \leq \alpha \leq 360° \end{array}$$

To convert $\lambda^*_\odot$ to the tropical longitude $\lambda_\odot$, we must add the true precession $\pi$. Copernicus has not yet derived a value of precession, so this cannot be done. He specifies only one period with any degree of precision in the *Commentariolus*, and that is the length of the sidereal year, but since he gives no epoch position, we cannot even compute the sidereal longitude of the sun as shown above.

## THE DAILY ROTATION

The second motion of the earth, and this one most certainly belongs to it, is that of the daily rotation turning swiftly on its poles in the order of the signs, that is, toward the east, on account of which the entire universe appears to be driven around in a headlong whirl. And thus the earth rotates together with the water that flows around[a] it and the nearby air.

[a] 190:10 *circumflua* aqua (SV circumfluis)

It is, I believe, significant that Copernicus calls the daily rotation the *second* motion of the earth in the *Commentariolus* (he puts it first in *De revolutionibus* I, 11 because it accounts for the "first" motion) since it is a necessary consequence of the annual motion while the annual revolution in no way follows from the daily rotation. This shows the order followed by Copernicus in his analysis of planetary motions. He did not begin his investigations with the assumption that the earth rotated; his concern was planetary theory. Having settled on the heliocentric theory and the annual motion of the earth, the diurnal rotation followed immediately as an inevitable (although perhaps initially an undesirable) corollary. One could, in principle, have a heliocentric theory without the daily rotation of the earth, but the consequences would strain the credulity of even the purest geometer.

### THE MOTION OF THE INCLINATION

The third is the motion of the inclination. Now the axis of the daily rotation is not parallel to the axis of the great sphere, but is inclined [to it] by an arc of about $23\frac{1}{2}°$ in our time. Therefore, while the center of the earth always remains in the plane of the ecliptic, that is, on the circumference of a circle on the great sphere, its poles are carried around, describing small circles on both sides about centers [lying on a line] parallel to the axis of the great sphere. This motion also completes revolutions lasting almost one year and nearly equal to [the motion of] the great sphere. But the axis of the great sphere preserves an invariable alignment with respect to the sphere of the fixed stars, directed toward the points called the poles of the ecliptic. Likewise, the motion of the inclination combined with the motion of the [great] sphere would always keep the poles of the daily rotation directed toward the same points of the heavens if their periods of revolution were exactly equal. Now, after a long period of time it has been found that the alignment of the earth with respect to the configuration of the fixed stars changes, on account of which it has seemed to many that the sphere of the fixed stars itself is moved by several motions. Although the rule [governing this motion] is not yet adequately understood,[a] it is, however, less extraordinary that all these things can occur through the movement of the earth. It is not for me to say what the poles of this motion are connected to. In more common matters, I know, of course, that an iron needle rubbed by a loadstone will always point toward the same region of the world. Still, it has seemed a preferable theory that [this motion] is brought about by some sphere, [and that] the poles themselves are moved in the direction of the motion of this sphere, which without doubt will necessarily be under the moon.

[a] 190:27 visum est. Lege nondum satis deprehensa, posse autem

The third motion of the earth is intended to account for the precession of the equinoxes and possibly a variation of the obliquity of the ecliptic although Copernicus only vaguely refers to the latter. The precession of the equinoxes was discovered by Hipparchus from a comparison of the tropical longitudes of fixed stars observed by Timocharis and himself (*Almagest* VII, 2; *Epitome* VII, 2). This discovery leads to two possible interpretations. Either (1) the equinoxes are precessing westward with respect to an unmoving sphere of the fixed stars, or (2) the sphere of the fixed stars is moving eastward with respect to fixed equinoxes. The former interpretation means that the axis of the diurnal rotation and the celestial equator are moving with respect to a fixed ecliptic and sphere of the fixed stars. The latter means that the sphere of the fixed stars is moving with respect to a fixed ecliptic, celestial equator, and axis of diurnal rotation. As long as the rate of this motion is uniform, it does not matter which alternative is chosen. It is possible that Hipparchus chose the former interpretation; it is certain that Ptolemy chose the latter. According to Ptolemy, Hipparchus estimated the rate of precession as at least 1° in 100 years, and Ptolemy confirmed precisely this value with a large number of observations.[3]

An altogether different theory is reported by Theon of Alexandria in his introduction to Ptolemy's *Handy Tables*, and Theon's account is reproduced at length by al-Battānī who erroneously gives his source as Ptolemy.[4] Theon reports that according to this theory the tropic points move first 8° to the east and then back 8° to the west (total arc 8° *not* 16°) at the uniform rate of 1° in 80 years. The origin of this theory is unknown, but it is probably pre-Ptolemaic and has some connection with a confusion caused by the Babylonian and then early Greek custom of placing the equinoxes and solstices at 8° of their respective signs rather than at 0°. In Battānī's account, it is not the solstices, but the sphere of the fixed stars that moves back and forth 8°. A summary of the theory, drawn from Battānī, is given by Regiomontanus in *Epitome* VII, 6. Owing to its brevity, Regiomontanus's account is not absolutely clear about whether the total oscillation is 8° or 8° on each side of a midpoint. Copernicus (*De rev.* III, 1) took the wrong interpretation and reports an oscillation not greater than 8° on either side, that is, about 16°. It is important to note that the oscillation in this theory is uniform with a sudden reverse of direction at the end of the 8° arc, so it has no connection with later theories of a non-uniform precession.

A different rate of continuous precession was derived by Battānī from the comparison of his own observations of stellar longitudes with those observed by Menelaus and reported by Ptolemy.[5] He found a rate of 1° in about 66 years, and specifically draws attention to the acceleration of this motion and the decrease in the length of the tropical year.[6] This now introduces a problem. As long as the rate of precession and the tropical year were uniform, it made no difference whether one spoke of a precession of the equinoxes or an eastward motion of the sphere of the fixed stars. Now, however, one must ask which is moving and whether the length of the sidereal year is uniform.

These problems are taken up in two treatises by Thābit ibn Qurra who reaches results that appear incompatible.[7] In *De anno solis* he shows that the sidereal year is uniform and equal to the anomalistic year so that the solar apogee is sidereally fixed. He derives different values for the sidereal year based on different pairs of observations, but settles on $365;15,23,34,43^d$ ($= 365^d6;9,25,53,12^h$). Thābit also finds a motion of the fixed stars, and hence of the solar apogee, of 0;49,39° per year, and from this, taking the mean daily motion of the sun to be about 0;59,8°, derives a tropical year of

$$365;15,23,34,43^d - \frac{0;0,49,39^\circ}{0;59,8^{\circ/d}}$$
$$= 365;15,23,34,43^d - 0;0,50,22,34^d$$
$$\approx 365;14,33,12^d (= 365^d5;49,16,48^h).$$

This tropical year is uniform. His only attempt to explain a non-uniform tropical year is a brief discussion of measuring the return of the sun to a solstice which is close to the apogee but moving so that the true (and hence, irregular) motion of the sun over the arc of precession interferes with a precise measurement. This shows nothing more than that, if the apogee of the sun moves, and in this case moves uniformly, then the true return of the sun to some point not moving with the apogee, for example, a solstice, will be non-uniform. All this says nothing about the diminution of the length of the tropical year.

In *De motu octavae sphaerae*, Thābit describes a geometrical model for a non-uniform motion of the sphere of the fixed stars which the apsidal lines of the sun and planets are assumed to share. The model also produces a variation of the obliquity of the ecliptic, but this concerns us here

[3] Pannekoek (1955) should be consulted on Hipparchus's and Ptolemy's rate of precession.

[4] Battānī, cap. 52; Nallino, 1: pp. 126–128 and notes pp. 298–304.

[5] *Almagest* VII: 3; *Epitome* VII, 5 & 6; Battānī, cap. 51; Nallino, 1: pp. 124–125, 292–295.

[6] Battānī, cap. 52; Nallino, 1: p. 127.

[7] Neugebauer, 1962₁.

only insofar as it is an attempt to explain simultaneously both an irregular precession and a change of obliquity as Copernicus does also. Thābit's model is described in detail in Peurbach's *Theoricae novae planetarum*, so it was certainly known to Copernicus. The model is shown in figure 3. We define the fixed equinox ♈̄ as the intersection of the equator and the fixed ecliptic, their angle of intersection being the fixed obliquity $\bar{\epsilon} = 23;33^\circ$. A circle of radius $r = 4;18,43^\circ$ is described about ♈̄ and on this circle a point ♈*0°, sidereal Aries 0°, moves through an angle $\vartheta$, and carries with it the movable ecliptic which meets the fixed ecliptic at ♋* and ♑*. With the movable ecliptic move the entire sphere of the fixed stars and the apogees of the sun and planets, while presumably the sun itself moves on the movable ecliptic, and the planes of the various planetary orbits are defined with respect to their intersection with the movable ecliptic. The intersection of the movable ecliptic and the equator is the true equinox ♈ which coincides with ♈*0° when ♈*0° is on the equator with $\vartheta = (0^\circ, 180^\circ)$, but elsewhere moves back and forth non-uniformly through an arc $\delta$ of the movable ecliptic measured on either side of ♈*0° such that the maximum distance is $\pm 10;45^\circ$ and its motion very close to

$$\text{♈}^*0^\circ - \text{♈} = \delta = 10;45^\circ \sin\vartheta.$$

$\delta$ is then added to sidereal longitudes to produce tropical longitudes. The true obliquity $\epsilon$ will also vary in a curious way which we need not consider. What we do want to know, however, is whether the mean motions of the sun and planets are uniform with respect to ♈*0° or the true equinox ♈, that is, whether their sidereal or tropical periods are uniform. Thābit does not explicitly, but he remarks (in Section [12]) that the motion of ♈*0° is common to the spheres of all the planets and is the only motion of the sphere of the fixed stars. This could mean only that the apsidal lines are siderealIy fixed, but probably means that all motions of the planetary spheres, and hence all planetary periods, are uniform with respect to ♈*0°, and thus the sidereal, not the tropical, periods are uniform. This was certainly the way Thābit's theory was understood by the compiler of the *Toledan Tables* in which mean motions and epoch positions are sidereal, and the conversion to tropical longitude is done with the tables for Thābit's motion which are always provided.

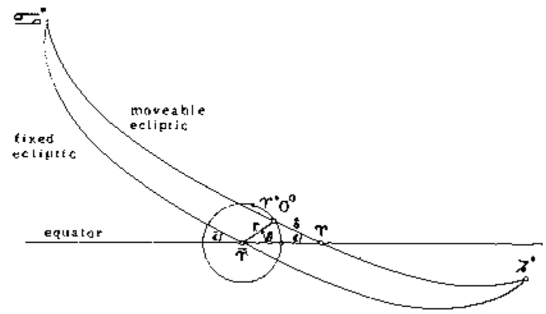


Fig. 3

Comparing Thābit's two treatises, we see that in *De anno solis* both the sidereal year and the precession are uniform, and hence the tropical year must also be uniform, while in *De motu octavae sphaerae* he probably intends a uniform sidereal year along with an irregular trepidation, so the tropical year is non-uniform. This latter arrangement is also found in the *Toledan Tables*, which are possibly the work of az-Zarqāl, and in other works of az-Zarqāl.[8]

The uniform sidereal year and the trepidation of the eighth sphere are combined without hesitation in the description of Thābit's theory in *Epitome* III, 2, on the length of the tropical year.

> On account of an irregularity of this kind in the length of the [tropical] year found by different astronomers, although investigated by similar methods and instruments, Thābit, searching for the cause of this irregularity, was led to assume a motion of the eighth sphere (which we call the trepidation) on two small circles on which the movable first points of Aries and Libra are carried around. He attempted by this assumption to account for both variations of inclinations of the ecliptic and different lengths of the [tropical] year, as is clear in examining the property of this motion. He also said that the length of the year is not the time from equinox to the same equinox nor from solstice to the same solstice, but rather the return of the sun from any point of the movable ecliptic back to the same point, or the return of the sun from any fixed star back to the same star, which he said takes place in 365 days 6;9,12 hours [= $365;15,23^d$].

This length of the sidereal year, rounded from the more precise value in *De anno solis*, is cited by Copernicus in *De rev.* III, 13. The account in the *Epitome* provides the same conditions maintained by Copernicus, that is, a uniform sidereal year, a non-uniform tropical year, and a non-uniform precession, although Thābit assumes

[8] Toomer, 1968: pp. 118–122; az-Zarqāl, *El Tratado sobre el movimiento de las estrellas fijas*, in Millas Vallicrosa, 1943–1950: pp. 239–343; Goldstein, 1964.

that the precession will some day reverse direction. While this last assumption may look foolish since no evidence for this surprising occurrence could be put forth, Thābit's invention does show a not unintelligent attempt to account simultaneously for three possibly related motions—the irregular precession, the diminution of the length of the tropical year, and the diminution of the obliquity of the ecliptic—and in its way does exactly what Copernicus seeks to accomplish in Book III of *De revolutionibus*, although Copernicus insists on adding yet another irregular motion to this collection, the diminution of the eccentricity of the earth's orbit.

The *Alphonsine Tables* return to a uniform tropical year—its length, $365;14,33,9,57\ldots^{d} = 365^{d}5;49,15,58,48^{h}$ is very close to Thābit's—and assign to the sphere of the fixed stars and the apsidal and nodal lines a combined uniform precession and irregular trepidation. The period of the precession is 49,000 years so its motion is about $0;0,26,26,54,22,40,45^{\circ}$ in a tropical year. The mean sidereal year is therefore

$$365;14,33,9,57^{d} + \frac{0;0,26,26,54,22,40,45^{\circ}}{0;59,8^{\circ/d}}$$
$$= 365;14,33,9,57^{d} + 0;0,26,50,9^{d}$$
$$= 365;15,0,0,6^{d} \approx 365\tfrac{1}{4}^{d}.$$

This much is simple and obvious, but its origin remains a mystery. The mean sidereal year, a cleverly achieved fake, is a Julian year, but the tropical year is very good and Copernicus's mean tropical year in *De revolutionibus*, $365;14,33,11,12\ldots^{d} = 365^{d}5;49,16,28^{h}$ is very close to it. The irregular component has a period of 7000 years, an amplitude of $\pm 9^{\circ}$, and the tabulated equation is close to $9^{\circ} \sin \vartheta$. In the period between A.D. 1 and A.D. 7000 the combined precession and trepidation will reverse direction from about A.D. 3000 to A.D. 4000, otherwise it is direct. The model underlying the irregular motion is unknown (it could be the same as Thābit's which is also tabulated as nearly a sine function), and no variation of obliquity is associated with it. Peurbach attempted to develop a model for it in the *Theoricae novae planetarum*. Johann Werner's criticisms of the Alphonsine precession in *De motu octavae sphaerae tractatus secundus* are directed partially against its accuracy, partially against John of Saxony's instructions for the use of the tables, and partially against Peurbach's model. Since the Alphonsine tropical year is uniform, while the sidereal year varies, it in no way accounts for the change of the length of the tropical year.

We can now consider the problems Copernicus took up in treating the precession. He knows and wishes to account for the following:

1. The apsidal and nodal lines of the sun and planets are sidereally fixed, and move along with the sphere of the fixed stars with respect to the equinoxes.
2. The length of the tropical years, as he shows in the following section, is non-uniform.
3. The length of the sidereal year is uniform. We shall show how he determined this in the commentary to the next section.
4. If the sidereal period of the sun is uniform, then the sidereal periods of all the planets must also be uniform since otherwise their anomalistic periods (return to mean conjunction with the sun) would gradually move out of phase with the motion of the sun. Therefore, their tropical periods are non-uniform. A further consideration here is that, at the end of the next section, Copernicus seems to draw the immediate conclusion that, since the apsidal lines are sidereally fixed, the sidereal periods of the planets must be uniform. This probably follows from the belief that, if the apsidal lines are sidereally fixed, the actual positioning of the planetary spheres is also sidereally fixed, and therefore the periods of the motions of the epicycles or the planets themselves caused by the revolutions of their spheres must also be uniform with respect to the sphere of the fixed stars rather than to some point, for example, an equinox, which does not move uniformly with respect to the sphere of the fixed. In any case, the uniformity of the sidereal year is by itself enough to necessitate uniform sidereal periods for all the planets.
5. The difference between the sidereal and tropical year is the precession, and since the tropical year is non-uniform, the precession must be non-uniform.

In sum, the apsidal and nodal lines are fixed, and the solar and planetary periods are uniform with respect to the sphere of the fixed stars, while with respect to the equinoxes the apsidal and nodal lines and the fixed stars move non-uniformly, and the solar and planetary periods are non-uniform. Copernicus now has two choices. He can either assign to the sphere of the fixed stars and to the spheres, apsidal lines, and nodal lines of every planet an identical motion that is non-uniform with respect to a hypothetically fixed equinox, which is what is done in the *Toledan Tables*, or he can let the sphere of the fixed stars, the apsidal and nodal lines, and hence the positioning of the planetary spheres with respect to the fixed stars, be at rest and grant a single, necessarily non-uniform motion to the earth's axis that will shift the equinox non-uniformly along the ecliptic. His choice was obvious and correct. Since the annual motion of the earth so completely replaces the motion of the second anomaly of all the planets, and the diurnal rotation of the earth replaces the diurnal rotation of the entire universe, it was a more than reasonable step to replace the precessional motion common to all the planets and the fixed stars by a single motion of the earth. This is the *third* motion of the earth. It was, I believe, the last of the earth's motions that Copernicus arrived at in his analysis of planetary motion, and it is the least completely developed section in the *Commentariolus*, being little more than a qualitative model, put forth tentatively and not yet able to account for all the motions it is supposed to replace. Copernicus's explanation itself presents several difficulties:

1. The earth's third motion, *motus declinationis*, translated here as "motion of the inclination" (the transliteration "motion in declination" would be meaningless), is the circular motion of the earth's inclined axis that keeps it parallel to itself as the earth moves about the sun. The term *motus declinationis*, retained by Copernicus in *De revolutionibus*, is not very clear, and its meaning becomes even less clear when he uses it in III, 3 to refer to the variation of obliquity which is a different motion of the earth's axis that is not circular, but reciprocal, and is more properly a motion of the inclination.

2. The annual motion of the center of the earth completes a revolution in a sidereal year while the motion of the inclination completes a revolution in the opposite direction in a mean tropical year. Copernicus explains none of this, and in his treatment of the latitude of the moon, he contrasts the regression of the lunar nodes with the motion of the inclination in such a way that it seems as though he did not realize that the motion of the inclination must be opposite to the annual motion.

3. The difference between the annual motion and the motion of the inclination is the precession. In the following section, Copernicus states that the sidereal year is of constant length, but the tropical year is of variable length. Therefore, the rate of precession is not uniform, but nothing is said about it in describing the motion of the inclination.

4. The statement that the obliquity of the ecliptic is about $23\frac{1}{2}°$ "in our time" leads one to believe that Copernicus will have something to say here about a variable obliquity in some way related to the motion of the inclination, as he does in *De revolutionibus*. However, he says nothing more about the obliquity. In connection with this, a variant reading in S for 190:32 would make the second to the last phrase of this section read "the poles themselves are moved in the direction of the inclination (*nutum*) of this sphere," which, if correct, seems to refer to a variable obliquity. Accepting this variant would make Copernicus's description still more problematical, but it may indeed be what he wrote.

With these problems in mind, and assuming that Copernicus did correctly understand the direction and period of the motion of the inclination mentioned in (2) above, we can describe Copernicus's model as follows (see fig. 4):

We let the center of the great sphere be $\bar{S}$, place the earth on the circumference of the great sphere at $T_0$, and define the sidereally fixed direction $\bar{S}T_0\Upsilon^*$. The axis of the great sphere has the sidereally fixed direction $E^*$, while the axis of the earth's daily rotation is inclined from $E^*$, that is, from the axis of the ecliptic by angle $\epsilon \approx 23\frac{1}{2}°$, and has the initial sidereal direction $F_0^*$. The earth then moves from $T_0$ to $T$ through angle $\bar{\sigma}^*$ of its mean sidereal motion, completing a revolution with respect to $\Upsilon^*$ in a sidereal year. In the same time, the earth's axis will turn in the opposite direction from $V_0$ to $V$ through angle $\bar{\sigma}$, completing a revolution in a mean tropical year. If $\bar{\sigma} = \bar{\sigma}^*$, the earth's axis would remain completely parallel to itself, and hold the direction $F_0^*$. Since, however, the tropical year is shorter than the sidereal year, therefore $\bar{\sigma} > \bar{\sigma}^*$, and the direction of the earth's axis will advance from $F_0^*$ to $F^*$ through the angle of the precession $\pi = \bar{\sigma} - \bar{\sigma}^*$. This will cause the equinox to move westward along the ecliptic, so a precession of the equinoxes with respect to the fixed stars and all other sidereally fixed points, such as apsidal and nodal lines, has been achieved.

So far, however, Copernicus has developed a model for only a uniform precession, while the variation of the length of the tropical year treated in the following section clearly demands a non-

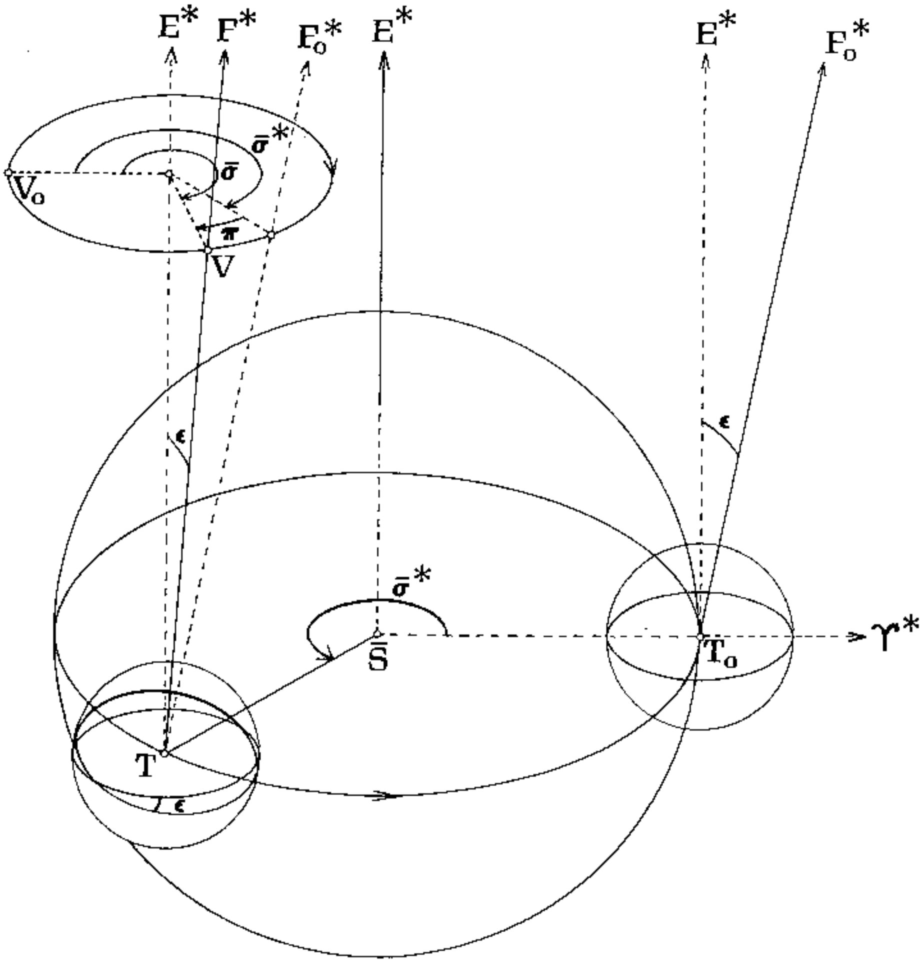


Fig. 4

uniform precession, as he surely knows. Evidently, Copernicus has not yet invented the model for non-uniform precession contained in Book III of *De revolutionibus*, or any other model for that matter, as the hesitant language at the end of his treatment of the third motion shows. Nor has he found a model for the variation of the obliquity that he seems, obliquely, to have referred to in this section. And finally, no parameters for any of these motions are mentioned, from which we can conclude that he has neither derived any of his own nor trusts any existing parameters from earlier sources sufficiently to extract them for his own use.

All in all, the theory of precession is the most primitive model in the *Commentariolus*. Nevertheless, Copernicus has succeeded in his principal object of removing precessional motion from the sphere of the fixed stars and the apsidal and nodal lines of the planets, replacing it by a single motion of the earth. What remains to be done, and is carried out in Book III of *De revolutionibus*, is to account for the irregularity of the precession and the variation of the obliquity. When did Copernicus do this? At the end of the letter criticizing Werner, dated June 4, 1524, Copernicus tells Wapowski, in all too typical Copernican fashion:

> What, finally, do I think about the motion of the sphere of the fixed stars? Since [my theories] are intended for another place, I have thought it unnecessary and irrelevant to dwell upon [them] further here . . . (Prowe, 2: pp. 182–183).

This is not terribly helpful, but he evidently has a theory and intends to publish it. However, the derivation of the parameters for the irregular precession in Book III depends upon material from Werner's book, so it is likely that whatever Copernicus had in mind when he wrote the letter was revised still later, ironically, with the assistance of a work he attacked harshly.

### 4. THAT UNIFORM MOTIONS SHOULD BE MEASURED NOT WITH RESPECT TO THE EQUINOXES, BUT TO THE FIXED STARS

Since the equinoxes and the other cardinal points of the universe move considerably, whoever attempts to derive the uniform annual revolution from them is necessarily mistaken, for it has been found to be irregular by many observations in different ages. Hipparchus found it to be $365\frac{1}{4}$ days, al-Battānī, the "Chaldaean," found such a year to be 365 days 5 hours 46 minutes, that is, $13\frac{3}{5}$ or $\frac{1}{3}$ minutes shorter than [the year found] by Ptolemy (*sic!*). Again, *Hispalensis*[a] found it longer than the year found by al-Battānī[b] by 1/20 part of an hour since he fixed the tropical year at 365 days 5 hours 49 minutes.

Lest it be thought that this irregularity has resulted from error in the observations, if one considers each of them carefully he will find that the irregularity has always corresponded to the movement of the equinoxes. For when the cardinal points of the world moved at the rate of 1° in 100 years, as was found in the time of Ptolemy, the length of the year was then what was reported by Ptolemy himself. When, however, in the following centuries they moved with a faster motion, since they are opposite in direction to the lower motions, the year became shorter as the shift of the cardinal points became faster, for by their faster motion they intercepted the annual motion in a shorter time. Therefore, whoever measures the annual uniform [motion] with respect to the fixed stars will do better. For example, we have done this with respect to *Spica Virginis*, and have found that the year has always been 365 days 6 hours and about $\frac{1}{6}$ of an hour, much as it was found even in ancient Egypt. The same principle should also be applied to the other motions of the planets since their apsides, which are also fixed with respect to the sphere of the fixed stars (and thus heaven itself by truthful evidence), show the rules of their motions.

[a] 191: *Hispaniensis*? (would be written Hispāiensis)
[b] 191:11 *hoc* longiorem (SV huic)

The summary of various lengths of the tropical year is frightfully garbled, and while part of it is easily explained by reference to Copernicus's source for the information, part is sheer nonsense resulting either from manuscript corruption or carelessness by Copernicus. The first error, the attribution to Hipparchus of a tropical year of $365\frac{1}{4}$ days instead $365\frac{1}{4} - \frac{1}{300}$ days, is found in *Epitome* III, 2, which gives Ptolemy's tropical year correctly as $365\frac{1}{4} - \frac{1}{300}$ days and al-Battānī's as $365\frac{1}{4} - \frac{1}{106}$, days, that is, $365\frac{1}{4}$ days less $0;13\frac{3}{5}$ hours or $365^{d}5;46\frac{2}{5}^{h}$. The misinformation about Hipparchus in the *Epitome* was taken from the so-called *Almagestum minor* III, 1 (unpublished) which in turn is a careless copy of al-Battānī's statement that Hipparchus found the year to be $365\frac{1}{4}$ days "although he showed it to be less than this."[1] It is curious that in *De revolutionibus* III, 13, Copernicus persists in something rather like Battānī's error, reporting that Hipparchus found the year to be a little less than $365\frac{1}{4}$ days, while Ptolemy showed it to be $365\frac{1}{4} - \frac{1}{300}$ days.

[1] Battānī, cap. 27; Nallino, 1: p. 40; also cap. 52; Nallino, 1: p. 127, and better, Plato of Tivoli's version where $365\frac{1}{4}$ days is attributed to Hipparchus without qualification.

Now that Copernicus's source has been established, we can see what his account *should* say:

Hipparchus found it to be $365\frac{1}{4}$ days, *Ptolemy $365\frac{1}{4} - \frac{1}{300}$ days*, al-Battānī, the "Chaldaean" found such a year to be 365 days 5 hours 46 minutes, that is, $13\frac{3}{5}$ minutes *shorter than* [*the year found*] *by Hipparchus* or $8\frac{1}{2} + \frac{1}{3}$ minutes shorter than [the year found] by Ptolemy.

This admittedly drastic restoration would at least give sense and consistency to the text, but I hesitate to tamper with the text since the trouble may be due to Copernicus in the first place.

The last value cited for the tropical year, 365 days 5 hours 49 minutes is that of the *Alphonsine Tables* so it is likely that *Hispalensis* (*Hispaniensis?*) refers to Alfonso X who was, after all, a Spaniard.[2]

Now, assuming that Copernicus originally understood all this correctly, he has the following values for the length of the tropical year:

| | |
|---|---|
| Hipparchus | $365;15^d = 365^d6^h$ |
| Ptolemy | $365;14,48^d = 365^d5;55,12^h$ |
| al-Battānī | $365;14,26^d = 365^d5;46,24^h$ |
| Alphonsine Tables | $\sim 365;14,33,10^d = 365^d5;49,16^h$ |

Therefore, the difference between Battānī and Hipparchus is

$$365^d6^h - 365^d5;46,24^h = 0;13,36^h = 13\tfrac{3}{5}{}^m,$$

between Battānī and Ptolemy

$$365^d5;55,12^h - 365^d5;46,24^h = 0;8,48^h \approx 8\tfrac{1}{2} + \tfrac{1}{3}{}^m,$$

and between the *Alphonsine Tables* and Battānī

$$365^d5;49,16^h - 365^d5;46,24^h = 0;2,52^h \approx 0;3^h = 1/20^h.$$

[2] The identification of Hispalensis with Alfonso de Cordoba Hispalensis made by L. Birkenmajer and followed by A. Birkenmajer (1933: p. 96), Rossmann (1948: p. 42), and Rosen (1971: p. 66) is possible, but seems unlikely since $365^d5;49^h$ is simply a rounding of the tropical year in the *Alphonsine Tables* which is, after all, the source of almost every parameter in the *Commentariolus*. All Alfonso de Cordoba did for Zacuto's *almanach perpetuum* (in the 1502 Venice edition only) was to extend the almanach for Venus. The tropical year mentioned by him, like the tropical year in Zacuto's almanach, is simply the Alphonsine value. Could *Hispalensis* in fact be a misreading of *Hispaniensis*, which would undoubtedly refer to good king Alfonso?

Copernicus next says that the rate of precession has always corresponded to the length of the tropical year. This means that *if the sidereal year is uniform*, the reported rated of precession has always corresponded to the difference between the reported tropical year and the uniform sidereal year. Therefore, the shorter the tropical year, the faster the precession. In order to prove this, Copernicus must first find the length of the sidereal year and show that it is uniform, and then show that the rate of precession has always corresponded to the required difference. All of this, however, is circular, since the length of the sidereal year must be found in the first place by adding an observed or computed precessional motion of some fixed star to either a given tropical year or to two widely spaced observations of the equinox, which in themselves lead to some length for the tropical year. Now, if Copernicus can do this with a number of tropical years and precessions, and in each case reach the same or nearly the same value for the length of the sidereal year, he can consider that he has proved his point, that is, that the sidereal year is uniform and the rate of precession always corresponds to the difference between the sidereal and tropical year.

He explains that he has determined the length of the sidereal year by means of Spica, and has found that it has always been 365 days 6 hours and about $\frac{1}{6}$ of an hour. The remark that this is much the same as the old Egyptian value again comes from *Epitome* III, 2 where "the most ancient Egyptians" are credited with a sidereal year of $365\frac{1}{4} + \frac{1}{130}$ days, that is $365;15,27,41,32^d$ or $365^d6;11,5^h$, in fairly good agreement with Copernicus's value. The source for the *Epitome* is again the *Almagestum minor* III, 1, where, however, the sidereal year of $365\frac{1}{4} + \frac{1}{130}$ days is attributed to the Egyptians and Babylonians. This in turn is derived from al-Battānī in Plato of Tivoli's translation where the fraction is 1/131.[3] Reference has already been made to Thābit's sidereal year cited in *Epitome* III, 2 as $365^d6;9,12^h$, which is also close to Copernicus's 365 days 6 hours and about $\frac{1}{6}$ hour.

Copernicus's derivation of this value for the sidereal year from a measurement of the motion of the sun with respect to Spica can in fact be

[3] Battānī, cap. 27; Nallino, 1: p. 40 n. 1.

reconstructed. Better still, in his copy of the 1515 edition of the Gerard of Cremona translation of the *Almagest*, Copernicus noted down a still more precise value for the sidereal year that probably shows the exact number behind the rough expression in the *Commentariolus*, and this number can be recomputed with full precision. L. Birkenmajer (p. 248) transcribes from Copernicus's copy of the 1515 *Almagest* the following numbers:

$$0;59,8,11,16,12$$

and

$$365;15,24,45.$$

The former is the mean daily sidereal motion of the sun, and the latter is the length of the corresponding sidereal year which is equal to $365^d6;9,54^h$, certainly in perfect agreement with the $\sim\frac{1}{6}$ hour in the *Commentariolus*. Of course, we cannot know that this precise value is prior to the rough one in the *Commentariolus*, especially since the *Commentariolus* was probably written before Copernicus owned the 1515 *Almagest* in which he wrote these numbers. However, the exact value can be recomputed from information found in the *Epitome*, and there is nothing to prevent Copernicus from writing down a number he computed years earlier.

The sidereal year of $365;15,24,45^d$ is derived from the motion of the sun with respect to Spica as follows: In *Epitome* VII, 4, Regiomontanus says that between the time of Hipparchus and Ptolemy Spica moved 2;40° in 265 Egyptian years. Therefore, in 265 Egyptian years the difference between the sidereal and tropical motion of the sun must be 2;40°, so however far the sun may move in 265 Egyptian years with respect to an equinox (the amount need not be computed here), it will have to advance 2;40° farther to move the same distance with respect to Spica. The time required by the sun to move this distance is

$$\frac{\pi}{\bar{\lambda}_{\odot}{}^{\circ/d}} = \frac{2;40^\circ}{0;59,8^{\circ/d}} = 2;42,20,42^d.$$

Thus in one Egyptian year, the time required by the sun to overtake Spica's motion with respect to an equinox is

$$\frac{2;42,20,42^d}{265} = 0;0,36,45^d,$$

and since the tropical year exceeds the Egyptian year by only about 1/1500 of a tropical year, without appreciable error the sidereal year exceeds the tropical year by $0;0,36,45^d$. Now, as noted above from *Epitome* III, 2, Ptolemy's tropical year is $365;14,48^d$. The corresponding sidereal year derived from the motion of the sun with respect to Spica is therefore

$$365;14,48^d + 0;0,36,45^d = 365;15,24,45^d$$

in exact agreement with Copernicus's note in the 1515 *Almagest*. Since

$$365;15,24,45^d = 365^d6;9,54^h \approx 365^d6\tfrac{1}{6}^h$$

it is not unreasonable to assume that this was Copernicus's method of deriving the length of the sidereal year in the *Commentariolus* and may indeed be the very computation he carried out. The method is exceedingly simple, and this is because Copernicus can begin with a length for the tropical year that he assumes in advance to be correct. If one desires to find the sidereal year without assuming a tropical year, it is necessary to use two widely spaced observations of the sun *with tropical coordinates*, as for example, two equinox observations as in *De revolutionibus* III, 18, and then the solar equation must be taken into account. However, since Copernicus specifically rules out the possibility of error in the various tropical years cited in the *Commentariolus*, he is justified in using the simple method shown above.

He must next show that the other tropical years with their corresponding rates of precession will also lead to a sidereal year of about $365^d6;10^h$. These computations can also be reconstructed although not with the full precision of the previous derivation since there is no way of knowing the exact result of Copernicus's computation. Consider first al-Battānī. According to *Epitome* VII, 6, Battānī's rate of precession is 1° in about 66 solar years, and this is accompanied by several fixed star observations by Menelaus, Ptolemy, and Battānī that contain errors both in chronology and longitude, and lead to conflicting results. Nevertheless, the constant of precession itself allows us to compute the sidereal year corresponding to Battānī's tropical year. With a precession of 1° in 66 solar years, we find that 66 sidereal years exceed 66 solar years by

$$\frac{\pi}{\bar{\lambda}_{\odot}{}^{\circ/d}} = \frac{1^\circ}{0;59,8^{\circ/d}} = 1;0,53^d,$$

and therefore the difference in the length of the tropical and sidereal year is

$$\frac{1;0,53^d}{66} \approx 0;0,55,21^d.$$

Using Battānī's tropical year of 365;14,26$^d$, the sidereal year is

$$365;14,26^d + 0;0,55,21^d = 365;15,21,21^d = 365^d6;8,32^h$$

which agrees fairly well with $\sim\frac{1}{6}$ hour. A slightly closer result can be obtained from using the observations of β Scorpionis by Menelaus and Battānī cited in *Epitome* VII, 6, and also by Copernicus in *De rev.* III, 2, which lead to a sidereal year of 365;15,21,40$^d$ = 365$^d$6;8,40$^h$.

For finding the sidereal year corresponding to the Alphonsine tropical year we must first decide what rate of precession to use. A likely possibility is comparing stellar longitudes from the Alphonsine star catalog with Battānī's observations in *Epitome* VII, 6. This, unfortunately, leads to inconsistent results. Using β Scorpionis the precession is 5;38° which is more-or-less consistent with Battānī's and the Alphonsine star catalogs, but using Regulus it is 4;48° which is erroneous. The date of Battānī's observation is given in the *Epitome* as Era Nabonassar 1626, which Copernicus states equivalently in *De revolutionibus* III, 2 as Era Philipp 1202, while Era Alfonso corresponds to Era Philipp 1575 + 230 days. The difference is 373 Egyptian years and 230 days, which may safely be rounded to 374 Egyptian years. With the precession of 5;38°, the excess of the sidereal over the tropical year in 374 Egyptian years is

$$\frac{\pi}{\bar{\lambda}_{\odot}{}^{\circ/d}} = \frac{5;38^\circ}{0;59,8^{\circ/d}} = 5;42,57^d,$$

and in one Egyptian year or, without noticeable error, in one tropical year, the excess is

$$\frac{5;42,57^d}{374} = 0;0,55,1^d.$$

The length of the sidereal year is thus

$$365;14,33,10^d + 0;0,55,1^d = 365;15,28,11^d = 365^d6;11,16^h,$$

again in fairly good agreement with Copernicus's value.

In concluding this section, it may be observed that even though we cannot be certain of exactly how Copernicus carried out his computations, he certainly had adequate information to derive a sidereal year of about 365$^d$6;10$^h$ and to show that the reported lengths of the tropical year always corresponded more-or-less with their accompanying rates of precession. I have tried out a fair number of other pairs of tropical years and rates of precession between fixed star observations—various combinations of Hipparchus, Ptolemy, Battānī, the *Alphonsine Tables*, and even Copernicus—and the sidereal year always comes within ±0;2$^h$ of 365$^d$6;10$^h$.

## 5. THE MOON

### MOTION IN LONGITUDE

The moon appears to us to wander about with four motions in addition to the annual revolution that has been mentioned. For in its deferent sphere it completes revolutions in a [sidereal] month about the center of the earth in the order of the signs. The deferent sphere in turn carries what [astronomers] call the epicycle of the first anomaly or of the argument, but we call the first or larger epicycle, and[a] attached to the first epicycle is another epicycle which it carries around in a period [i.e., an anomalistic month] slightly longer than the [sidereal] month in the direction opposite to the motion of the sphere in [its] higher arc. Finally, the moon, fixed in the second epicycle, completes two revolutions in a [synodic] month in the direction opposite to the motion of the first epicycle, such that whenever the center of the larger epicycle reaches the line passing from the center of the great sphere through the center of the earth, which we call the semidiameter[b] of the great sphere, then the moon is closest to the center of the larger epicycle. This takes place near new and full moon, but on the other hand in quadratures, midway between new and full moon, it is most distant. The length of the semidiameter[c] of the larger epicycle is $\frac{1\frac{1}{18}}{10}$ part[d] (*sic*) of the semidiameter[e] of its deferent sphere, and the semidiameter of the smaller epicycle is $\frac{1}{5-\frac{1}{4}}$ part of the larger.

Now, because of these, the moon appears sometimes fast and sometimes slow, also sometimes descending and sometimes ascending, and the motion of the smaller epicycle introduces into the first anomaly a variation of two components.[f] For it withdraws the moon from uniform motion on the circumference of the larger epicycle, the maximum equation of which amounts in this case to $12\frac{1}{4}°$[g] of the circumference itself of length or semidiameter corresponding [to the larger epicycle],[h] and it also sometimes draws the moon away from and sometimes brings it toward the center[i] of the larger epicycle by an amount equal to the length of its semidiameter. Since, therefore, on account of the smaller epicycle the moon describes irregular circumferences of circles around the center of the larger epicycle, the result is that the first anomaly is altered in a complex way. It is for this reason that near conjunctions and oppositions with the sun the maximum equation of this anomaly does not exceed 4;56°, but in quadratures it is increased to 7;36°.[j]

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[a] 192:19 et *hic?* (S et *anni*, here omitted)
[b] 192:25 quam *semidiametrum* (S diametrum)
[c] 193:2 *semidiametri* epicycli (S diametri)
[d] 193:4 *10am unius partis . . . cum 18am decem particularum*, i. e., $\frac{1;0}{10} + \frac{0;10}{18}$ ? (S 10 partem . . . cum 18 unius particulae)
[e] 193:3 *semidiametro* (S)
[f] 193:8 *primae* quidem *diversitati* dupliciter *variationem* (S prima . . . diversitate . . . variationum)
[g] 193:9 *12* gradus et quadrantem (S 17)
[h] 193:10 de circumferentia ipsa quantitatis seu *semidiametri* respondentis, (S *diametri*)
[i] 193:11 *eam* quoque *a centro* maioris (S eum quoque centrum)
[j] 193:16 *7* gradus (S 6)

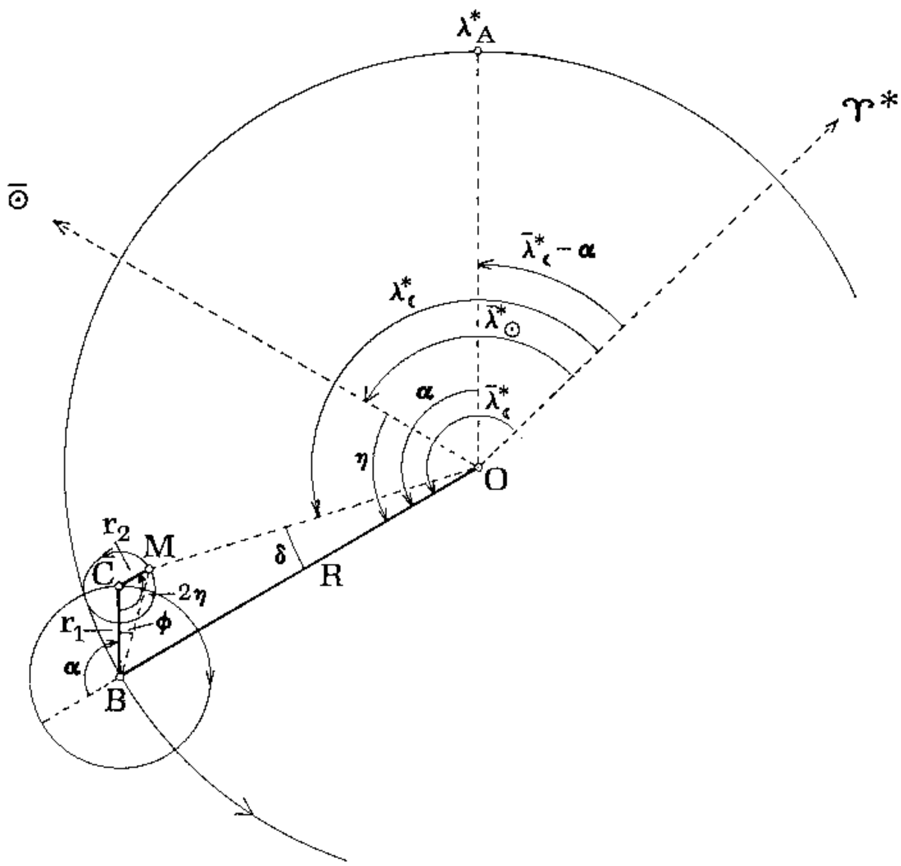


Fig. 5

Copernicus's lunar model is, except for its parameters, identical to the lunar model of Ibn ash-Shāṭir.[1] Both astronomers chose the radii of the two epicycles to produce predetermined maximum equations in syzygy and quadrature. Ibn ash-Shāṭir required maximum equations of 4;56° and 7;40°; Copernicus required 4;56° and 7;36°, in agreement with the *Alphonsine Tables.* Owing to an error in the expression of the radius of the larger epicycle in the text of the *Commentariolus* and in U, Copernicus's radii will not produce the maximum equations he requires. The description in the *Commentariolus* is also not aided by Copernicus's failure to distinguish between different kinds of months, so I have added these in the translation.

The model is shown in figure 5. The earth is located at $O$, and the center $B$ of the first epicycle of radius $r_1$ moves uniformly about $O$ on a circle of radius $R$ through angle $\bar{\lambda}^*_☾$, completing a revolution with respect to $\Upsilon^*$ in a sidereal month. The center $C$ of the second epicycle moves about $B$ in the opposite direction through angle $\alpha$, completing a revolution in an anomalistic month. Since $\bar{\lambda}^*_☾ > \alpha$, the increase of $\alpha$ will be equal to the distance of $B$ from an imaginary apsidal line $O\lambda^*_A$ that moves slowly eastward through angle $(\bar{\lambda}^*_☾ - \alpha)$. The model as described so far is a representation of the first anomaly which depends on the distance of the moon from $\lambda^*_A$.

The second anomaly depends upon the distance of the moon from the mean sun $\bar{\odot}$ which, due to the annual revolution of the earth, moves through angle $\bar{\lambda}^*_\odot$. The mean elongation of the moon from the mean sun, that is, the distance of B from the direction $O\bar{\odot}$, is angle $\eta = \bar{\lambda}^*_☾ - \bar{\lambda}^*$. The period of the mean elongation is a synodic month. The equation of the anomaly of the moon must now be varied such that it is

[1] Ibn ash-Shāṭir's lunar theory is described in Roberts, 1957.

considerably larger at quadrature to the mean sun than at conjunction and opposition. This variation is provided by the second epicycle of radius $r_2$ which carries the moon $M$ about $C$ in the direction opposite to the motion of $C$ about $B$ through angle $2\eta$ which completes a revolution in half a synodic month such that when $\bar{\lambda}^*_{\leftmoon} = \bar{\lambda}^*_{\odot}$ or $\bar{\lambda}^*_{\leftmoon} = \bar{\lambda}^*_{\odot} + 180°$, $2\eta = 0°$ and $M$ lies on $r_1$ closest to $B$. When $\bar{\lambda}^*_{\leftmoon} = \bar{\lambda}^*_{\odot} \pm 90°$, $2\eta = 180°$, and $M$ lies in the direction of $r_1$ farthest from $B$. The distance $MB$ will therefore vary between $r_1 - r_2$ for $\eta = (0°,180°)$, and $r_1 + r_2$ for $\eta = (90°,270°)$, thereby producing the smallest equation of the anomaly at mean conjunction and opposition, and the largest at mean quadratures.

The second epicycle also introduces a correction to the mean anomaly $\alpha$ since the direction $BM$ of the moon with respect to $B$ will differ from the direction of $r_1$ by angle $\varphi$, intended to be equivalent to the equation of center introduced by the prosneusis of the apogee of the epicycle in Ptolemy's model. Since $\varphi$ is governed by $2\eta$, the motion of $M$ about $C$, when $2\eta = (0°,180°)$, $\varphi = 0°$, so $\alpha$ is unaffected at syzygy and quadrature. For $0° \leq 2\eta \leq 180°$, $\varphi \geq 0°$ so that $\alpha$ is increased when $0° \leq \eta \leq 90°$ and $180° \leq \eta \leq 270°$, that is, in the first and third quarters of the synodic month, but when $180° \leq 2\eta \leq 360°$, $\varphi \leq 0°$ so that $\alpha$ is decreased for $90° \leq \eta \leq 180°$ and $270° \leq \eta \leq 360°$ in the second and fourth quarters, all in agreement with Ptolemy's prosneusis. The maximum value of $\varphi$ and the value of $2\eta$ at which it occurs depend upon the relative lengths of $r_1$ and $r_2$. Since $r_1$ and $r_2$ are chosen only with regard to producing the proper maximum equations of the anomaly at syzygy and quadrature, the relation of the value of $\varphi$ at any value of $2\eta$ to Ptolemy's prosneusis is fortuitous.

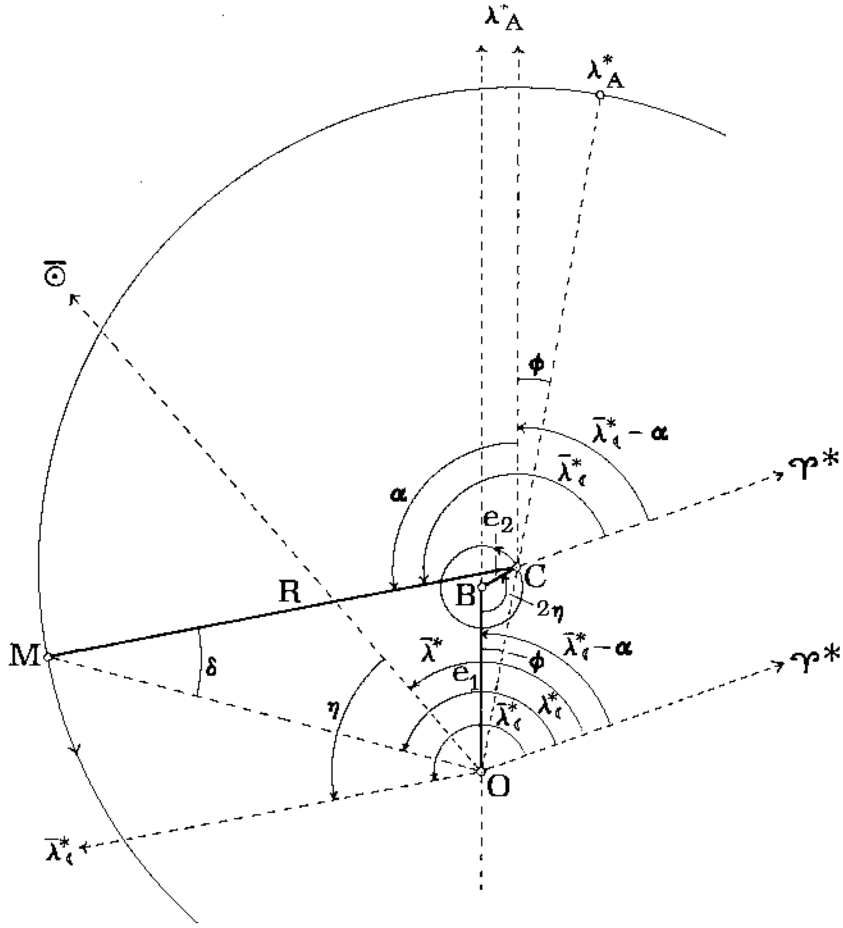


Fig. 6

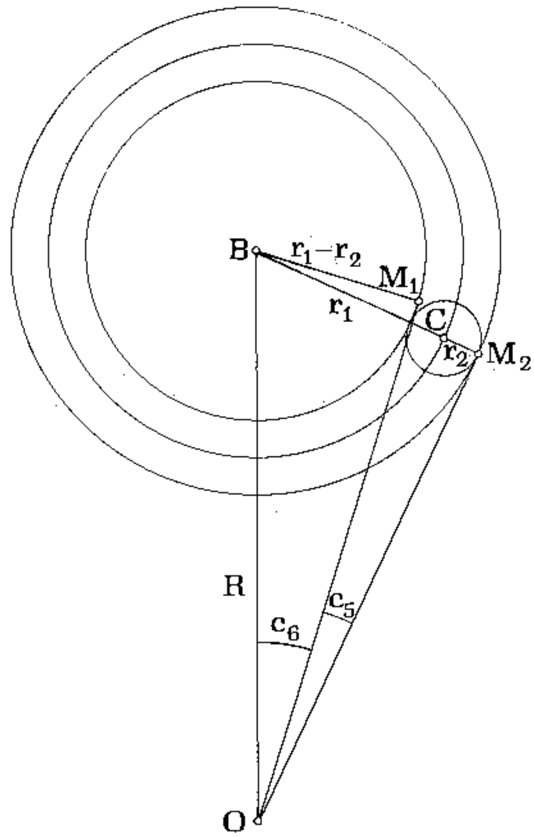


Fig. 7

Before taking up Copernicus's derivation of the parameters, it is useful to examine what is really going on in the model. This can be seen most clearly by converting the double epicycle model to its equivalent eccentric form as in figure 6. With the earth at $O$, a distance $OB = e_1 = r_1$ is allowed to move eastward about $O$ through angle $(\bar{\lambda}^*_{\leftmoon} - \alpha)$, the motion of the mean apsidal line $O\bar{\lambda}^*_A$. A circle of radius $BC = e_2 = r_2$ is described about $B$, and $C$ will move about $B$ through $2\eta$ such that $C$ lies on $OB$ closest to $O$ when $2\eta = 0°$, and in the direction $OB$ farthest from $O$ when $2\eta = 180°$. Finally, the moon moves uniformly about C on a circle of radius CM = R through angle $\bar{\lambda}^*_{\leftmoon}$. The first effect of the model is to vary the eccentricity of the moon's orbit from $e_1 - e_2 = r_1 - r_2$ for $\eta = (0°,180°)$ to $e_1 + e_2 = r_1 + r_2$ for $\eta = (90°,270°)$. The second effect is to vary the direction of $O\lambda^*_A$, the true apsidal line, on either side of the mean apsidal line $O\bar{\lambda}^*_A$ by angle $\varphi$. Since the mean anomaly $\alpha = \bar{\lambda}^*_{\leftmoon} - (\bar{\lambda}^*_{\leftmoon} - \alpha)$ is measured from the direction of $\bar{\lambda}^*_A$, it will be increased or decreased by $\varphi$, just as in the former model. Therefore, Copernicus's lunar model is equivalent to a uniform motion of the moon on a circle eccentric to the earth, having a bimonthly variation in the eccentricity and a corresponding bimonthly variation in the direction of a slowly rotating apsidal line. The principle is thus identical to the fully developed solar model in *De revolutionibus* III, 20, except that the change of eccentricity and irregular shift of the apsidal line in the solar model take place very slowly while in the lunar model they are quite rapid.[2]

Given the mean motions $\bar{\lambda}^*_{\leftmoon}$, $\eta$, and $\alpha$, the true sidereal longitude of the moon $\lambda^*_{\leftmoon}$ is found directly from the model as follows: First find the equation of center $\varphi$ from

$$\tan\varphi = \frac{|r_2 \sin 2\eta|}{r_1 - r_2\cos 2\eta},$$

and then form

$$\alpha' = \alpha \pm \varphi. \quad \begin{array}{l} + \text{ for } 0° \leq 2\eta \leq 180° \\ - \text{ for } 180° \leq 2\eta \leq 360° \end{array}$$

Next compute the effective radius of the epicycle $r = BM$ from

$$r = \sqrt{r_1^2 + r_2^2 - 2r_1r_2\cos 2\eta},$$

and find the equation of the anomaly $\delta$ from

$$\tan\delta = \frac{|r\sin\alpha'|}{R + r\cos\alpha'}.$$

The true sidereal longitude is then

$$\lambda^*_{\leftmoon} = \bar{\lambda}^*_{\leftmoon} \pm \delta. \quad \begin{array}{l} - \text{ for } 0° \leq \alpha' \leq 180° \\ + \text{ for } 180° \leq \alpha' \leq 360° \end{array}$$

[2] Here one must again consider the identity of Copernicus's and az-Zarqāl's solar models (see above, p. 443, n. 2), for it is apparent that the lunar models of Ibn ash-Shāṭir and Copernicus, and the solar models of az-Zarqāl and Copernicus are really all the same model aside from the trivial substitution of a double eccentricity for a double epicycle. In *De rev.* III, 20 Copernicus demonstrates this equivalence for his solar model, but he does not do so for the lunar model.

It is also of interest to note that the combined motions of the two epicycles generate an ellipse about center $B$ (fig. 5) with semimajor axis $r_1 + r_2$ and semiminor axis $r_1 - r_2$. The ellipse turns in the direction opposite to the motion of $B$ about 0 through $\eta$ such that the major axis always lies in the direction of the mean sun while the moon itself moves on the ellipse in the same direction non-uniformly through $\alpha'$. The ellipses generated by the two epicycles in the planetary models, which do not rotate, are discussed below (pp. 470, 491, 501).

The computation of $\delta$ can be done more rapidly by

$$\tan\delta = \frac{|-[r_1\sin\alpha + r_2\sin(2\eta-\alpha)]|}{R + r_1\cos\alpha - r_2\cos(2\eta-\alpha)}.$$

The true sidereal longitude is, as before

$$\lambda^*_{\text{☾}} = \bar{\lambda}^*_{\text{☾}} \pm \delta. \quad \begin{matrix} - \\ + \end{matrix} \text{ for } \begin{matrix} -[r_1\sin\alpha + \\ r_2\sin(2\eta-\alpha)] \end{matrix} \begin{matrix} \leq 0 \\ \geq 0 \end{matrix}$$

The parameters of the lunar model are derived from the maximum equations of the anomaly at syzygy and quadrature in the *Alphonsine Tables* which are respectively 4;56° and 7;36°. The method of derivation is shown in figure 7 in which the center of the earth is $O$, the center of the first epicycle $B$, and the center of the second epicycle $C$. When the moon $M_1$ is at conjunction or opposition, the maximum equation of the anomaly, using $c$ for column numbers from the *Alphonsine Tables*, is

$$c_{6\max} = 4;56°,$$

and when the moon $M_2$ is at quadrature, the maximum equation is

$$c_{6\max} + c_{5\max} = 4;56° + 2;40° = 7;36°.$$

Since the angles at $M_1$ and $M_2$ are right angles, where $R$ is 60 or 100000,

$$BM_1 = r_1 - r_2\sin 4;56° = 5;10 = 8600$$

and

$$BM_2 = r_1 + r_2 = \sin 7;36° = 7;56 = 13226.$$

Therefore,

$$r_1 = \frac{1}{2}(5;10 + 7;56) = 6;33 \approx \frac{1}{10}\cdot 60 + \frac{1}{18}\cdot 10$$
$$= \frac{1}{2}(8600 + 13226) = 10913$$

and

$$r_2 = 6;33 - 5;10 = 1;23$$
$$= 10913 - 8600 = 2313,$$

so that

$$\frac{r_2}{r_1} = \frac{1;23}{6;33} = \frac{1}{4;44} = \frac{2313}{10913} = \frac{1}{4.72}.$$

The text of the *Commentariolus* apparently gives the proportions

$$\frac{r_1}{R} = \frac{1\frac{1}{18}}{10} \quad\text{and}\quad \frac{r_2}{r_1} = \frac{1}{5-\frac{1}{4}}\left[= \frac{1}{4;45} = \frac{4}{19}\right].$$

Thus

$$r_1 = 6;20 = 10555$$

and

$$r_2 = \frac{4}{19}r_1 = 1;20 = 2222,$$

in poor agreement with the values derived above. With these erroneous values the maximum equations, of course, do not fit those given in the *Commentariolus*. Thus, at syzygy

$$\sin\delta_{\max} = r_1 - r_2 = 5;0 = 8333 \qquad \delta_{\max} = 4;47°,$$

and at quadrature

$$\sin\delta_{\max} = r_1 + r_2 = 7;40 = 12777 \quad \delta_{\max} = 7;20°.$$

Since the maximum equations should be 4;56° and 7;36°, there is evidently an error in the radii of the epicycles. The problem lies in the proportion of the larger epicycle to the sphere, which should not be $\frac{1\frac{1}{18}}{10}$, but

$$\frac{1}{10}\cdot 60 + \frac{1}{18}\cdot 10 = 6;33,20$$

or

$$\frac{1}{10}\cdot 1;0 + \frac{1}{18}\cdot 0;10 = \frac{1;0}{10} + \frac{0;10}{18} = 0;6,33,20$$

in excellent agreement with the accurately derived value. The latter form would require emending *unius particulae* to *decem particularum* (Prowe **2**: p. 193:4). But U contains the fraction $\frac{10}{1\frac{1}{18}}$ (*twice*!), so the incorrect number seems confirmed. The notation of this proportion both in the *Commentariolus* and U has given me no end of difficulty. I can only assume that Copernicus derived the radii of the epicycles correctly from the maximum equations, originally wrote the equivalent of $r_1 = \frac{1}{10}\cdot 1;0 + \frac{1}{18}\cdot 0;10$, but then made an error writing the complex fraction in U (unless it *is* written correctly and I cannot read it) that was carried over to the text of the *Commentariolus*. The proportion of the smaller to the larger epicycle $\frac{4}{19}$ is correct since it is confirmed by the accurately derived radii.

The maximum value of the equation of center $\varphi$ follows directly from the radii of the two epi-

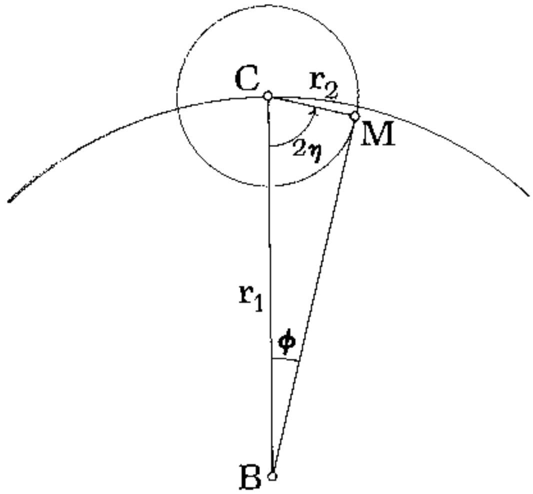


Fig. 8

cycles. In figure 8, $B$ is the center of the larger epicycle of radius $r_1$, $C$ the center of the smaller epicycle of radius $r_2$, and $M$ the moon when the equation of center $\varphi$ is maximum so that the angle at $M$ is a right angle. Now, using the accurate values of $r_1$ and $r_2$ from our derivation,

$$\sin \varphi_{max} = \frac{2313}{10913} = \frac{21195}{100000}, \quad \varphi_{max} = 12;14^\circ$$

in good agreement with the emended text reading $12\frac{1}{4}^\circ$. On the other hand, the rounded sexagesimal values of $r_1$ and $r_2$ lead to $\varphi_{max} = 12;12^\circ$ and Copernicus's proportion gives

$$\sin \varphi_{max} = \frac{4}{19} = \frac{21053}{100000}, \quad \varphi_{max} = 12;9^\circ$$

which would lead to an emendation of the text to $12\frac{1}{6}^\circ$. This would not be unreasonable. Note, however, that the value $12\frac{1}{4}^\circ$, since it is derived from the accurate values of $r_1$ and $r_2$, indicates that Copernicus did in fact derive them carefully, and thus $\frac{1\frac{1}{18}}{10}$ instead of $\frac{1}{10} + \frac{0;10}{18}$ in the expression of $r_1$, would be confirmed as only an error in Copernicus's notation.

On the title page of Copernicus's copy of the *Alphonsine Tables*, the following note is written (L. Birkenmajer, plate facing p. 28):

> epicyclus ☾ [lunae] a ad b proportio
> ut 4.47 ad unum / $\frac{44}{9}$

These lead to two further values of $\varphi_{max}$.

1. $\sin \varphi_{max} = \frac{1}{4;47} = \frac{12;32,37}{60} = \frac{20905}{100000}$
   $\varphi_{max} = 12;4^\circ$

2. $\sin \varphi_{max} = \frac{9}{44} = \frac{12;16,22}{60} = \frac{20455}{100000}$
   $\varphi_{max} = 11;48^\circ$

Then, in the lunar correction tables in this copy of the *Alphonsine Tables* Copernicus wrote an additional column corresponding to $c_3$, that is, the equation of center $\varphi$ (L. Birkenmajer, pp. 47–48). In this column $\varphi_{max} = 12;6^\circ$, so that

3. $\sin \varphi_{max} = \sin 12;6^\circ = \frac{12;34,38}{60} = \frac{20962}{100000}$.

Note that $\frac{12;34,38}{60} = \frac{1}{4;46}$, so 1 and 3 probably represent slightly different roundings of the same proportion between the epicyclic radii, while 2 is unrelated to these. Then note that

$$\frac{1}{4;46} = \frac{1}{5 - 0;14}, \quad \frac{1}{4;47} = \frac{1}{5 - 0;13},$$

so 1 and 3 are also very close to the ratio of $\frac{1}{5 - \frac{1}{4}}$ in the *Commentariolus*. I do not know why Copernicus used so many nearly identical expressions of this ratio leading to various values of $\varphi_{max}$.

The value of $2\eta$ at which $\varphi_{max}$ occurs is $90^\circ - \varphi_{max}$. Thus, where $\varphi_{max} = 12;14^\circ$, it occurs at $2\eta = 77;46^\circ$, and where $\varphi_{max} = 12;6^\circ$, it occurs at $2\eta = 77;54^\circ$ so that respectively, $\eta = 38;53^\circ$ or $38;57^\circ$. This differs considerably from the equation of center in the *Alphonsine Tables* and the *Almagest*, where $\varphi_{max} = 13;9^\circ$ for $\eta \approx 57^\circ$, but, as I have mentioned, the equation of center produced by the two epicycles only crudely and accidently approximates Ptolemy's prosneusis. The differences can amount to as much as about $4\frac{1}{2}^\circ$ in the equation of center, and this can cause differences of slightly over $0;25^\circ$ in the equation of the anomaly. Since, however, these differences cannot occur at conjunction or opposition where an eclipse would make them very noticeable, Copernicus, who only vaguely understood the prosneusis, could feel safe in ignoring them.

Finally, it is of interest to note that Copernicus's maximum equation at syzygy, $4;56^\circ$, which he took from the *Alphonsine Tables* and still retained in *De revolutionibus*, has an extensive earlier history. Ibn ash-Shāṭir used this same value. It occurred prior to the *Alphonsine Tables* in al-Khwārizmī's tables (Suter); $c_6$ in the *Alphonsine Tables* is a recomputation of Ptolemy's equation of the anomaly using this borrowed parameter. Khwārizmī's tables are to a great extent based upon Indian sources, and indeed, in the *Ārdharātrika* system found in Lātadeva's version of the *Sūryasiddhānta* (preserved in the *Pañchasiddhāntika* of Varāhamihira) and in Brahmagupta's *Khaṇḍakhādyaka* the maximum *manda* equation of the moon is 4;56° (Pingree). Copernicus would no doubt be surprised to discover that one of the fundamental parameters of his lunar theory was employed in India some thousand years earlier.

### OBJECTIONS TO PTOLEMY'S LUNAR THEORY

Those who hold the opinion that this is brought about by means of an eccentric circle, in addition to the improper non-uniform motion in the circle itself, have fallen into two obvious errors. For it follows by a mathematical proportion that when the moon in quadratures rests at the lowest part of the epicycle, it should appear about four times larger (if only the whole moon were illuminated) than the new and full moon, unless in addition an increase and decrease in the size of its body is rashly maintained.[a] In the same way it also causes the parallax, owing to the perceptible size of the earth compared to the distance of the moon, to increase greatly near quadratures. If, however, one investigates more carefully, he will find that both [the apparent size and parallax] differ only very slightly in quadratures from what they amount to at new and full moon, and consequently he will not easily doubt our more reasonable theory.

[a] 194:2 et temerarie *asseritur*. (S asserit)

Copernicus criticizes Ptolemy's model on the grounds that it does not preserve uniform circular motion and that it produces a severe variation in the distance of the moon in no way corresponding to the observed variation of the apparent lunar diameter and parallax. The model is shown in figure 9, in which longitudes are measured tropically, although they could just as well be measured sidereally since Ptolemy's precession is uniform. Measured in the order of the signs from the direction to the vernal equinox ♈, the mean sun moves through $\bar{\lambda}_\odot$ and the center $C$ of the moon's epicycle through $\bar{\lambda}_☾$, their difference being the mean elongation $\eta$. These motions are all uniform with respect to the center of the earth $O$. The center $C$ of the epicycle moves on an eccentric circle with apogee $A$ whose center $B$ moves in the direction opposite to the motion of $C$ through $\eta$ on a circle of radius $e$. Letting $OA = R$, the radius $BC = R - e$. The moon $M$ moves on the epicycle of radius $r$ in the direction opposite to the motion of $C$ through the mean anomaly $\alpha$, measured from the mean apogee of the epicycle $H$ which has a prosneusis, a direction, through $C$ to a point $N$ which moves on circle $OB$ such that arc $BN = 180°$. The effects of the model are as follows:

1. The motion on the epicycle accounts for the first anomaly, and is equivalent to motion on an eccentric with an eccentricity equal to $r$ and an apsidal line rotating about $O$ through $\bar{\lambda}_☾ - \alpha$.

2. The motion of $B$ about $O$ varies the distance $OC$ such that when $\eta = (0°, 180°)$, at mean conjunction and opposition, $OC = (R - e) + e = R$, and when $\eta = (90°, 270°)$, at mean quadrature, $OC = (R - e) - e = R - 2e$. This will optically vary the radius of the epicycle $r$ as seen from $O$, thereby varying the equation of the anomaly $\delta$.

3. The prosneusis of the mean apogee of the epicycle toward $N$ introduces an equation of center $\varphi$, the difference in the directions $NCH$ to the mean apogee and $OCT$ to the true apogee as

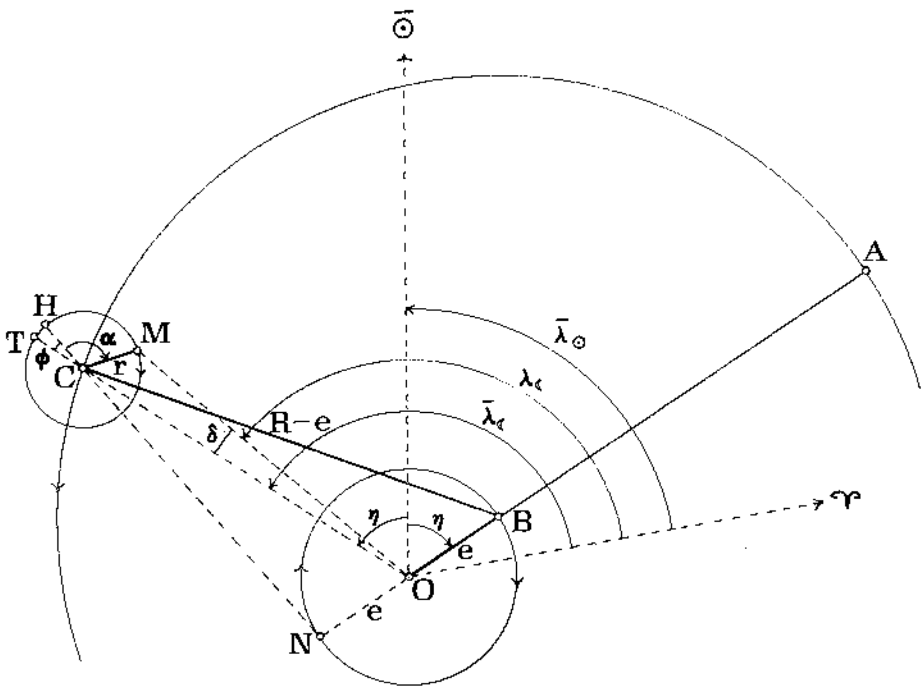


Fig. 9

seen from $O$. The magnitude of $\varphi$ is determined by the eccentricity $ON = e$ and the distance $OC$.

Copernicus's objections to the model are:

1. The motion of $C$ is uniform with respect to $O$ while it moves on a circle with center $B$. The motion of $M$ is uniform with respect to $HCN$ rather than $TCO$. These are violations of uniform circular motion.

2. The distance $OM$ has a greatly exaggerated bimonthly variation completely belied by the observed variation in the lunar diameter and parallax. According to Ptolemy's parameters, where $R = 60$, $e = 10;19$ and $r = 5;15$. Therefore, at syzygy the greatest distance is

$$R + r = 65;15$$

and the least distance

$$R - r = 54;45,$$

but at quadrature, the greatest distance is

$$R - 2e + r = 44;37$$

and the least distance

$$R - 2e - r = 34;7.$$

The maximum possible variation in distance is thus

$$\frac{R + r}{R - 2e - r} = \frac{65;15}{34;7} \approx \frac{2}{1}.$$

This will produce a variation of about 2 to 1 in the apparent diameter of the moon or about 4 to 1 in the apparent area of the lunar disc. Hence Copernicus's statement that if the entire moon were illuminated at quadrature when at the perigee of the epicycle, it would appear four times larger than at new or full moon. This observation is also made in *Epitome* V, 22 following an examination of the variation in the apparent diameter of the moon using al-Battānī's greatest and least diameters at syzygy as the basis of the examination. The comment in the *Epitome* is:

> But it is remarkable that when the moon is in quadrature in the perigee of the epicycle, it does not appear so large [i.e., 0;56,22° in diameter] since if the entire moon were illuminated it should appear four times the size it appears in opposition when it is at the apogee of the epicycle.

Copernicus paraphrases this statement, and adds the remark that the only way of compensating for this large variation would be to maintain, improbably, that the body of the moon itself undergoes a bimonthly variation in its true size.

The true geocentric distances corresponding to the greatest and least relative distances of the moon are given in *Epitome* V, 22 as 64;10 and 33;30 terrestrial radii, the latter being a rounding of Ptolemy's 33;33 while the former is exactly Ptolemy's value (*Almagest* V, 17; Manitius 1, 316). The corresponding horizontal parallaxes are, from *Epitome* V, 24, for greatest distance 0;53,34° and for least distance 1;43°. Copernicus asserts that the parallax increases considerably near quadrature. This is obvious. Possibly he also means that it is too large at quadrature. This would be confirmed by an observation of an occultation in which the parallax of the moon given by Ptolemy's parallax table is too large to produce the observed occultation. Indeed, in the 1497 observation of the occultation of Aldebaran with the moon at 74° of mean elongation cited in *De revolutionibus* IV, 27, using Copernicus's mean motions, Ptolemy's table gives a lunar parallax in the circle of altitude of 1;32° while Copernicus computes 1;0°. Using Copernicus's angle of intersection of the ecliptic and circle of altitude, 29°, from Ptolemy's parallax of 1;32°, the parallax in longitude is 1;24° and the parallax in latitude 0;44°, compared with Copernicus's computation of 0;51° in longitude (which is incorrect) and 0;30° in latitude. Neglecting in this context that there are severe problems in Copernicus's computation of the parallax in *De rev.* IV, 27, it is apparent that Ptolemy's parallax at 74° of mean elongation is considerably larger than the parallax that shows the moon to occult Aldebaran as observed.[3] Of course, as Copernicus's lunar theory is presented in the *Commentariolus*, it is wholly inadequate to determine parallax since no lunar distance is given. It is possible, however, that with the aid of the treatment of parallax in Book V of the *Epitome*, he could have computed Ptolemy's parallax for this and other observed occultations, and found it consistently too large when the moon was near quadrature.

The variation in relative lunar distance in Copernicus's model is considerably less than in Ptolemy's, and gives the moon a greater variation of distance at quadrature than at syzygy. Using the notation of figure 7 and the sexagesimal values of the radii computed accurately from the maximum equations, the greatest and least distance at syzygy are

$$R \pm (r_1 - r_2) = 60 \pm 5;10$$

and at quadrature

$$R \pm (r_1 + r_2) = 60 \pm 7;56,$$

so the maximum variation, at quadrature, is only about 17 to 13.

[3] Some of the problems in this observation are discussed in Neugebauer ($1968_1$: p. 100) where it is shown that only through a combination of errors both in the computation of the lunar parallax and in the latitude of Aldebaran is Copernicus able to demonstrate that his lunar theory will in fact reproduce the occultation.

MOTION IN LATITUDE

With these three motions in longitude, the moon travels through the points of [its] motion in latitude.[a] (*sic*) The axes of the epicycles are parallel to the axis of the sphere on account of which [the moon] does not depart from [the plane of the deferent]. But the sphere keeps its axis inclined to the axis of the great sphere or ecliptic, and therefore causes the moon to depart from the plane of the ecliptic. It is inclined at an angle which subtends 5° of the circumference of a circle. The poles of the sphere are carried about [centers lying on a line] parallel to the axis[b] of the ecliptic, in almost the same way explained about the inclination, but in this case opposite to the order of the signs and with a motion so much slower that it completes one revolution only in the nineteenth year. It is believed by most [astronomers] that this [motion] takes place in a higher sphere to which the poles [are] attached [and] are carried around in the direction of this motion.[c]

And so the moon seems to have such a structure of motions.

[a] 194:8 circumit. . . . puncta latitudinis motus?
[b] 194:15 *axi* eclipticae (SV axis)
[c] 194:19 ad hunc *motum* (SV modum)

The first sentence of the section makes little sense to me, and I suspect textual corruption, possibly the loss of a full line between *circumit* and *puncta*. It is also possible that the meaning could be "With these three motions the moon travels about in longitude," as the end of the section on longitude, and the last three words, *puncta latitudinis motus*, are the end of the first sentence on the theory of latitude, the beginning of the sentence having fallen out of the text.

Copernicus's representation of the lunar latitude is simple enough, yet his description does have its peculiarities. The plane of the lunar orbit is inclined to the plane of the ecliptic at an angle of 5°, and the nodes regress slowly through the ecliptic, completing a revolution in a little under 19 years. Copernicus seems to be describing the simple model shown in figure 10. Since neither the anomaly nor elongation introduces any variation into the lunar latitude, the two epicycles lie in the plane of the deferent. The axis of the deferent $F^*$ is inclined 5° to the axis of the ecliptic $E^*$, and $F^*$ moves in the direction opposite to the order of the signs through angle $\nu$, describing a circle about $E^*$ in a little under 19 years. $F^*$ governs the motion of a sphere concentric to the earth at $O$, causing the ascending node ☊ and descending node ☋ of the plane of the deferent, which is inclined 5° to the plane of the ecliptic, to regress along the ecliptic. Meanwhile the moon ☾ moves in the order of the signs in the plane of the deferent.

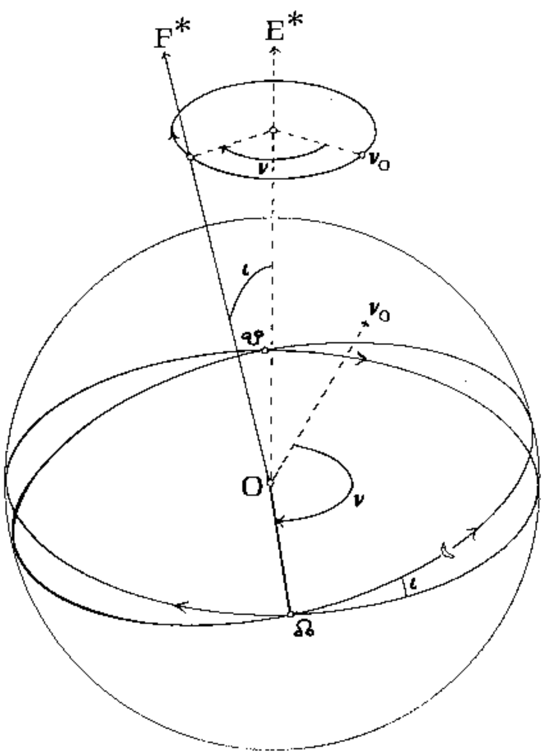

Fig. 10

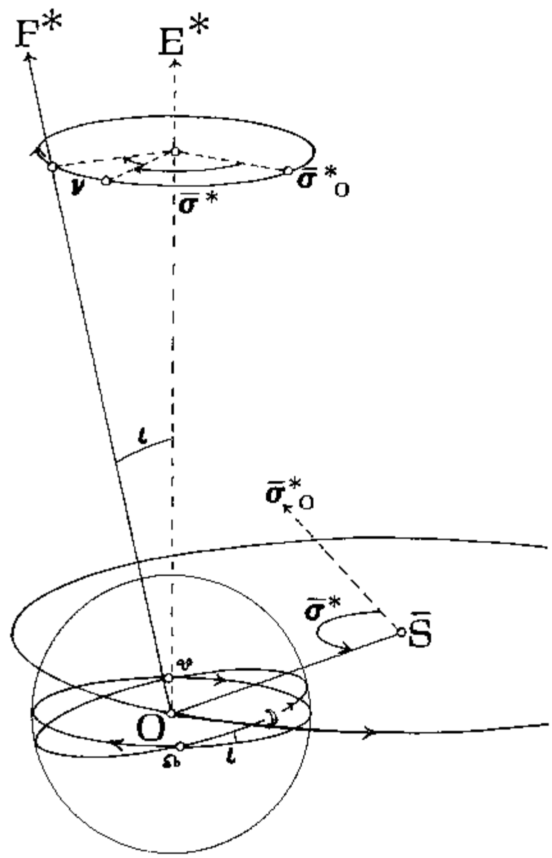

Fig. 11

The odd thing about Copernicus's description is his statement that, in contrast to the motion of the inclination, $F^*$ moves opposite to the order of the signs and with a slower motion than the motion of the inclination. The specification of direction implies, as we have mentioned earlier, that Copernicus thinks the motion of the inclination takes place in the order of the signs, which would be incorrect. We ignored this possible error when describing the model for the motion of the inclination. The assertion that the motion of the axis of the lunar orbit is slower than the motion of the inclination shows, among other things, that at this point Copernicus has forgotten that the earth is also revolving around the sun. The correct model is shown in figure 11. The earth $O$ moves around the mean sun $\bar{S}$ through angle $\bar{\sigma}^*$ of the annual sidereal motion. The axis of the lunar orbit $F^*$ must move in the opposite direction through angle $(\bar{\sigma}^* + \nu)$, completing a revolution in somewhat under one year $(\sim 345\frac{1}{2}^d)$; at the end of the year $F^*$ and the lunar nodes will have regressed by $\nu^{\circ/y}$ $(\sim 19;20^\circ)$. Since the period of the precession of the equinoxes takes thousands of years while the regression of the lunar nodes takes only about 19 years,

$$\bar{\sigma}^* + \nu > \bar{\sigma}^* + \pi;$$

therefore, $\nu > \pi$ and the motion of the axis of the lunar orbit is much *faster*, not slower, than the motion of the inclination. It makes no difference whether the motion of the inclination and of the lunar nodes are viewed geocentrically or heliocentrically since either way $\nu > \pi$ and the motion of the axis of the lunar sphere must be faster than the motion of the axis of the daily rotation. All of this makes me believe that when Copernicus wrote the *Commentariolus* he really did not know that the motion of the inclination must be opposite to the order of the signs. He did not understand his own precession model, which would not even work as he describes it, and he was confused about the model for the regression of the lunar nodes.

## 6. THE THREE SUPERIOR PLANETS—SATURN, JUPITER, AND MARS

### THE FIRST ANOMALY

Saturn, Jupiter, and Mars have the same model for their motions. Indeed, their spheres, completely surrounding the great [sphere of] the annual [motion], revolve in the order of the signs about the center of the great sphere itself which is their common center. But the sphere of Saturn completes a revolution in the 30th year, the sphere of Jupiter in the 12th, and the sphere of Mars in the 23rd[a] month, just as if the size of the spheres slowed down these revolutions. For where the semidiameter of the great sphere is given 25 parts, the semidiameter of the sphere of Mars will receive 38[b] parts, that of Jupiter $130\frac{5}{12}$ parts, and that of Saturn $230\frac{5}{6}$ parts[c] I call the *semidiameter* the distance from the center of the sphere to the center of the first epicycle.

Now each sphere has two epicycles, one of which carries the other in almost the same way as was explained about the moon, but by a different rule. For the first epicycle performs revolutions equal but in the direction opposite to the motion of the sphere, while the second epicycle, moving in the direction opposite to the first, carries the planet through two revolutions so arranged that whenever the second epicycle is at greatest or least distance from the center of the sphere, the planet is then closest to the center of the [first] epicycle, but when the [second epicycle] is in quadrants halfway between, then the planet is farthest [from the center of the first epicycle]. Now, from the combination of these motions of the sphere and the epicycles, and from the commensurability of [their] revolutions, the result is that the farthest distances and closest approaches[d] of this kind retain positions fixed with respect to the sphere of the fixed stars. And [the planets] continuously observe relations in [these] motions that are everywhere fixed [with respect to the sphere of the fixed stars] so that they keep their apsides immovable—Saturn near the star said to be "on the elbow" of Sagittarius, Jupiter 8° east of the star called "the end of the tail" of Leo, and Mars $6\frac{1}{2}$° west of "the heart" of Leo.

The sizes of the epicycles are as follows: In the case of Saturn the semidiameter of the first is 19;41 parts where the semidiameter of the great sphere was assumed to be 25 parts, and the second epicycle has a semidiameter of 6;34 parts. So also in the case of Jupiter the first has a semidiameter of 10;6 parts, and the second of 3;22 parts. In the case of Mars the first is of 5;34 parts, and the second of [1];51 parts. Thus in each case the semidiameter of the first is three times the length of the second. It is decided to call the anomaly which the motion of the epicycles produces upon the motion of the sphere the *first anomaly*, which, as has been mentioned, observes[e] passages that are everywhere fixed with respect to the sphere of the fixed stars.

[a] 195:1 vigesimo *tertio* (SV vigesimo nono) See above p. 441.
[b] 195:4 *38* partes (SV 30)
[c] 195:5 *230* et *dextantem* unius (SV 236 et sextantem)
[d] 195:16 elongationes et accessiones *maximae* (S maximo V maxime)
[e] 196:10 *observat* limites (SV observant)

In the first part of this section Copernicus hints at the curious notion that the planetary periods become longer with increasing distance from the center of the great sphere "as if the size of the spheres slowed down (*remoretur*) these revolutions." This could indicate that after finding that the heliocentric theory gave the order and distances of the planets with certainty, Copernicus attempted to find some precise correspondence between distance and period, but failing to accomplish this, fell back on the possibility that the sheer massiveness of the spheres retarded the planetary motions. There is no mention of this theory in *De revolutionibus*, and Copernicus obviously does not take it very seriously here. Nevertheless, the very mention of the possibility that the spheres have some kind of mass is further evidence that Copernicus had no doubt that the motions of the planets were controlled by solid spheres. In fact, if one grants that Copernicus believed that the heliocentric theory was the true representation of the motions of the planets—and there is no reason not to—one must also grant that he believed each sphere and each motion in his planetary theory to be necessary, down to the complicated mechanisms for the motion of Mercury and the latitudes of Mercury and Venus. Even though he offers some alternative but equivalent models in the *Commentariolus* and *De revolutionibus*, he does not doubt that one of them must be the combination of material spheres responsible for the apparent planetary motions.

Another remark that is of interest in this connection is Copernicus's specification that "I call the semidiameter [of the sphere] the distance from the center of the sphere to the center of the first epicycle." If he were talking only about circles and epicycles, this distinction would be unnecessary since the center of the epicycle is obviously located at the circumference of the circle on which it moves. With solid spheres, however, the radius of the solid sphere itself must extend to the outer edge of the epicycle, or better, to the outer surface of the epicyclic sphere. The arrangement is shown in figure 12. The radius of the solid sphere is $\bar{S}T$, and the epicyclic sphere with center $F$ is tangent to the larger sphere at $T$ and presumably rotates on an axis attached to the solid sphere at $M$ and $N$. Hence Copernicus must specify that he is calling $\bar{S}F$, not $\bar{S}T$, the semidiameter of the sphere. The second epicyclic sphere is entirely contained within the first in the same way. The intersection of spheres is not permitted.

It was precisely this meticulous and, from our point of view, archaic concern for the representation of the motions of the planets by the rotation of spheres that led Copernicus, like Ibn ash-Shāṭir before him, to investigate alternatives to Ptolemy's planetary models. At the beginning of the *Commentariolus* Copernicus explains that this was indeed the original motivation of his researches in planetary theory. In the course of laboring over this "exceedingly difficult and nearly insoluble problem" he found something vastly more important than the strict representation of planetary motions by rotating spheres, but the motion of the earth and planets about the sun was not what he was originally looking for. His original concern was the first, not the second, anomaly because it was in the representation of the first anomaly that Ptolemy's model violated the uniform and circular motion permitted to the rotation of a sphere.

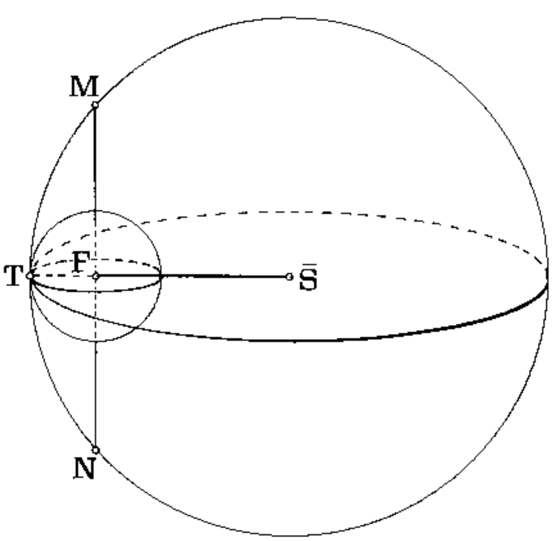


Fig. 12

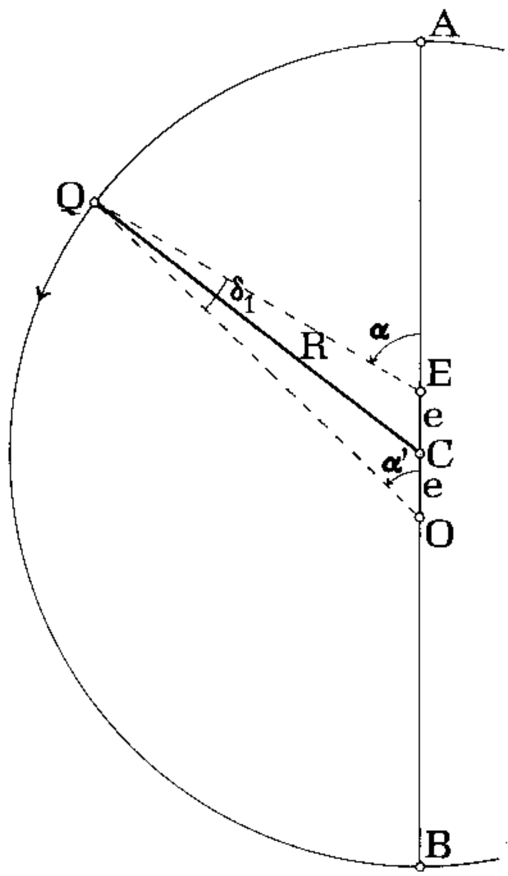


Fig. 13

Both Ibn ash-Shāṭir and Copernicus had the same objection to Ptolemy's representation of the first anomaly—the violation of uniform circular motion resulting from the bisection of the eccentricity. Ptolemy's model is shown in figure 13. We let the sidereally fixed apsidal line be $AB$, and taking a point $C$ describe a circle of radius $R$ on which moves the center of the epicycle $Q$. Placing the earth at $O$, removed from $C$ by the eccentricity $e$, $A$ will be the apogee and $B$ the perigee. We then take a point $E$ located in the direction of $A$ at the distance $2e$ from $O$, and say that $Q$ always moves uniformly with respect to $E$ while maintaining a constant distance from $C$.

We know from Ptolemy's account in *Almagest* IX, 2 that earlier astronomers had developed a model in which $Q$ both moved uniformly and maintained a fixed distance with respect to $E$, and that Hipparchus showed that this model was inadequate, but proposed no alternative.

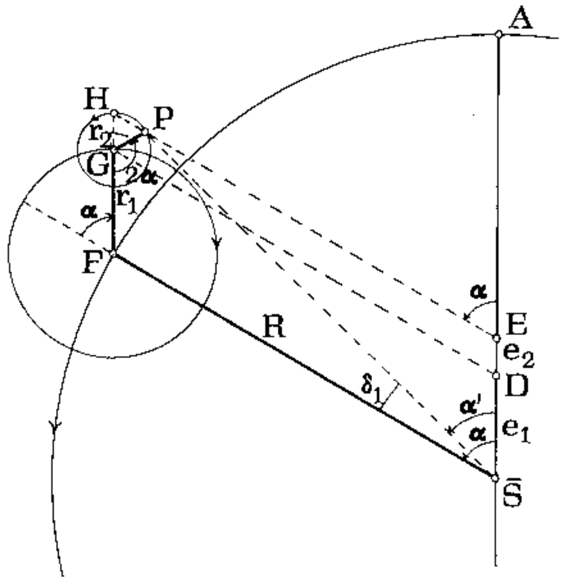


Fig. 14

Ptolemy found that the motion of the planet is better represented if $OE$ be bisected at $C$ so that $Q$ would remain at a constant distance from $C$ while still moving uniformly with respect to $E$. This bisection of the eccentricity, for which Ptolemy presented no analysis, was the best representation of the first anomaly before Kepler. Although it seems simple, the bisection must have been the result of an enormous amount of painstaking analysis of planetary motion. Kepler remarks on this in *Astronomia nova* xix.[1]

It was the bisected eccentricity that Ibn ash-Shāṭir and Copernicus found objectionable. They both considered $Q$ to move on a sphere with its diameter passing through $C$. Since a sphere can rotate uniformly only about its diameter, it seemed impossible for $Q$ to move uniformly with respect to $E$.[2] The problem was therefore to find some other way of representing the motion of $Q$ such that (1) uniform circular motion is preserved, and (2) the equation of center $\delta_1$ is identical or as close as possible to the equation resulting from the bisected eccentricity. Now, where $\alpha$ is the distance of $Q$ from $A$, the equation of center is given by

$$\tan \delta_1 = \frac{|2e \sin \alpha|}{\sqrt{R^2 - e^2 \sin^2 \alpha} + e \cos \alpha},$$

and the distance of $Q$ from $A$ as seen from $O$, that is, $\alpha'$ by

$$\alpha' = \alpha \pm \delta_1. \quad \begin{matrix} - \text{ for } 0° \leq \alpha \leq 180° \\ + \text{ for } 180° \leq \alpha \leq 360° \end{matrix}$$

The model used by Ibn ash-Shāṭir and Copernicus, shown in figure 14, fulfills both requirements of preserving uniform circular motion and giving an equation of center nearly indistinguishable from Ptolemy's.[3] A sidereally fixed direction $\bar{S}A$ is taken, and we let $\bar{S}$ be the earth in Ibn ash-Shāṭir's model, but the mean sun in Copernicus's model. With radius $R$ we describe a circle on which the center $F$ of the larger epicycle of radius $r_1$ moves uniformly through angle $\alpha$ measured from the apsidal direction $\bar{S}A$. The center $G$ of the smaller epicycle is carried by $r_1$ in the direction opposite to the motion of $F$ through angle $\alpha$; $r_1$ will therefore remain parallel to the direction $\bar{S}A$. By the simple equivalence demonstrated in *Epitome* III, 9 (= *Almagest* III, 3; Manitius 1, 160–61), $G$ will move uniformly on an eccentric circle about center $D$, located on the apsidal line in the direction of $A$ at the distance $e_1 = r_1$ from $\bar{S}$. In *De revolutionibus* Copernicus replaces the larger epicycle by this eccentricity $e_1$. The planet $P$ is carried by radius $r_2$ of the smaller epicycle through $2\alpha$ such that when $\alpha = 0°$, $P$ lies on $r_1$ between $G$ and $F$. Since all motions depend upon the distance $\alpha$ from the sidereally fixed apsidal line, the first anomaly and its corrections are sidereally fixed.

Next extend $FG$ to meet the smaller epicycle at $H$, and draw $HE$ parallel to $F\bar{S}$ and $GD$, meet-

[1] Kepler, *Werke* 3: pp. 174, 177.

[2] Kepler gives this analysis of Copernicus's motivation in *Astronomia nova* vi (*Werke* 3: p. 97), and also points out that since Tycho Brahe had disproved the "solidity of the spheres," the objection was no longer of consequence, and further, that the way was now open to examine models with a divided eccentricity measured from the true sun.

[3] Ibn ash-Shāṭir's planetary theory in longitude is explained in Kennedy and Roberts, 1959, and in Kennedy, 1966. The method of computing and tabulating corrections used by Ibn ash-Shāṭir and by Copernicus in *De rev.* V, 33–34 are also identical (Abbud, 1962), in that both compute equations of the anomaly for greatest distance, and provide a correction for least distance and an interpolation factor for arbitrary values of the eccentric anomaly. Ptolemy, on the other hand, computes the equations for mean distance, and provides corrections for greatest and least distances. The differences in the tabulated equations of Ibn ash-Shāṭir and Copernicus are the result of their different parameters, and in the case of Mercury, of their choosing different norms for the factor of interpolation. Note that all of the following analysis of the first anomaly applies as well to Ibn ash-Shāṭir as to Copernicus.

ing the apsidal line at $E$. Since

$$GH = GP = r_2$$

therefore

$$GPH = GHP = AEH = \alpha,$$

and $P$ must lie on $EH$, so that $P$ moves uniformly with respect to $E$. $E$ may be called the equant point. Further, since

$$ADG = DGP = \alpha$$

and

$$DEP = GPE = 180° - \alpha,$$

thus

$$DE = e_2 = r_2$$

and

$$\bar{S}E = e_1 + e_2 = r_1 + r_2.$$

We compare this model with Ptolemy's by superimposing both models in figure 15. Provided that

$$r_1 + r_2 = 2e,$$

$P$ will move uniformly with respect to a point $E$ located at the distance $2e$ from $\bar{S}$, exactly as in Ptolemy's model. The values of $r_1$ and $r_2$ must next be chosen such that $P$ will be as close as possible to $Q$. Now

$$EP = R - 2r_2 \cos\alpha$$

and

$$EQ = \sqrt{R^2 - e^2 \sin^2\alpha} - e\cos\alpha.$$

$P$ will therefore be closest to $Q$ if

$$r_2 = \tfrac{1}{2}e,$$

for then

$$PQ = EP - EQ \approx \frac{e^2 \sin^2\alpha}{2R}.$$

In the case of the planet with the largest eccentricity, Mars, $e = 0.1$, so the greatest possible distance between $P$ and $Q$ is about .005 which will not noticeably affect the equation of center. Then since

$$r_2 = \tfrac{1}{2}e \quad \text{and} \quad r_1 + r_2 = 2e,$$

necessarily

$$r_1 = \tfrac{3}{2}e,$$

and the optimum epicyclic radii to reproduce the equation of center of Ptolemy's bisected eccentricity have been found. If, on the other hand, the eccentricity is divided in a different ratio, then where $OC = e'$ and $CE = e''$, it is required that

$$r_2 = \tfrac{1}{2}e''$$

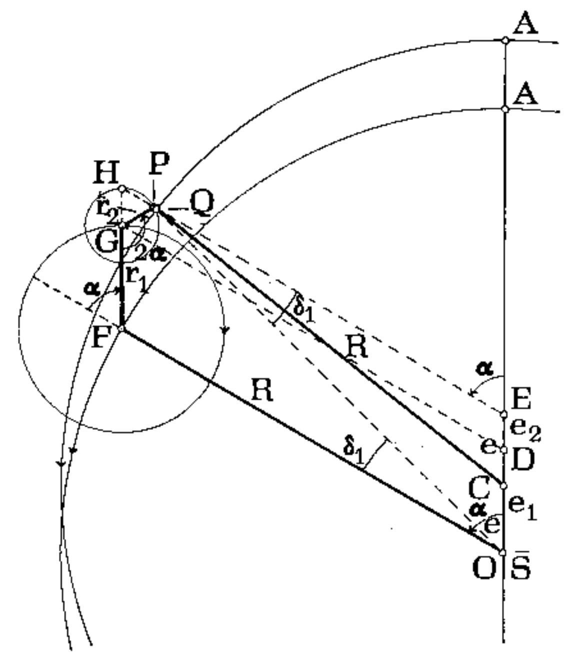


Fig. 15

and

$$r = e' + \tfrac{1}{2}e''.$$

Neither in the *Commentariolus,* where admittedly it would not be expected, nor in *De revolutionibus*, where it would, does Copernicus present any analysis of this sort. One may seriously wonder whether he understood the fundamental properties of his model for the first anomaly, and this of course bears strongly on the important question of whether the model was his own invention or something he learned of from a still undiscovered transmission to the west of a description of Ibn ash-Shāṭir's planetary theory. My own inclination is to suspect the latter, not because I think Copernicus incapable of carrying out such an analysis of the first anomaly in Ptolemy's model (he certainly shows considerable ingenuity in deriving the heliocentric representation of the second anomaly), but rather because the identity with the earlier planetary theory of Copernicus's models for the moon *and* the first anomaly of the planets *and* the variation of the radius of Mercury's orbit *and* the generation of rectilinear motion by two circular motions seems too remarkable a series of coincidences to admit the possibility of independent discovery.

The equation of center in Copernicus's model is simpler to compute than in Ptolemy's, al-

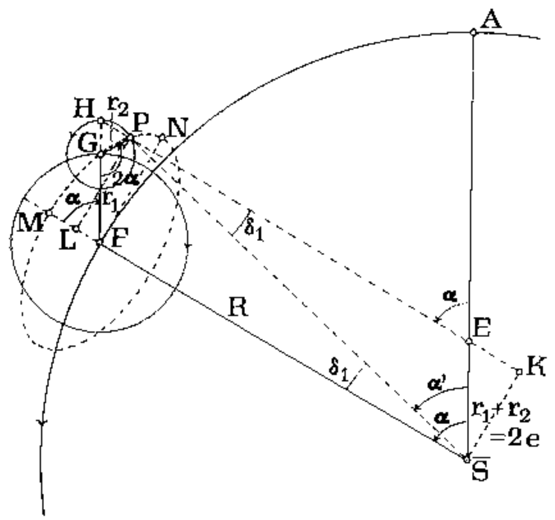


Fig. 16

though Copernicus was not aware of this and in *De revolutionibus* used an unnecessarily cumbersome method. In figure 16 the equation of center is

$$\delta_1 = F\bar{S}P = EP\bar{S}.$$

Now

$$PK = R - 2r_2 \cos\alpha + (r_1 + r_2)\cos\alpha = R + (r_1 - r_2)\cos\alpha,$$

and

$$\bar{S}K = (r_1 + r_2)\sin\alpha.$$

Therefore

$$\tan\delta_1 = \frac{|(r_1 + r_2)\sin\alpha|}{R + (r_1 - r_2)\cos\alpha}.$$

If now

$$r_1 + r_2 = 2e \quad \text{and} \quad r_1 - r_2 = e,$$

the equation becomes

$$\tan\delta_1 = \frac{|2e\sin\alpha|}{R + e\cos\alpha}.$$

The lower term exceeds the lower term in the equation of the bisected eccentricity only by the small amount $PQ$ (fig. 15), that is,

$$\frac{e^2\sin^2\alpha}{2R},$$

which even in the case of Mars cannot produce a difference of 0;2°. As in Ptolemy's model

$$\alpha' = \alpha \pm \delta_1. \quad \begin{matrix} - \text{ for } 0^\circ \leq \alpha \leq 180^\circ \\ + \text{ for } 180^\circ \leq \alpha \leq 360^\circ \end{matrix}$$

An interesting property of the model, also shown in figure 16, is that the motion of $P$ with respect to $F$ generates an ellipse. Now,

$$LP = (r_1 + r_2)\sin\alpha,$$

and

$$FL = (r_1 - r_2)\cos\alpha.$$

Then since

$$\frac{LP}{FG + GP} = \frac{(r_1 + r_2)\sin\alpha}{r_1 + r_2} = \sin\alpha$$

and

$$\frac{FL}{FG - GP} = \frac{(r_1 - r_2)\cos\alpha}{r_1 - r_2} = \cos\alpha,$$

it follows that

$$\frac{LP^2}{(FG + GP)^2} + \frac{FL^2}{(FG - GP)^2} = \sin^2\alpha + \cos^2\alpha = 1,$$

and $P$ will describe an ellipse about $F$ with semimajor axis

$$FN = r_1 + r_2 = 2e$$

and semiminor axis

$$FM = r_1 - r_2 = e.$$

It is also immediately evident that the equation of center is given by

$$\tan\delta_1 = \frac{LP}{\bar{S}F + FL} = \frac{|(r_1 + r_2)\sin\alpha|}{R + (r_1 - r_2)\cos\alpha}.$$

This ellipse is actually used by François Viète for the model of the first anomaly in his unfinished and unpublished astronomical treatise *Ad Harmonicon Coeleste.* Viète was certainly unacquainted with the *Commentariolus*, and developed his planetary theory from the *Almagest*, *De revolutionibus*, and the *Prutenic Tables*, although he considered Ptolemy, Copernicus, and Reinhold to be inferior mathematicians. His understanding of the geometry underlying planetary theory is far more sophisticated than that of Copernicus, and I owe a good deal of my own analysis of Copernicus to a study of Viète. Although Viète never specifically examines Copernicus's theory, his own models make very clear the properties of Copernicus's models and their relation to Ptolemy's.[4] The most ingenious part of his treatise is the demonstration of a direct solution to Ptolemy's problem of finding the eccentricity and direction of the apsidal line from three observed oppositions. Viète shows that in a model producing Copernicus's equation of center rather than Ptolemy's, the laborious iterative method used by Ptolemy, Copernicus, and Reinhold can be replaced by a single direct solution. Reinhold, who carried the derivation of Jupiter's eccentricity and apsidal direction through nine iterations in his commentary on *De revolutionibus*, would have been amazed to learn this.

### THE DERIVATION OF THE HELIOCENTRIC THEORY

The critical document for reconstructing Copernicus's derivation of his planetary theory is the page of notes we have called U. It contains two groups of numbers. The higher numbers are taken from the sines of the maximum equations of center and of the anomaly in the *Alphonsine Tables*, and the lower show the conversion of these to the parameters in the *Commentariolus*. This in itself grants enormous importance to U. But there is more. The expressions used by Copernicus when writing U show a stage in the development of his planetary theory prior to the *Commentariolus*—indeed the contents of U may not even be fully "Copernican"—and provide the most compelling evidence for the analysis by which Copernicus arrived at his great discovery.

Consider the lines:

| | | |
|---|---|---|
| | Eccentricitas Martis | 6583 |
| | Jovis ecce[ntricitas] | 1917 |
| | Saturni ecce[ntricitas] | 1083 |
| 376 | Mercurii ecce[ntricitas] | 2256 [or 2259] |

The numerical values themselves will be discussed in the next section on the derivation of the parameters. They are, in fact, the sines of the maximum equations of the anomaly when, in Ptolemy's model, the center of the epicycle is at mean distance from the earth, as shown in figure 23. These numbers directly give the proportion of the radius of the epicycle to the radius of the eccentric where the radius of the eccentric is 10000. Copernicus, however, calls the number for each planet an *eccentricitas*. The substitution of an eccentricity for the epicyclic radius can refer only to the eccentric model for the second anomaly mentioned briefly by Ptolemy in *Almagest* XII, 1 (Manitius **2**, 268–269); it is this alternate model that leads directly to the heliocentric theory.[5]

Ptolemy's description of the model is prefatory to the demonstration of Apollonius's theorem on the location of the stationary points and the length of the retrograde arc. Apollonius evidently proved the theorem for both the epicyclic and equivalent eccentric model of the second anomaly, and Ptolemy does the same. The description of the eccentric model is so concise that Ptolemy must have assumed that his reader was already familiar with it. Perhaps for this reason there are almost no later astronomers who pay any attention to the model. However, a notable exception, and for our interests a crucially important exception, is Regiomontanus. For some reason the eccentric model must have caught Regiomontanus's attention, and in *Epitome* XII, 1 he wrote a detailed description of the equivalence of the two models for the superior planets, thereby calling the attention of any reader of the *Epitome* to the alternate model. Ptolemy had said that the eccentric representation of the second anomaly was usable only for the superior planets, but in *Epitome* XII, 2 Regiomontanus describes an equivalent eccentric model for the inferior planets which is evidently his own invention. In the analysis of these models the second anomaly is treated independently of additional complications that would be introduced by the first anomaly.

I believe that Copernicus arrived at the heliocentric theory after a careful investigation of these two propositions in Book XII of the *Epitome* in which he drew further conclusions from the eccentric model of the second anomaly leading to the heliocentric representation of planetary motion. Regiomontanus either failed to recognize the immediate heliocentric conversion of the

[4] Viète is skeptical about both the Copernican and Tychonic representations of the second anomaly, but he is more than happy to provide a variety of models for geocentric, Copernican, and Tychonic theories with dazzling virtuosity and lofty disdain for his slow-witted predecessors who had not the good fortune to be mathematicians and French.

[5] The equivalence of the epicyclic and eccentric representations of the second anomaly and their relation to Copernican and Tychonic theory are discussed by Herz (1887–1894: **2**: pp. 75–76; 1897: **1**: pp. 55–58), who, divitiarum suarum ipse ignarus, unfortunately does not propose that Copernicus followed this reasonable analysis.

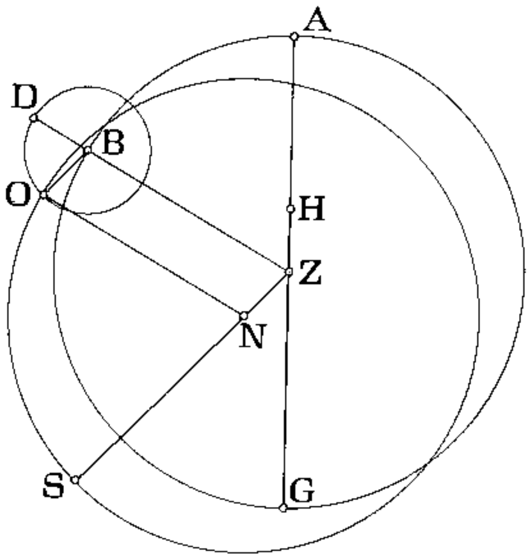


Fig. 17

model—which I think unlikely in view of his profound understanding of planetary theory throughout the *Epitome* and his careful and original analysis of the eccentric model of the second anomaly—or recognized the conversion very clearly but refused to believe that it was physically possible.[6] In any case, the use of the word *eccentricitas* in U makes it next to certain that Copernicus's derivation of his theory rests upon the eccentric model of the second anomaly and therefore upon these two propositions in the *Epitome*. In this way Regiomontanus provided the foundation of Copernicus's great discovery. It is even possible that, had Regiomontanus not written his detailed description of the eccentric model, Copernicus would never have developed the heliocentric theory.

*Epitome* XII, 1 is as follows (the figure in the text is figure 17, and I have drawn a modified version in figure 18):

1. *If you assume only one anomaly in the superior planets, an epicycle on a concentric or*

[6] It is also possible that Regiomontanus saw no particular sense in assuming that an empty point, the mean sun, was the center of the planetary system. Kepler expressed his suspicion of this in *Mysterium cosmographicum,* cap. 18 (*Werke* **1**: pp. 62–63 = **8**: p. 101) where he chose to measure planetary distances from the true sun; in the *Astronomia nova* he proved that it was indeed incorrect.

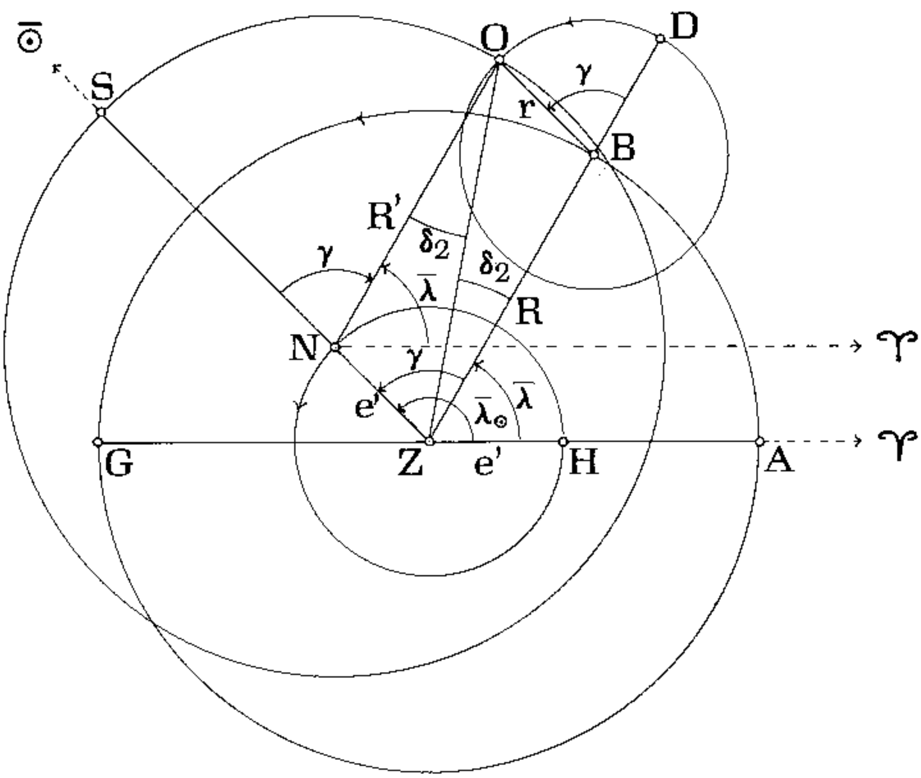


Fig. 18

*an eccentric without an epicycle will be a sufficient cause for this anomaly.*

Consider the anomaly that is referred to the sun. Now let us assume that the motion of the epicycle on the concentric and the motion of the planet on the epicycle added together are equal to the mean motion of the sun, just as the earlier demonstrations required. Let the center of the eccentric move in the order of the signs with a velocity equal to the motion of the sun, and let the planet itself move in the same direction [on the eccentric] with the velocity with which the epicycle moves on the concentric. Indeed the line drawn from the center of the world parallel to the line extending from the center of the eccentric through the center of the planet determines the mean position of the planet.

Now, let a circle concentric to the world be *ABG* about center *Z*, and let *A* be the point in which the center of the epicycle was located when the planet was in the apogee of the epicycle, that is, in point *D*, and when the sun in its mean motion was in conjunction with the planet and point *H* was the center of the eccentric. Now, however, let the epicycle be at point *B* and the planet in the epicycle at point *O*. So, after drawing lines *AZG*, *ZBD*, *BO*, *NO*, *ZO*, and *ZS*, angle *AZB* will be the motion in longitude and angle *DBO* the motion of anomaly or the mean motion of the argument. Let angle *AZS* be the mean motion of the sun. Therefore, the center of the eccentric will be on line *ZS*, and let it be *N*.

Let us assume first that the concentric and eccentric are equal, and that the proportion of the semidiameter of the concentric to the semidiameter of the epicycle is equal to the proportion of the semidiameter of the eccentric to the distance between the centers. Therefore, line *ZH* or *ZN* will be equal to *BO*. Now, since

$$\text{angle } AZB + \text{angle } DBO = \text{angle } AZS,$$

subtracting the common angle *AZB*,

$$\text{angle } AZS - \text{angle } AZB = \text{angle } BZS = \text{angle } DBO.$$

Therefore, the two lines *ZN* and *BO* are parallel, and because they are equal, the two lines *ZB* and *NO* will be equal and parallel. Hence, when a circle is described about center *N* with a semidiameter equal to the semidiameter of the concentric, its circumference will pass through point *O*. And because *ZB* is assumed to be the line of the mean motion of the planet, which in fact is parallel to line *NO* drawn from the center of the eccentric, [therefore] the planet will be on line *NO* and therefore it will be in point *O*. But in the epicyclic model it was also placed in the same point. Therefore, in both models the line [*ZO*] along which the planet is seen by an observer located at the center of the world is the same, and angle *SNO* will be equal to angle *DBO* of the mean anomaly.

But if you assume that the semidiameters of the eccentric and concentric are unequal, so long as the proportion of the semidiameter of the concentric to the semidiameter of the epicycle is equal to the proportion of the semidiameter of the eccentric to the distance between the centers, the same thing will follow, as you can very easily find out from what was demonstrated for the moon.[7]

The principle in the conversion is simply that, upon completing parallelogram *ZBON* and describing circles of radii *NO* and *ZN*, the center of the eccentric *N* will move about *Z* with the velocity of the mean sun $\bar{\lambda}_{\odot}$ and, in fact, *N* will always lie in the direction of $\bar{\odot}$, while the planet *O* will move in the opposite direction on the eccentric with the velocity of the mean anomaly $\gamma$. In both models the planet will lie on line *ZO* at *O*. If the motion of *O* about *N* is referred to the fixed direction $N\Upsilon$, it will move in the same direction as *N* through the mean motion in longitude $\bar{\lambda}$, and this is the way Regiomontanus describes the motion of the planet on the eccentric. Ptolemy had said instead that the planet moves in the opposite direction through $\gamma$ measured from *S* which, as the figure shows, is equivalent. In both the epicyclic and eccentric models, the equations of the anomaly $\delta_2$ are equal because the epicyclic radius $r$ has become the eccentricity $e'$. This is the eccentricity in Copernicus's notes. Regiomontanus also points out that $e'$ does not have to be equal to $r$; it is sufficient that the proportion of the eccentricity to the radius of the eccentric be equal to the proportion of the radius of the epicycle to the radius of the concentric, that is, if $R'$ is the radius of the eccentric

$$\frac{e'}{R'} = \frac{r}{R}.$$

Thus if the eccentricity $e'$ is given a constant value $c$ for all the planets, the radius of each

[7] The reference is to IV, 8. The same thing is proved for the sun in III, 10.

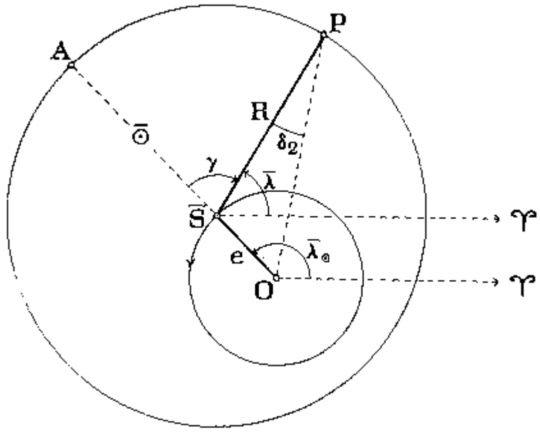


Fig. 19

planet's eccentric $R'$ is simply

$$R' = \frac{R}{e'} \cdot c.$$

This is exactly what Copernicus does in the lower part of U as is shown by the heading "Proportion of the heavenly spheres to an eccentricity of 25 parts." He has already shown in the higher numbers the relative eccentricity $e' = r$ where $R = 10000$. In the lower numbers, he finds $R'$ where $c = 25$ parts, and $R'$ becomes the semidiameter of the planet's sphere.

In holding the eccentricity constant, Copernicus has, of course, done something of enormous importance, for although he did not mention it in U, we know that he also assumed the eccentricity to be the distance between the earth and the mean sun. Figure 19 shows the eccentric model of the second anomaly. The earth is at $O$, the center of the eccentric $\bar{S}$ moves through $\bar{\lambda}_\odot$ measured from ♈ on a circle whose radius is the eccentricity $e$, and the planet $P$ moves in the opposite direction through the mean anomaly $\gamma$ on the eccentric of radius $R$. When the motion of $P$ is measured from the fixed direction to ♈, it moves in the same direction as $\bar{S}$ through the mean motion in longitude $\bar{\lambda}$ because

$$\bar{\lambda} = \bar{\lambda}_\odot - \gamma.$$

The equation of the anomaly is $\delta_2$. Now, as seen from $O$, the center of the eccentric $\bar{S}$ always lies in the direction of the mean sun $\bar{\odot}$. What happens if $\bar{\odot}$ is moved to $\bar{S}$, that is, if it is assumed that $\bar{S}$ is in fact the mean sun? This was the crucial part of Copernicus's investigation. What happens is that $e$ becomes the distance of the mean sun from the earth and $R$ becomes the distance of the planet from the mean sun, now located at the center of its circle. By the method just shown, Copernicus can now measure the radii of the orbits of the superior planets in proportion to the constant distance from the earth to the mean sun, as indeed he does in the lower part of U.

But the work is not yet complete. So far we have not the Copernican, but the so-called Tychonic theory. The center of the planetary orbits is the mean sun, which in turn moves in a circle about the fixed earth. And what of the inferior planets? Since in the epicyclic model of the second anomaly the centers of the epicycles of the inferior planets always lie in the direction from the earth to the mean sun, it would be easy to assume that the centers of the epicycles coincide with the mean sun, thus bringing the inferior planets into the Tychonic system already reached for the superior planets. Did Copernicus ever consider the Tychonic theory? In the course of his analysis of the second anomaly he must have been aware of its possibility. Indeed we have seen that the eccentric model of the second anomaly for the superior planets leads directly to the Tychonic theory. Could the term Great Sphere, used by Copernicus for the sphere carrying the earth around the mean sun, have originally designated the sphere carrying the mean sun, the center of all the planetary spheres, around the fixed earth? I do not know the answer to this question, but calling the sphere performing this important function the Great Sphere would surely be appropriate.

However, when Copernicus wrote U, he was not representing the second anomaly of the inferior planets by an epicycle, for he also refers to the sine of the maximum equation of the anomaly of Mercury as an eccentricity. This brings us to Regiomontanus's own eccentric model for the inferior planets described in *Epitome* XII, 2. Although only the first part of XII, 2 is of interest in this context, I have for completeness translated the whole proposition. The figure in the text is again figure 17, and in figure 20 I have redrawn it with motions and distances added.

2. *The same thing is necessarily seen in the case of Venus and Mercury.*

Let us assume that the motion of the epicycle on the concentric is equal in velocity to the mean motion of the sun, and that the motion of anomaly [of the planet on the epicycle] is proper

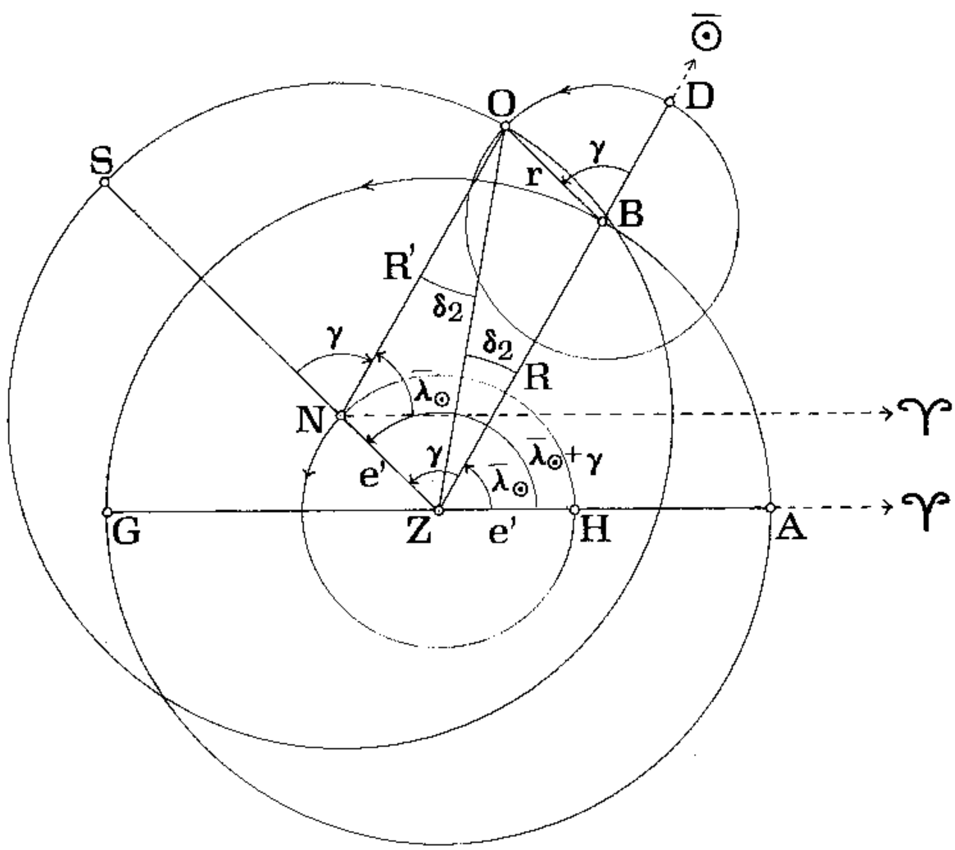


Fig. 20

to each planet, but that the motion of the center of the eccentric in the order of the signs is equal to the sum of the mean motion of the sun and the mean motion of the anomaly.

Now, repeat the previous figure in which angle *AZB* is the mean motion of the sun. Angle *BZS* will be equal to angle *DBO*, the motion of anomaly. Therefore, line *ZN* will be parallel to line *OB*, and the rest will be as before.

From these things it clearly follows that whatever in the way of station and retrogradation happens to the planet in the model of the epicycle and concentric happens to it also in the eccentric model even though both the center of the eccentric and the line of the mean motion of the planet move only in the order of the signs. In fact, [the station and retrogradation] will occur in corresponding places. I mean that if at a certain distance of the planet from the apogee of the epicycle the planet appears stationary, at an equal distance from the apogee of the eccentric it will likewise appear stationary.

So now if the planet had only one anomaly in its motion, as Apollonius and other ancients thought, it would be sufficient to show the cause of the station or retrogradation by means of the epicyclic model. Since, however, we demonstrated earlier [that the planet has] an anomaly of two components, that is, due to the eccentric and to the epicycle, we would labor in vain to find the stationary points in an eccentric by itself or in an epicycle and concentric. Therefore, I do not bother with that. So now let us take up the matter at hand, and in order that we may understand it more clearly, let us investigate a certain preliminary theorem.[8]

[8] There follows in XII, 3 the lemma about the triangle with a cut-off segment given in *Almagest* XII, 1 (Manitius, 2: pp. 272–273). Note that Regiomontanus's remark that Apollonius and the ancients were ignorant of the first anomaly is reproduced by Copernicus in *De rev.* V, 35, in which Copernicus then gives the lemma and the proof of Apollonius's theorem. To the best of my knowledge, Regiomontanus's eccentric model of the second anomaly for the inferior planets has never received any notice, but neither to my knowledge has his description of the corresponding model for the superior planets. While I do not

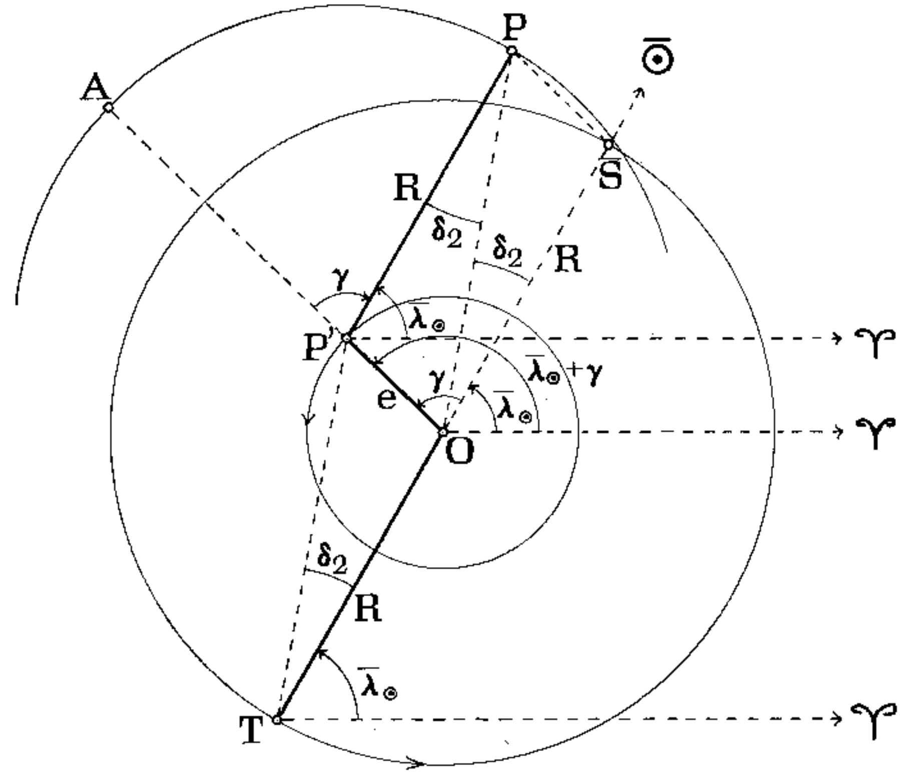


Fig. 21

The description is not as complete as for the superior planets since repetition of details is unnecessary, but the principal features of the model are brought out. The center $N$ of the eccentric moves with the velocity of the mean sun plus the mean anomaly, that is $AZN = \bar{\lambda}_{\odot} + \gamma$, while the planet $O$, moving in the opposite direction on the eccentric through angle $\gamma$ measured from the apogee $S$, moves in the same direction as $N$ through $\bar{\lambda}_{\odot}$ when its motion is measured from ♈. Again, in both models the planet will lie on $ZO$ at $O$. The equation of the anomaly $\delta_2$ is the same in both models for the radius of the epicycle $r$ has become the eccentricity $e'$, the eccentricity mentioned in U for Mercury. But in the case of an inferior planet, $e'$ cannot be fixed as the distance from the earth to the mean sun. Since the mean sun lies somewhere in the direction $ZB$ while $e'$, which corresponds to $r$, subtends the angle of the planet's apparent distance from the mean sun, $e'$ must show the radius of the planet's sphere.

The transformation, which leads directly to the Copernican rather than Tychonic theory, is shown in figure 21. In the eccentric model the earth is at $O$, the center of the eccentric $P'$ moves about $O$ through $\bar{\lambda}_{\odot} + \gamma$ on a circle of radius $e$, and the planet $P$ moves on the eccentric of radius $R$ in the direction opposite to the motion of $P'$ through $\gamma$ measured from $A$, but in the same direction as $P'$ through $\bar{\lambda}_{\odot}$ when measured from ♈. $P'P$ thus remains parallel to the direction $O\bar{\odot}$ from the earth to the mean sun. Now the mean sun could simply be placed at $\bar{S}$, which, as figure 20 shows, corresponds to the center of the epicycle, but the note on Mercury in U makes it clear that it is $e$, not the radius of the epicycle, that corresponds to the sine of the maximum equation of the anomaly and thus to the radius of the sphere of an inferior planet. Therefore, it is necessary to move the mean sun beyond $\bar{S}$ through distance $R$ to $O$, move the planet through $R$ from $P$ to $P'$, and likewise move the earth through $R$ from $O$ to $T$, leaving $O$ the fixed point as before so that $T$ moves about $O$ on a circle of radius $R$. This seemingly complicated transformation is really just moving the three points

believe that Regiomontanus ever advocated the heliocentric theory, he was, through these two propositions, virtually handing it to any taker.

$\bar{S}$, $P$, and $O$ in the same direction through the same distance $R$. Copernicus has already fixed the distance $TO = R = \bar{S}O$ as the distance from the earth to the mean sun in the analysis of the superior planets, so $e$ now shows the radius of the sphere of an inferior planet in proportion to the distance between the earth and the mean sun.

There is still a difficult question. Granting that the eccentric model for the second anomaly of the inferior planets leads to the Copernican theory, we have seen that the corresponding model for the superior planets leads to the Tychonic. Why did Copernicus choose one rather than the other? While I am confident that the evidence provided by U and *Epitome* XII, 1 and 2 confirms the preceding reconstruction of Copernicus's derivation of the revolution of the planets around the mean sun, U offers no evidence of whether the earth revolves around the mean sun or the mean sun around the earth—it shows only that the distance from the earth to the center of the planetary spheres has been made constant—and the two models in the *Epitome* clearly offer either possibility. Some further consideration outside our analysis must have moved Copernicus to his final decision, and this decision may have been made after he wrote U.

The Tychonic theory has two distinct advantages. There can be no stellar parallax, so the fixed stars need not be at an enormous distance, and the diurnal rotation of the earth is not necessary, so the one point on which Copernicus knows himself to be vulnerable to the attack of natural philosophers is eliminated. Copernicus, however, was evidently not impressed by these arguments.

There are any number of possible reasons for Copernicus's choice. Some, of course, have to do with the sun's being very large, and very beautiful, and very majestic, so that it should not move, motion being inappropriate to large, beautiful, majestic things. Now Copernicus wrote an elegant pastiche of classical pronouncements on the grandeur of the sun in *De rev.* I, 10, but the absence of any rhetoric of this kind in the *Commentariolus*, and its utter uselessness for the investigation of planetary theory, lead me to believe that such sentiments, however admirable, had nothing to do with Copernicus's decision to let the earth rather than the sun move.[9]

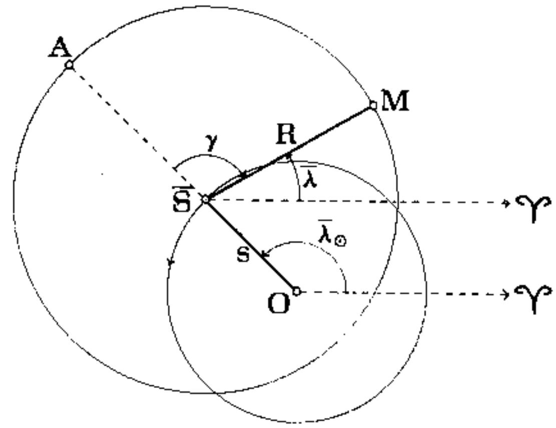


Fig. 22

There is, however, a different consideration very closely connected with the principles of Copernicus's planetary theory that may have seemed to Copernicus a highly compelling reason for preferring the motion of the earth. It is a peculiarity of the Tychonic theory that cannot fail to strike anyone who has seen an illustration of the system. This is the intersection of the orbits of Mars and the mean sun shown in figure 22. The earth is at $O$, the mean sun at $\bar{S}$, moving about $O$ through $\bar{\lambda}_\odot$ on a circle of radius $s$, and Mars at $M$, moving about $\bar{S}$ in the opposite direction through $\gamma$ on a circle of radius $R$. Since $R \approx \frac{3}{2}\, s$, the circles intersect. There is, of course, no danger of the bodies of Mars and the sun colliding since the sun is always at the center of Mars's orbit. Nevertheless, Copernicus, who held as his first principle that the planets are carried by spheres, could well have considered this intersection of two spheres highly objectionable if not simply impossible. The intersection would be of no concern to Tycho since it was Tycho who disproved the existence of planetary spheres. I can cite no evidence for this conjecture, but the reasoning does seem in keeping with Copernicus's meticulous insistence on constructing planetary motions out of the rotation of spheres. If correct, it is indeed ironic that the most admittedly medieval aspect of Copernicus's theory would be partially responsible for his most radical assertion that the earth moves about the sun.

In conclusion, the preceding analysis of the

[9] I realize that a number of fashionable scholars (if I may abuse that word), who have sacrificed their reason on the altar of spiritualism, think otherwise. Let them dispute with those who believe Copernicus found his *inspiration* from the Presocratics.

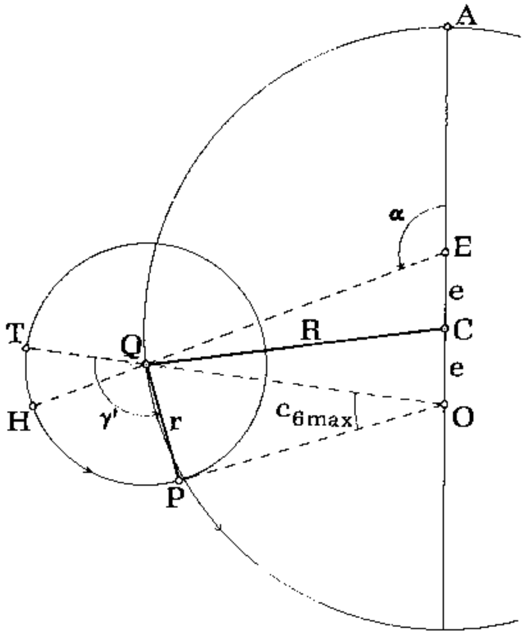


Fig. 23

derivation of Copernicus's heliocentric theory may be summarized as follows:

1. The use of the word *eccentricitas* in U for the sine of the maximum equation of the anomaly shows that Copernicus was investigating the eccentric model of the second anomaly. My entire analysis hangs on this one word.

2. Copernicus's attention was drawn to this model by Regiomontanus's description in *Epitome* XII, 1 and 2.

3. The lower part of U, in which the eccentricity is held constant at 25 parts and the radii of the planetary spheres measured in the same units as the constant eccentricity, shows that Copernicus had (probably) identified the center of the planetary spheres with the mean sun.

4. The final decision to let the earth move about the mean sun may have been determined by the intersection of the spheres of the mean sun and Mars if the mean sun is allowed to revolve around the earth. This last point, since there is no written evidence, is pure speculation.

THE DERIVATION OF THE PARAMETERS

The derivation of the parameters in the *Commentariolus* is shown by U. Consider first the eccentricities in the higher group of numbers. These are the sines of the maximum equations of the anomaly when the epicycle in Ptolemy's model is at mean distance from the earth. The conditions are shown in figure 23 in which $OQ = CQ = R$ and $OP$ is tangent the epicycle. The equation of the anomaly at mean distance is $c_6$ in the *Alphonsine Tables*. Where $R = 10000$, the sine of $c_{6max}$ will be the radius of the epicycle $r$. Comparing these with the eccentricities in U and the value of $r$ in the *Almagest*, we have:

| | $c_{6max}$ | sin $c_{6max}$ | U Eccentricity | *Almagest r* |
|---|---|---|---|---|
| Mars | 41;10° | 6583 | 6583 | 6583 |
| Jupiter | 11;3° | 1917 | 1917 | 1917 |
| Saturn | 6;13° | 1083 | 1083 | 1083 |

The sine of $c_{6max}$ gives $r$ with full precision to 4 places; from this alone it would not be certain that Copernicus was extracting parameters from the *Alphonsine Tables* rather than taking the epicyclic radii from the *Epitome.*

That he was using sines of equations is, however, made clear from the radii of the two epicycles of the first anomaly. These are called epicycle *primus* and *secundus* for Mars, and epicycle *a* and *b* for the other planets. It was shown earlier that $r_1 + r_2 = 2e$, where $e$ is the eccentricity in Ptolemy's model. $2e$ can be extracted directly from the maximum equation of center, $c_{3max}$ in the *Alphonsine Tables*, which occurs when $\alpha = 90° + \frac{1}{2}c_{3max}$ and $QC$ is perpendicular to $EO$

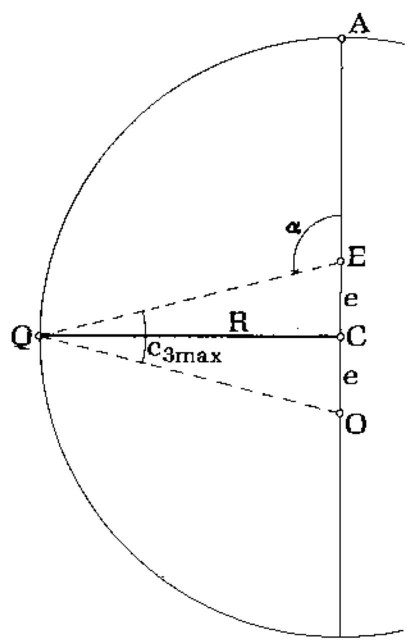


Fig. 24

as shown in figure 24. Now, accurately

$$2e = 2\tan \tfrac{1}{2}c_{3\max}R.$$

Copernicus, however, computed

$$2e = \sin c_{3\max}R,$$

and the result differs from the correct value enough to be noticeable. In the following comparison, note that in U the first epicycle of Mars, 1492, must be reduced to 1482 as the stroke under the 9 indicates. We also give for comparison $(c_3 + c_4)_{\max}$ and $2e$ from the *Almagest*.

| | Mars | Jupiter | Saturn |
|---|---|---|---|
| Alphonsine $c_{3\max}$ | 11;24° | 5;57° | 6;31° |
| $\sin c_{3\max}$ | 1977 | 1037 | 1135 |
| U $r_1 + r_2$ | 1976 | 1036 | 1136 |
| U $r_1$ | 1482 | 777 | 852 |
| U $r_2$ | 494 | 259 | 284 |
| *Almagest* $(c_3 + c_4)_{\max}$ | 11;25° | 5;15° | 6;31° |
| *Almagest* $2e$ | 2000 | 918 | 1139 |

The differences of 1 between $\sin c_{3\max}$ and $r_1 + r_2$ are the result of $r_1 + r_2$ necessarily being divisible by 4. The disagreements with the *Almagest* are sufficient to rule out the *Epitome* as the source of the epicyclic radii. The most notable difference is for Jupiter where $2e$ in the *Alphonsine Tables* is in fact 1038 instead of 918.[10] Thus there is no question that only the *Alphonsine Tables* provide the maximum equations leading to the values of $r_1$ and $r_2$ contained in U.

Copernicus now has the eccentricity $e$ of the model in figure 19, and the sine of $c_{3\max}$, that is, the sum of the epicyclic radii, both where $R = 10000$. He now finds $r_2$ by $r_2 = \frac{1}{4}\sin c_{3\max}$ and $r_1$ by $r_1 = 3r_2$, rounding to integers to produce the results in U. Next, and this is the most important step, he holds $e$ constant at 25 parts, that is, he places the mean sun at $\bar{S}$ and lets $e$ be the distance between the earth and mean sun, and computes the radii of the planetary spheres and epicycles in proportion to this unit. The radius $R'$ of the sphere follows from

$$R' = \frac{10000}{e}\cdot 25 = \frac{25\cdot 2{,}46{,}40}{e}$$

the radius of the larger epicycle $r_1'$, from

$$r_1' = \frac{r_1}{10000}\cdot R' = \frac{r_1R'}{2{,}46{,}40}$$

and then the radius of the smaller epicycle $r_2'$ is

$$r_2' = \tfrac{1}{3}r_1'.$$

It appears from the fractions that these computations were done sexagesimally or at least the results were converted to sexagesimal fractions before being converted to the various fractions in U. The recomputations shown below were done sexagesimally. $r_1'$ was recomputed from rounded values of $R'$, that is, Mars 38, Jupiter 130 and Saturn 231; and $r_2'$ from the text values of $r_1'$.

| | | Mars | Jupiter | Saturn |
|---|---|---|---|---|
| U | $R'$ | 38 | 130;25 | $230\frac{5}{6}$ |
| recomp. | $R'$ | 37;58,35 | 130;24,43 | 230;50,24 |
| U | $r_1'$ | 5;34 | $10\frac{1}{10}$ | 19;41 |
| recomp. | $r_1'$ | 5;37,53 | 10;6,3 | 19;40,52 |
| U | $r_2'$ | [1];51 | $3\frac{11}{30}$ | $6\frac{17}{30}$ |
| recomp. | $r_2'$ | 1;51,20 | 3;22 | 6;33,04 |

Everything is satisfactory except for Mars $r_1'$, which should be 5;38, and $r_2'$, which Copernicus wrote incorrectly as 51′ instead of 1;51. This latter error was carried over into the text of the *Commentariolus*, indicating that Copernicus must have copied the parameters from U without noticing his mistake. One would, naturally, not expect him to have caught the computational error for $r_1'$.

The longitudes of the apsidal lines do not agree as well with the Alphonsine values. Just as he did for the sun, Copernicus gives these as distances from fixed stars. Thus,

$\lambda_A$ Saturn near the star on the elbow of Sagittarius,

$\lambda_A$ Jupiter 8° east of the end of the tail of Leo,

$\lambda_A$ Mars $6\frac{1}{2}$° west of the heart of Leo (Regulus).

The stars are respectively $h^2$ Sagittarii, $\beta$ Leonis, and $\alpha$ Leonis. We now compare the tropical

[10] This and Venus's equation of center, which is identical to that of the sun (see below pp. 490, 493), are the only significant deviations between the planetary correction tables in the *Alphonsine Tables* and the *Toledan Tables*. The latter in turn are identical to Battānī's tables except for the 0;1° difference in the maximum equation of center for Mars, 11;24°, which is likewise found in the *Alphonsine Tables* (Toomer, 1968: pp. 63–64). Battānī's tables are identical to Ptolemy's *Handy Tables* except for Venus's equation of center. The line of transmission of these tables from Ptolemy to the Alphonsine astronomers is obvious.

longitudes of the apogees from the *Alphonsine Tables* for the supposed epoch of the Alphonsine star catalog, Era Alfonso = June 0, 1252, with the longitudes of these stars in the catalog.

$h^2$ Sagittarii $- \lambda_A$ Saturn
$= 4,41;58° - 4,10;39° = 31;19°$ west

$\lambda_A$ Jupiter $- \beta$ Leonis
$= 2,50;53° - 2,41;38° = 9;15°$ east

$\alpha$ Leonis $- \lambda_A$ Mars
$= 2,19;38° - 2,12;28° = 7;10°$ west

Mars differs from Copernicus's value by 0;40° west, Jupiter by 1;15° east, but Saturn by the enormous amount of 31;19° west. Since such a longitude for Saturn's apogee is unheard of, I would guess that Copernicus made an error of $1^s = 30°$ in noting its position. I cannot explain the small differences for Mars and Jupiter, just as I cannot explain the small difference of 1;47° west for the sun.[11]

[11] On the correct location of Saturn's node, see below (p. 488). It is possible that Copernicus computed by inspection from the tables and produced careless results; it is also possible that the text of the *Commentariolus* is corrupt (but for every number?), but there is no way of verifying this. The sidereal longitudes of the apsidal lines written on f. 15r of U (see above, p. 427) are from *De revolutionibus* and do not fit the positions given in the *Commentariolus* at all. I have tried using (1) Ptolemy's apogees, (2) the Alphonsine apogees, (3) Ptolemy's star catalog in Valla's printing and (4) in the 1515 *Almagest*, and (5) the Alphonsine star catalog in every combination, but to no use. Trying various epochs and different values of precession between stellar longitudes and apogees at different epochs is also useless; the best agreement remains the Alphonsine positions given above. It is unthinkable that the apsidal longitudes in the *Commentariolus* were independently derived because the epicyclic radii were merely extracted from tables and the two parameters must come from the same derivation from three oppositions.

### DESCRIPTION OF THE SECOND ANOMALY

Now there is another anomaly, in which the planet is seen sometimes to move retrograde and often to stand still, which does not result from the motion of the planet, but rather from the motion of the earth in the great sphere changing the position of the observer. For the earth, exceeding the motion of the planet in velocity, overcomes the motion of the planet because the line of sight [passing through the planet] to the fixed stars[a] is moving in the direction opposite to [the motion of] the position of the observer. This happens especially when the earth is closest to the planet, that is, when it is between the sun and and the planet at the evening rising of the planet. It is, however, the other way around near evening setting or morning rising [when the motion of the observer] carries the line of sight [passing through the planet] forward. Where, however, the line of sight [passing through the planet] moves in the direction opposite to the motion [of the observer] with an equal velocity, [the planet] appears[b] to stand still since under these conditions the opposite motions cancel each other out. This usually occurs near trine aspect to the sun. In all these [apparent motions], the lower the sphere in which the planet moves, the greater this anomaly. Thus it is smaller in Saturn than in Jupiter, and again greatest in Mars, in the proportion of the semidiameter of the great sphere to the semidiameters of their spheres. The maximum [equation] for each occurs when the planet is seen along the line of sight tangent to the circumference of the great sphere. These three planets do indeed [appear] to us to wander about.

[a] 196:14 ad *firmamentum* (SV firmamenti)
[b] 196:20 stare *videtur* (S)

This is Copernicus's explanation of how the motion of the earth causes the second anomaly. While what he intends to say is clear enough, the description is highly elliptical, and I am far from certain that my filled-out translation of this passage is correct.

The remark that the stationary points are at trine aspect (±120°) to the sun, which Copernicus may have taken from Pliny II, 59 (Rosen, p. 78), is a fairly crude estimate. The anomaly at which a station occurs must be determined by Apollonius's theorem. How to do this is shown in *Epitome* XII, but no numerical values are given. It is probable that Copernicus did not carry out the derivations since he does not do so even in *De revolutionibus*. Now imagine that a station occurs when the planet is at aphelion.[12] This will simplify what follows since the equation

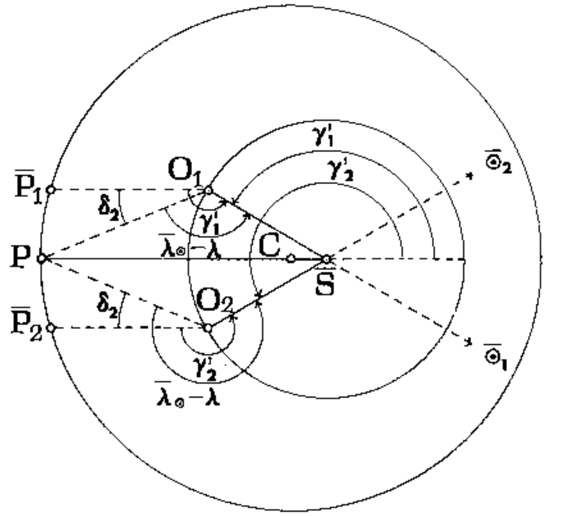


Fig. 25

of center is eliminated. In figure 25 the earth is at $O$, the mean planet at $\bar{P}$, the true planet at $P$

[12] The reader will note, and perhaps forgive, my misuse of *aphelion* to mean the point of greatest distance from the mean sun.

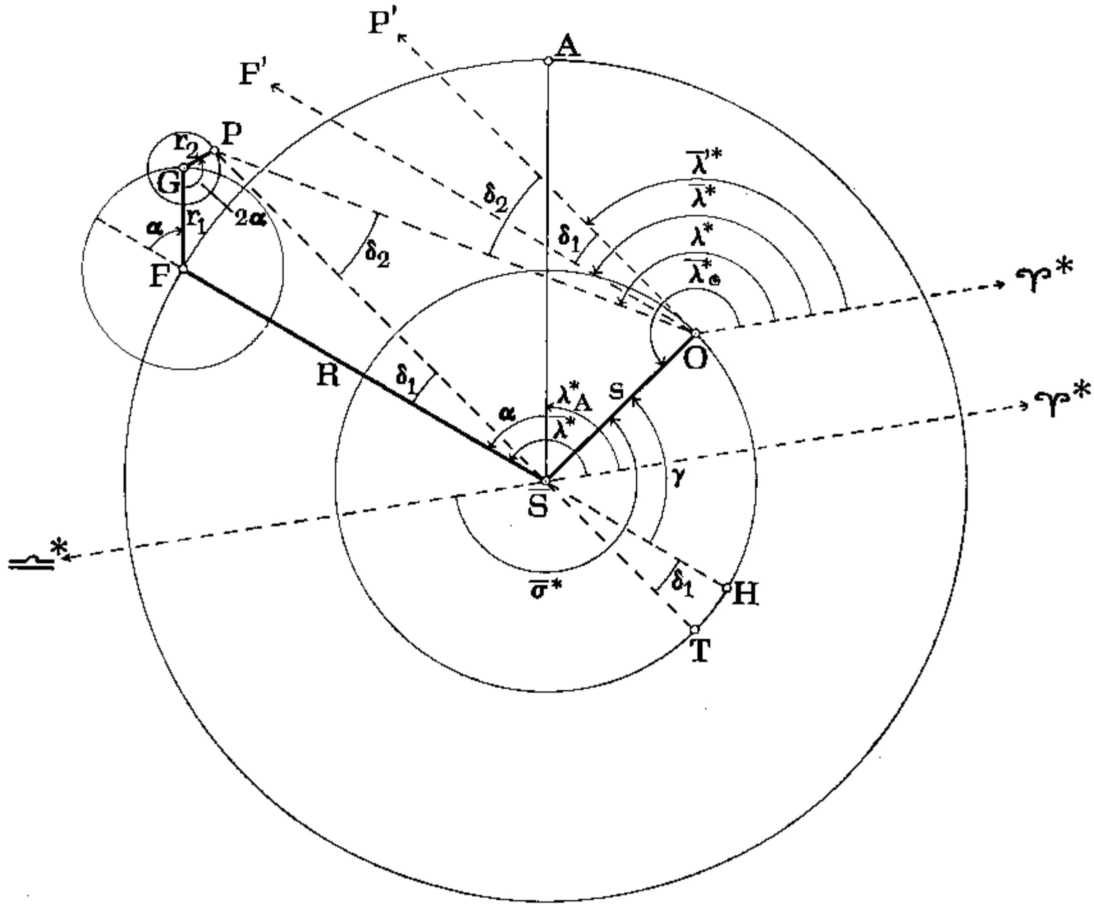


Fig. 26

with an anomaly $\gamma$, and the angle between the mean sun and true planet, $\bar{\lambda}_\odot - \lambda$, is

$$\bar{\lambda}_\odot - \lambda = \gamma \pm \delta_2$$

where the equation of the anomaly $\delta_2$ is negative for first station and positive for second station. For these conditions, highly simplified by eliminating the equation of center, we find from the tables in *Almagest* XII, 8 and XI, 11 the following values of $\bar{\lambda}_\odot - \lambda$:

| | First Station | Second Station |
|---|---|---|
| Saturn | 112;45° − 5;37° = 107;8° | 247;15° + 5;37° = 252;52° |
| Jupiter | 124;5° − 9;36° = 114;29° | 235;55° + 9;36° = 245;31° |
| Mars | 157;28° − 27;8° = 130;20° | 202;32° + 27;8° = 229;40° |

Saturn is 12;52°, Jupiter 5;31°, and Mars 10;20° from trine aspect to the mean sun, from which the true sun can differ by about ±2°. Introducing the equation of center can produce stations as far as 25° from trine aspect in the case of Mars, so Copernicus's estimate is very crude indeed.

Having now analyzed the first and second anomalies separately, we finally consider the complete Copernican model. Since the mean sun is the center of the orbit of both the planet and the earth, the second anomaly is treated exactly as in Ptolemy's with no regard taken of the anomaly of the terrestrial (solar) motion. The model is shown in figure 26. With the mean sun $\bar{S}$ as center, we describe a circle of radius $s$ carrying the earth at $O$ and a circle of radius $R$ carrying the center $F$ of the larger epicycle. We let $F$ move through $\bar{\lambda}^*$ measured from ♈*, and $O$ move through $\bar{\sigma}^*$ measured ♎* so that $\bar{S}$ will appear from $O$ to have moved through $\bar{\lambda}^*_\odot = \bar{\sigma}^*$ from ♈*. Taking $\bar{S}A$ as the direction of the apsidal line of longitude $\lambda^*_A$, we say that $F$ is distant from $A$ by $\alpha = \bar{\lambda}^* - \lambda^*_A$, and we describe the two epicycles as in figures 14–16 of the first anomaly. Let $F\bar{S}$ meet the orbit of the earth at $H$, and draw $P\bar{S}T$. Draw $OF'$ parallel to $\bar{S}F$, and $OP'$ parallel to $\bar{S}P$. Now, measured from $H$, $O$ has moved through $\gamma = \bar{\lambda}^*_\odot - \bar{\lambda}^*$. We find the equation of center $\delta_1$ as shown earlier and form

$$\bar{\lambda}'^* = \bar{\lambda}^* \pm \delta_1 \quad \begin{array}{l} - \text{ for } 0° \leq \alpha \leq 180° \\ + \text{ for } 180° \leq \alpha \leq 360° \end{array}$$

and

$$\gamma' = \gamma \pm \delta_1. \quad \begin{array}{l} + \text{ for } 0° \leq \alpha \leq 180° \\ - \text{ for } 180° \leq \alpha \leq 360° \end{array}$$

We then compute the distance $\bar{S}P$ from

$$\bar{S}P = \sqrt{[R + (r_1 - r_2)\cos\alpha]^2 + [(r_1 + r_2)\sin\alpha]^2}.$$

The equation of the anomaly $\delta_2$ follows from

$$\tan\delta_2 = \frac{|s \sin\gamma'|}{\bar{S}P + s\cos\gamma'},$$

and the true longitude $\lambda^*$ is then

$$\lambda^* = \bar{\lambda}'^* \pm \delta_2. \quad \begin{array}{l} + \text{ for } 0° \leq \gamma' \leq 180° \\ - \text{ for } 180° \leq \gamma' \leq 360° \end{array}$$

To do all this more rapidly, we let $\delta$ be the difference between $\bar{\lambda}^*$ and $\lambda^*$, and find the entire correction from

$$\tan\delta = \frac{|-(r_1 + r_2)\sin\alpha + s\sin\gamma|}{R + (r_1 - r_2)\cos\alpha + s\cos\gamma}.$$

This is Viète's method. It is simpler, faster, and avoids all errors of approximation. The true longitude is

$$\lambda^* = \bar{\lambda}^* \pm \delta. \quad \begin{array}{l} + \\ - \end{array} \text{ for } -(r_1 + r_2)\sin\alpha + s\sin\gamma \begin{array}{l} \geq 0 \\ \leq 0 \end{array}$$

## MOTION IN LATITUDE

They perform a digression in latitude made up of two components. Since the circumferences of the epicycles remain in the same plane,[a] they incline from [the plane of] the ecliptic along with their sphere by an amount equal to the inclinations of axes [which] cannot be carried around as in the case of the moon, but always incline toward the same place in the heavens. Consequently the intersections of the circles of the sphere and of the ecliptic, which are called the nodes, retain permanent positions with respect to the fixed stars. Thus, Saturn keeps the node from which it begins to ascend to the north $8\frac{1}{2}°$ east of the star said to be the eastern star in the head of Gemini, Jupiter 4° west of the same star, and Mars $6\frac{1}{2}°$ west of *Vergiliae*.

Now when the planet is in these nodes and the nodes located diametrically opposite, it has no latitude, but its maximum latitude, which takes place at quadratures to the nodes, is very irregular. For the inclination of the axes and the circles seems unsteady,[b] as though it were pivoted upon the nodes. Indeed the maximum [inclination] occurs when the earth is closest to the planet, that is, at the evening rising of the planet, for then in the case of Saturn the axis is inclined $2\frac{2}{3}°$, in the case of Jupiter $1\frac{2}{3}°$, and in the case of Mars $1\frac{5}{6}°$.[c] On the other hand, near evening setting and morning rising, when the earth is most distant, this inclination is less, in the case of Saturn and Jupiter by 5/12°, and in the case of Mars by $1\frac{2}{3}°$. Thus, this irregularity, which increases and decreases uniformly with the latitude, is observed especially in maximum latitudes, and elsewhere,[d] the closer the planet is to the node the smaller it becomes.

It also happens that the latitudes are varied to our observations owing to the motion of the earth in the great sphere so that, as expected, the closeness or distance [of the earth to the planet] increases or decreases the angles of the apparent latitude as a mathematical proportion requires.[e]

If indeed this motion of libration takes place in a straight line,[f] it is still possible that such a motion be composed from two spheres. Since these are concentric, [the higher] one carries around the inclined poles of the other, and the lower revolves the poles of the sphere carrying the epicycles in the direction opposite to [the motion of] the higher sphere with twice the velocity. And the poles [of the sphere carrying the epicycles] are inclined from the poles of the next higher sphere by the same amount that the poles of this sphere are inclined from the poles of the highest sphere.

And this [is the description] of Saturn, Jupiter, and Mars, and of the spheres surrounding [the sphere of] the earth.

[a] 197:5 permanentibus, cum
[b] 197:17 *instabilis* videtur (SV instare videtur)
[c] 197:21 parte una et *dextante* (SV sextante)
[d] 197:25 ac *alibi* (SV aliqui)
[e] 197:30 exposcit. Siquidem
[f] 197:31 contingit, fieri autem

While Copernicus's representation of the second anomaly is a great success, and his representation of the first anomaly at least reasonable, his adaptation of Ptolemy's model for latitudes to a heliocentric arrangement is highly implausible and even ends up violating the principle of uniform circular motion. Ptolemy's latitude model in the *Almagest* is described at length in Peurbach's *Theoricae novae planetarum* and, of course, in Book XIII of the *Epitome*. Ptolemy

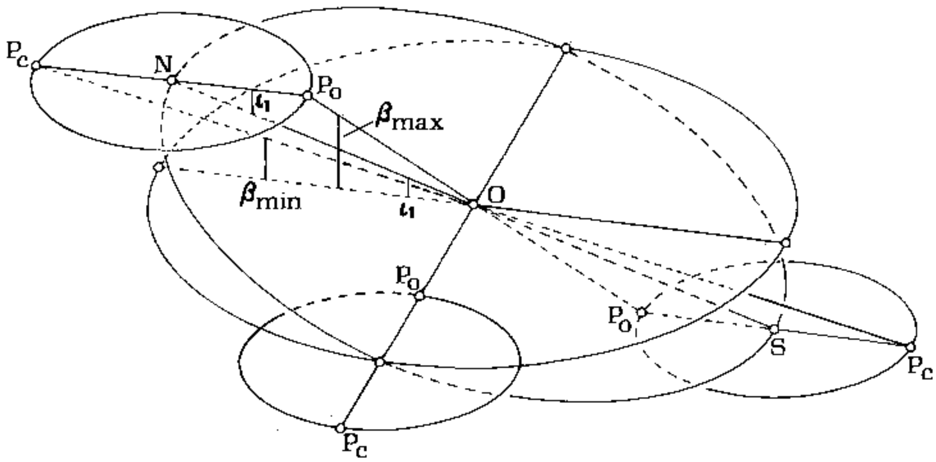


Fig. 27

considerably altered his latitude theory in the *Handy Tables* and *Planetary Hypotheses*, but these were unknown to Copernicus. The models for latitude are so artificial that they make the much maligned *Ad lectorem* in *De revolutionibus* seem altogether justified.[13] In Book VI of *De revolutionibus* Copernicus drops all pretense of deriving parameters from observations, and directly extracts from Ptolemy the required parameters for his own model. He does the same thing in the *Commentariolus*, but the conversion of parameters is carried out with less precision.

The apparent motions in latitude underlying Ptolemy's model are as follows:

1. When the center of the epicycle is at two diametrically opposite points of true ecliptic longitude the planet has no latitude no matter where it is on the epicycle. These points, the nodes, define the intersections of the plane of the planet's eccentric with the plane of the ecliptic, and the nodal line passes through the center of the earth.

2. When the center of the epicycle is 90° from the nodes, the maximum possible latitudes occur, but the quantities of these latitudes depend upon the position of the planet on the epicycle.

3. Apparent latitudes are greater at opposition, when the planet is at the perigee of the epicycle, than near conjunction, when it is near the apogee, no matter where the epicycle is on the eccentric.

These conditions suggest the following preliminary model (fig. 27): The earth at $O$ is on the line in which the plane $NS$ of the planet's eccentric intersects the plane of the ecliptic at an angle $\iota_1$. We place the center of the epicycle at the northern limit $N$ and the southern limit $S$, and let the diameter of the epicycle $P_cP_o$ passing through its apogee and perigee be inclined to $NS$ by angle $\iota_1$ so that $P_cP_o$ is parallel to the ecliptic and will remain so as the center of the epicycle moves along plane $NS$. Therefore, when the center of the epicycle is in the nodal line passing through $O$, $P_cP_o$ will lie in the plane of the ecliptic, and the planet will have no latitude. Now, the planet at $P_c$ is in true conjunction, and therefore at the apogee of the epicycle, and the planet at $P_o$ is in true opposition and therefore at the perigee of the epicycle. Since $P_cP_o$ is always parallel to the plane of the ecliptic, and $OP_c > OP_o$, $P_o$ will always be seen at a greater latitude $\beta$ than $P_c$. When the center of the epicycle is at $N$ or $S$, $P_o$ will have the maximum latitude at $N$ or $S$, $\beta_{max}$, which is the greatest possible latitude, and $P_c$ the least latitude at $N$ or $S$, $\beta_{min}$.

This model would fulfill the three stated conditions. However, Ptolemy reports two further conditions that necessitate altering the model:

[13] Yet one must admit that the models of motion in latitude, for what they are worth, are entirely his own invention, and since they are heliocentric they can have no relation to the models of the Marāgha astronomers (Roberts, 1966). While Ibn ash-Shāṭir's representation of the first anomaly can be taken directly into heliocentric theory, totally different latitude models are required by the conversion from geocentric to heliocentric theory since a direct conversion of geocentric latitude models would require the plane of the ecliptic to oscillate, which is impossible.

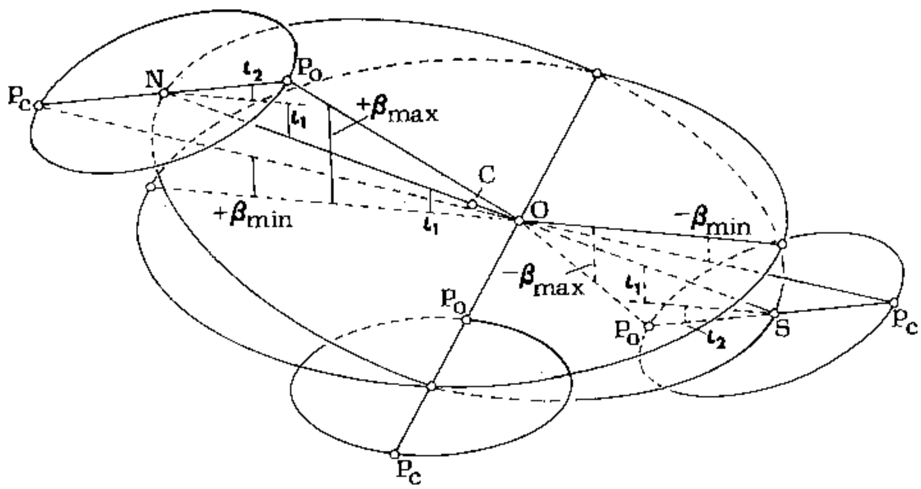


Fig. 28

4. In the case of Mars, the maximum latitudes are noticeably greater when the center of the epicycle is near perigee of the eccentric than near apogee. This is not noticeable in Jupiter and Saturn.

5. The apparent latitudes when the planet is in $P_o$ are always greater, and the apparent latitudes when the planet is in $P_c$ are always less, than the parallelism of $P_cP_o$ to the ecliptic permits.

The complete model is shown in figure 28. The center of the eccentric is $C$ located in the direction of $N$. Since $CN = CS$, thus $ON > OS$, and the observed latitudes will be greater at $S$ than at $N$. The length of $OC$ depends upon the distance of $N$ from the apogee of the eccentric. The apogee of Mars is exactly at $N$, so $OC = e$, and since Mars has a large eccentricity, the difference in latitude will be considerable. For Saturn $\lambda_N = \lambda_A - 50°$, for Jupiter $\lambda_N = \lambda_A + 20°$, so $OC < e$, and since $e$ is not large, the difference in latitude will be small. This accounts for the fourth condition.

To account for the larger latitudes near opposition and smaller latitudes near conjunction than are permitted by the assumption that the plane of the epicycle remains parallel to the plane of the ecliptic, we assume that when the epicycle is at $N$ or $S$, it is inclined to the plane of the ecliptic by angle $\iota_2$, so it is inclined to the plane of the eccentric by angle $(\iota_1 + \iota_2)$. It is obvious that the apparent latitude $\beta$ of $P_o$ will be larger and of $P_c$ smaller than if $P_cP_o$ were parallel to the ecliptic. As the center of the epicycle approaches the node, $\iota_2$ changes by $\iota_2 \cos \omega$, where $\omega$ is the distance from $N$. Thus, when the center of the epicycle is in the node, $\iota_2 \cos \omega = 0°$, so that $P_cP_o$ coincides with the ecliptic. All five conditions are now accounted for.

Copernicus wishes to find a model that will produce the same apparent latitudes and meet the same five conditions as Ptolemy's, but under the assumption that the earth and planets move about the mean sun as the center of their spheres. In figure 29, we show his model with the planet $P$ at the northern limit of latitude. The mean sun is at $\bar{S}$, and the earth is at $O_c$ when $P$ is at conjunction and in $O_o$ at opposition. In place of the two epicycles in the model for longitude, we assume for simplicity that the planet moves on an eccentric with center $C$ removed from $\bar{S}$ by $\bar{S}C$, the component of the eccentricity in the direction of the northern limit. Therefore, the maximum northern latitudes, near aphelion, will be smaller than the maximum southern latitudes, near perihelion (and not shown). Now if, as in figure 27, the plane of the epicycle remained parallel to the ecliptic, it would be sufficient to place the planet at $P$ in a plane inclined to the

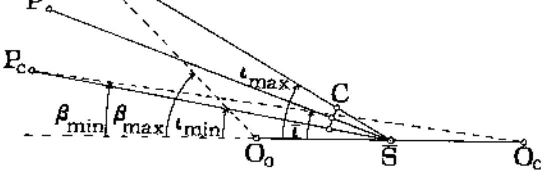


Fig. 29

plane of the ecliptic at a fixed angle $\bar{\iota}$ equal to $\iota_1$ in the epicyclic model. But we are given at opposition $\beta_{max}$ too large, and at conjunction $\beta_{min}$ too small, to result from $\bar{\iota}$. Therefore, the inclination of the eccentric to the ecliptic must vary. Since the variation corresponds to each planet's synodic period, the plane of the ecliptic cannot be moving, for it could not move simultaneously through three different arcs and periods. So the orbital plane of each superior planet must oscillate such that when $P$ is in opposition to $\bar{S}$, the inclination is $\iota_{max}$ and the planet is at $P_0$; but when in conjunction with $\bar{S}$, the inclination is $\iota_{min}$, and the planet is at $P_c$. This will produce the required difference between $\beta_{max}$ and $\beta_{min}$, and thus all five conditions accounted for by Ptolemy's model are fulfilled. The inclination $\iota$ at any time is given by

$$\iota = \iota_{min} + \tfrac{1}{2}(\iota_{max} - \iota_{min})(1 - \cos \gamma'),$$

where $\gamma'$ is the true anomaly measured from true conjunction with the mean sun.

Copernicus retains this model without alteration in *De revolutionibus*, never expressing any doubts about its plausibility, yet he must have wondered why the orbital planes of the superior planets should possess an oscillation corresponding to the relative positions of the planet and the earth. The oscillation of the plane of the epicycle through angle $\iota_2$ in Ptolemy's model (fig. 28) depends upon the distance of the center of the epicycle from the node, so $\iota_2$ varies from zero to its maximum value twice in the planet's longitudinal period. The inclination of $P_cP_o$ effected by $\iota_2$ produces the decreased latitudes at conjunction and increased latitudes at opposition required in each synodic period of the planet. But in order to produce the same result, Copernicus requires that the entire orbital plane oscillate from minimum to maximum to minimum inclination between any two conjunctions defining the planet's synodic period. Why should the planes of the planetary orbits have such a relation to the motion of the earth? Kepler, armed by his incredulity against this monstrosity, showed that the error lay in the assumption that the nodal line passed through the mean sun; if it were made to pass through the true sun, the plane of the orbit would remain fixed no matter where the earth and planet were located.

It is in this connection that Kepler makes the remark, "Copernicus, who was unaware of his own riches, generally chose to represent Ptolemy rather than nature, to which he had nevertheless come closer than anyone else."[14] This is a just estimate of Copernicus's work. But could Copernicus represent nature? For all his talk of how such and such a model or parameter or computation "agrees with observations," Copernicus had few observations and could use fewer. The handful cited in *De revolutionibus* required manipulation to force out Ptolemy's parameters; by themselves some led to impossible results. He was also impeded by his lack of technical proficiency, and even had great difficulty representing Ptolemy. As Viète says with complete truth, "Copernicus, an unfortunate computer, was a still more unfortunate geometer, and so failed to do what Ptolemy failed to do, but made even more mistakes."[15] Even if Copernicus knew what nature showed, he could not have chosen to represent it for sheer lack of mathematical originality. It was fortunate that he had Ptolemy to represent; otherwise he would have had nothing.

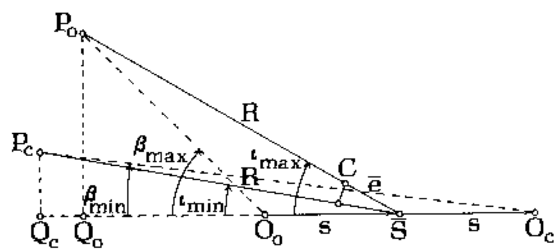


Fig. 30

The inclinations of the planes of the planetary orbits may be computed either from observations or latitude tables in this way (see fig. 30): We are given the maximum and minimum latitudes $\beta_{max}$ and $\beta_{min}$ at opposition and conjunction when the planet is at the northern or southern limit. We assume in the figure that the aphelion is north of the ecliptic, and we represent the component of the eccentricity in the direction of the northern limit by $\bar{e} = \bar{S}C$. The distance of the planet from the mean sun is therefore $\bar{S}P = R + \bar{e}$, and the distance of the earth from the mean sun $\bar{S}O$ is $s$. Let fall perpendiculars to the ecliptic $P_cQ_c$ and $P_oQ_o$. Since in no case will a

[14] *Astronomia nova* xiv (*Werke* 3: p. 141).

[15] Apollonius Gallus, Appendicula II (*Opera mathematica*, 1646: p. 343). While Viète has no high opinion of anyone, his criticisms of Copernicus, which always refer to his abilities as a mathematician rather than to the heliocentric theory, are by far the sharpest.

maximum inclination $\iota_{\max}$ exceed 3°, we may assume without error that

$$Q_c\bar{S} = Q_o\bar{S} = R + \bar{e}.$$

Now,

$$P_oQ_o = \tan\beta_{\max}Q_oO_o = \tan\beta_{\max}(R + \bar{e} - s)$$

and

$$P_cO_c = \tan\beta_{\min}Q_cO_c = \tan\beta_{\min}(R + \bar{e} + s).$$

Therefore

$$\tan\iota_{\max} = \tan\beta_{\max}\frac{R + \bar{e} - s}{R + \bar{e}}$$

and

$$\tan\iota_{\min} = \tan\beta_{\min}\frac{R + \bar{e} + s}{R + \bar{e}}.$$

But since the angles are so small, it is sufficient to use

$$\iota_{\max} = \beta_{\max}\frac{R + \bar{e} - s}{R + \bar{e}}$$

and

$$\iota_{\min} = \beta_{\min}\frac{R + \bar{e} + s}{R + \bar{e}}.$$

Then, if we wish also to check the southern latitudes, we use

$$\iota_{\max} = -\beta_{\max}\frac{R - \bar{e} - s}{R - \bar{e}}$$

and

$$\iota_{\min} = -\beta_{\min}\frac{R - \bar{e} + s}{R - \bar{e}}.$$

In the case of Jupiter and Saturn $\bar{e}$ produces so little effect on the latitudes that we may omit it, and compute the inclination from both northern and southern limits by

$$\iota_{\max} = \pm\beta_{\max}\frac{R - s}{R}$$

and

$$\iota_{\min} = \pm\beta_{\min}\frac{R + s}{R}.$$

Copernicus gives the following values for maximum and minimum inclination:

Saturn $\iota_{\max} = 2;40°$ $\iota_{\min} = 2;40° - 0;25° = 2;15°$

Jupiter $\iota_{\max} = 1;40°$ $\iota_{\min} = 1;40° - 0;25° = 1;15°$

Mars $\iota_{\max} = 1;50°$ $\iota_{\min} = 1;50° - 1;40° = 0;10°$

The 1;50° for Mars depends upon a textual emendation of *sextante* to *dextante*. I do not know how precisely these round numbers represent the inclinations Copernicus derived by computation.

Now, where do we look for the maximum and minimum apparent latitudes Copernicus used to find the inclinations? One possibility is *Epitome* XIII, 5 and 6 which contains Ptolemy's values, although with an error for Mars. Another is, of course, the tables of latitude in the *Alphonsine Tables*, which, aside from some variants or errors near extremal values, are the same as the *Almagest* tables. A third possibility is a large set of latitude tables written out by Copernicus on some of the pages bound in with U (Prowe, 2: pp. 231–238). These have extremal values differing slightly from both the *Alphonsine* and *Almagest* tables, and are written at 1° intervals. They are almost identical (only two extremal values differ) to the latitude tables in the so-called *Tabula resoluta*, a set of tables computed on the basis of the *Alphonsine Tables* in the late fourteenth or early fifteenth century and published by Johann Schoener at Nuremberg in 1536. While I do not know the source of the tables in Copernicus's notes, I assume that they were not computed, but copied from some earlier latitude tables.[16]

I have computed the inclinations from all of these, and in all cases the agreement with the *Commentariolus* is fair—never bad, never perfect—so there is no reason to choose one rather than another. Here, I give the inclinations from the extremal latitudes in *Epitome* XIII, 5 and 6, and from the tables copied by Copernicus. For Saturn and Jupiter, I have ignored the component of eccentricity in the direction of the northern limit, and rounded $R$ Saturn to 231 and $R$ Jupiter to 130 where $s = 25$. In the case of Mars $R = 38$, and since the northern limit is at the apogee, $\bar{e} = r_1 - r_2 = 5;34 - [1];51 = 3;43$. For each planet I give first the extreme latitudes $\beta$ from the *Epitome* and Copernicus's tables, and

[16] It is, of course, possible that he copied them after he wrote the *Commentariolus* so perhaps they should not be considered here. But since the other large tables on the pages with U, those copied from Peurbach's *Eclipse Tables*, were probably copied before they were printed in 1514, these latitude tables are probably equally early.

then the resulting inclination $\iota$:

| | Saturn $\beta$ | Saturn $\iota$ | Jupiter $\beta$ | Jupiter $\iota$ | Mars $\beta$ | Mars $\iota$ |
|---|---|---|---|---|---|---|
| *Epitome* | | | | | +max | |
| max | 3° | 2;41° | 2° | 1;37° | 4;15° | 1;42° |
| | | | | | −max | |
| min | 2° | 2;13° | 1° | 1;12° | 7° | 1;54° |
| Tables | | | | | | |
| +max | 3;3° | 2;43° | 2;5° | 1;41° | 4;21° | 1;44° |
| +min | 2;2° | 2;15° | 1;6° | 1;19° | 0;5° | 0;8° |
| −max | 3;5° | 2;45° | 2;8° | 1;43° | 7;2° | 1;54° |
| −min | 2;1° | 2;14° | 1;4° | 1;18° | 0;2° | 0;4° |

The agreement with Copernicus's values is fair or even good in the case of Saturn and Jupiter, depending upon how closely one wants to trust the precision of the simple fractions in the *Commentariolus*. The inclinations for Mars, on the other hand, are chaos, giving no clear values at all. Emending the erroneous *sextante* in the text to something other than *dextante* would not help. Copernicus computes Mars $\iota_{max}$ = 1;51° in *De rev.* VI, 3, but in order to do so, he first makes up values of $\pm\beta_{max}$ that are based upon nothing except producing the required inclination. In the *Planetary Hypotheses* Ptolemy sets the inclination of Mars's orbit at 1;50°, and in the *Astronomia nova* Kepler shows that the correct fixed inclination is in fact 1;50°, but I cannot see what these have to do with Copernicus's identical result for $\iota_{max}$.

Copernicus places the ascending nodes as follows:

| | |
|---|---|
| Saturn | 8½° east of Pollux |
| Jupiter | 4° west of Pollux |
| Mars | 6½° west of Vergiliae |

The *Vergiliae* are the Pleiades, and it is not certain which of the four stars in the group listed in the Alphonsine catalog Copernicus intends by this vague designation. I would guess that it is the most western, 16 Tauri, but the most eastern, $\eta$ Tauri is only 1;30° further east. Now, according to Ptolemy and the *Alphonsine Tables*, the longitudes of the northern limits are

| | |
|---|---|
| Saturn | $\lambda_N = \lambda_A - 50°$, |
| Jupiter | $\lambda_N = \lambda_A + 20°$, |
| Mars | $\lambda_N = \lambda_A$. |

Therefore, the ascending nodes, $\lambda_0 = \lambda_N - 90°$, will be

| | |
|---|---|
| Saturn | $\lambda_0 = \lambda_A - 140°$, |
| Jupiter | $\lambda_0 = \lambda_A - 70°$, |
| Mars | $\lambda_0 = \lambda_A - 90°$. |

Taking the tropical longitudes of the apogees for Era Alfonso from the *Alphonsine Tables*, we find the nodes at

| | |
|---|---|
| Saturn | $\lambda_0$ = 4,10;39° − 2,20° = 1,50;39°, |
| Jupiter | $\lambda_0$ = 2,50;53° − 1,10° = 1,40;53°, |
| Mars | $\lambda_0$ = 2,12;28° − 1,30° = 42;28°. |

Thus,

$\lambda_0$ Saturn − $\lambda$ Pollux
= 1,50;39° − 1,43;48° = 6;51° east,

$\lambda$ Pollux − $\lambda_0$ Jupiter
= 1,43;48° − 1,40;53° = 2;55° west,

$\lambda$ 16 Tauri − $\lambda_0$ Mars
= 49;18° − 42;28° = 6;50° west.

Saturn is 1;39° west of Copernicus's value, Jupiter 1;5° east, and Mars 0;20° west. The differences are about the same order of magnitude as the differences in the longitudes of the apsidal lines except that the 30° error for Saturn has disappeared, for the node is about where it should be. This indicates that Copernicus once had Saturn's apsidal line in the right place, and made his error after he had correctly located the node. Just as I had no explanation of the small differences in the apsidal lines, so I have no explanation of these small differences in the nodes. However, everything is so close to its value in the *Alphonsine Tables* that Copernicus must have used them in some way for all apsidal lines and nodes.

In order to account for the libration of the orbital planes, Copernicus takes up one of the two devices for the generation of a rectilinear motion from two circular motions originally used, and indeed invented, by Nasir ad-Dīn aṭ-Ṭūsī, and used extensively by Ibn ash-Shāṭir and other of the Marāgha astronomers. The same mechanism was also used in a work published in 1536 by Giovanni Battista Amico for the construction of homocentric sphere models of planetary mo-

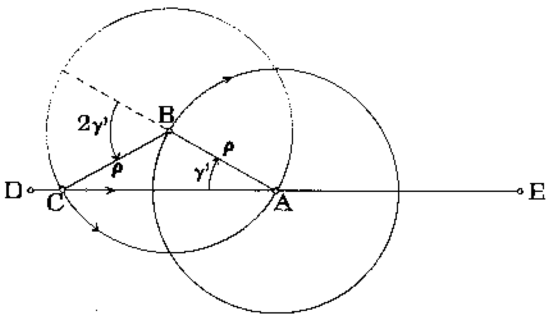


Fig. 31

tion both in longitude and latitude.[17] The device when represented in a plane is exactly like the two epicycles in the planetary model except that the radii of both are equal. It was shown earlier that the two epicycles generate an ellipse of semimajor axis $r_1 + r_2$ and semiminor axis $r_1 - r_2$. If $r_1 = r_2$, obviously $r_1 - r_2 = 0$, and the ellipse degenerates into a straight line of half-length $r_1 + r_2 = 2r_1$.[18]

The device is shown in figure 31. Copernicus, like Amico, envisions these motions as the rotation of spheres. $A$ is the pole of the higher sphere which carries at a distance $\rho = AB$ the pole $B$ of a lower sphere through angle $\gamma' = \bar{\lambda}_\odot - \lambda$ of the true anomaly of a superior planet. $B$ in turn carries at the same distance $\rho = BC = AB$ the pole $C$ of the sphere of the planet in the opposite direction through $2\gamma'$. $B$ and $C$ will describe small circles around poles $A$ and $B$. It is assumed that the result of all this is that $C$ librates on a great circle over arc $DE = 4\rho$. This is absolutely correct in a plane where $DE$ is a straight line, but is correct on a sphere only if $\rho$ is small. The largest libration in latitude, for Mars, is 1;40°, so $\rho$ is only 0;25° and this is sufficiently small. The motion of $C$ is given by

$$AC = 2\rho \cos\gamma'$$

or by

$$DC = 2\rho(1 - \cos\gamma').$$

Now Copernicus holds that this device is

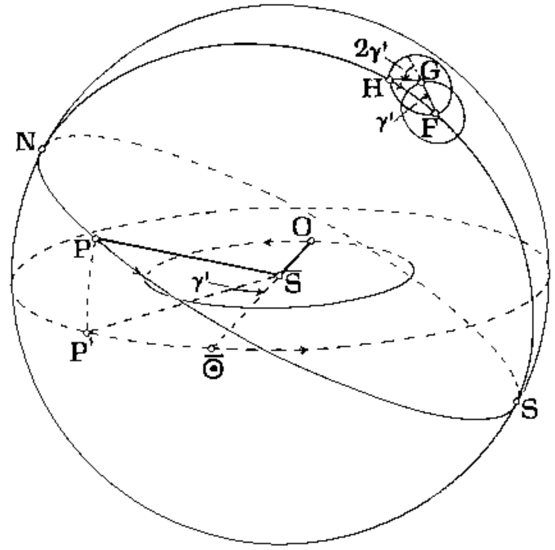


Fig. 32

located at the pole of the planet's sphere—evidently the outer solid sphere concentric to $\bar{S}$ shown in figure 12—and causes pole of the sphere to librate in the plane of the great circle passing through the northern and southern limits. A simple picture of the model is shown in figure 32. The earth is at $O$, the mean sun at $\bar{S}$ is projected on to the planet's sphere at $\bar{\odot}$, and the planet at $P$ is projected onto the ecliptic at $P'$. The libration is supposedly controlled by two yet higher spheres concentric to $\bar{S}$. The higher, with pole $F$, apparently revolves through $\gamma'$, and carries the pole $G$ of the lower. $G$ in turn revolves in the opposite direction through $2\gamma'$, and carries $H$, the pole of the planet's sphere. This will make $H$ librate on either side of $F$, and carry $N$ and $S$ up and down. But something is wrong. It is required by the computation of apparent latitudes that the libration of the orbital plane be a function of the true anomaly $\gamma'$, so the higher spheres are turning through $\gamma'$ and $2\gamma'$. But $\gamma'$ is not uniform, so they cannot do this without violating the principle of uniform circular motion. There is no way out of this difficulty, and therefore Copernicus passes over it in silence both in the *Commentariolus* and *De revolutionibus*. Nevertheless, since the motion in latitude of each planet requires this device, he has violated uniform circular motion for all three superior planets.

[17] Swerdlow, 1972, who obviously had given no serious attention to the *Commentariolus* when he carelessly said that Copernicus does not use the other Marāgha device, for he in fact uses it for Mercury (see below, p. 504).

[18] It is this property of the libration mechanism that Copernicus refers to in the canceled passage about the ellipse on f. 75r of his manuscript (Copernicus, 1944).

## 7. VENUS

### MOTION IN LONGITUDE

It remains to explain the theory of the planets which are surrounded by the circumference of the great sphere, that is, the theory of the motions of Venus and Mercury. Venus has a combination of circles very much like the superior planets, but with a different rule for [their] motions. The sphere along with its larger epicycle completes equal revolutions in the ninth month, as was stated previously, and by this composite motion, returns the smaller epicycle to the same point in a relation everywhere fixed with respect to the sphere of the fixed stars, and fixes the highest apsis of Venus at the point toward which we have said that the sun is removed [from the center of the great sphere]. The smaller epicycle, however, which has revolutions incommensurable with these, has maintained a commensurability[a] [of revolutions] in relation to the motion of the great sphere. Indeed it completes fully two revolutions for one revolution of the great sphere so arranged that whenever the earth is in the line produced from the apsidal line, the planet is then closest to the center of the larger epicycle, and when the earth is in the perpendicular through the quadrants [from the apsis], the planet is most distant, in about the same way that, in the case of the moon, the observant smaller epicycle looks at the sun. The proportion of the semidiameters of the great sphere and the sphere of Venus is as 25 to 18, and the larger epicycle receives $\frac{3}{4}$ of one part, and the smaller epicycle $\frac{1}{4}$.

[a] 198:14 *paritatem* reservavit (SV imparitatem)

In Ptolemy's model for Venus the center of the epicycle always lies in the direction of the mean sun, its mean motion in longitude is $\bar{\lambda}_{\odot}$, and its distance from the apogee $\alpha = \bar{\lambda}_{\odot} - \lambda_A$. The equation of center $\delta_1$ is really the equation of center of the earth moving about the true sun on an eccentric with a bisected eccentricity, and this receives confirmation from the double eccentricity of Venus, which is equal to the simple eccentricity of the sun. One generally finds in later planetary theory that the double eccentricity of Venus in an equant model or the equivalent is equal to the simple eccentricity of the sun. Examples are al-Battānī, the *Toledan Tables*, the *Alphonsine Tables*, and Ibn ash-Shāṭir.[1] This is also true in Indian planetary theory, for in the *Ārdharātrika* system the manda equations of the sun and Venus are both 2;14°, and these are carried over unchanged into al-Khwārizmī's tables.[2] Ptolemy found the apogee of the sun tropically fixed at Gemini 5;30°, and found the tropical longitude of the apogee of Venus, which he assumed to be sidereally fixed, at Taurus 25° in his own time. Later treatises generally have the apogees of the sun and Venus coincident and sidereally fixed. All the sources cited for the identical equations, including the Ārdharātrika system but excluding al-Khwārizmī, have this arrangement.[3]

Since the equations and apogees of Venus and the sun are identical in the *Alphonsine Tables*, Copernicus assumes the same in the *Commentariolus*, although he does not do so in *De revolutionibus*. The center of the epicycle moves with

[1] Nallino, 2: pp. 78–83, 126–131; Toomer, 1968: p. 65; Roberts, 1957: p. 430; Kennedy and Roberts, 1959: p. 230.

[2] Pingree, 1970; Suter, 1914: pp. 132–137, 156–161.

[3] Battānī, cap. 45; Nallino 1: p. 114; Khwārizmī, cap. 18; Suter, 1914: pp. 14–15; Neugebauer, 1962₂: p. 41; Neugebauer and Pingree, 1971: 2: p. 102.

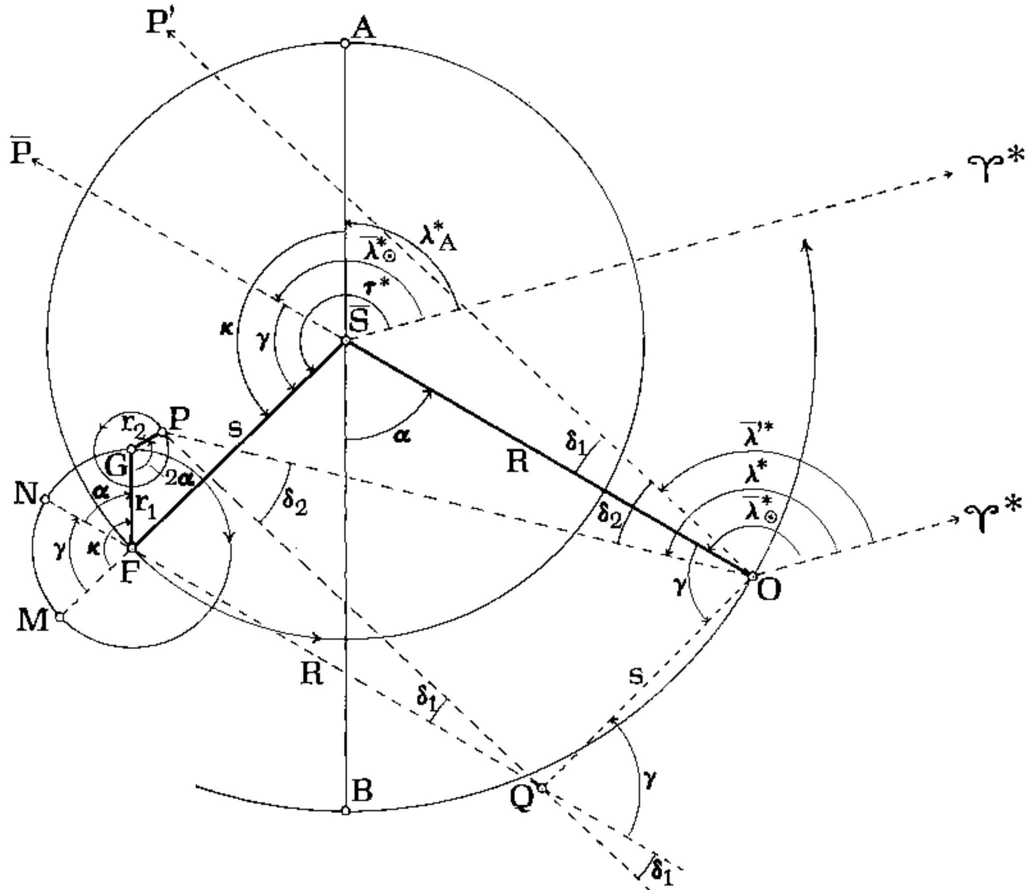


Fig. 33

the mean sun in Ptolemy's model, so the equation of center is naturally a function of the distance of the sun from the apsidal line. In order to produce the same result in the heliocentric model, Copernicus must have the equation of center depend upon the distance of the earth from Venus's apsidal line. This is remotely plausible in the *Commentariolus* where the apsidal lines and eccentricities of the earth and Venus coincide, but hardly so in *De revolutionibus* where they do not.

The model for Venus is shown in figure 33. The mean sun is at $\bar{S}$, and the earth is at $O$, having moved the distance $\alpha = \bar{\lambda}^*_\odot - \lambda^*_A$ from the apsidal line on a circle of radius $R$. As seen from $O$, $\bar{S}$ is at the longitude $\bar{\lambda}^*_\odot$ measured from ♈*. The center $F$ of Venus's first epicycle moves about $\bar{S}$ on a circle of radius $s$ through angle $\gamma$ of the mean anomaly when measured from the direction $O\bar{S}\bar{P}$ (as in Ptolemy's model), but through $\tau^* = \bar{\lambda}^*_\odot + \gamma$, Venus's mean heliocentric motion, when measured from ♈*. Measured from $A$, $F$ has moved through $\kappa = \tau^* - \lambda^*_A$, and the center $G$ of the second epicycle has been caried in the opposite direction through $\kappa$ measured from the direction $\bar{S}FM$, so $r_1 = FG$ remains parallel to the apsidal line. Now complete parallelogram $O\bar{S}FQ$, and extend $QF$ to $N$. We see that with respect to $QFN$, $r_1$ has moved through $\alpha = \kappa - \gamma$. Venus itself is at $P$, moving on the second epicycle of radius $r_2$ in the direction opposite to the motion of $r_1$ through $2\alpha$. $P$ will in fact describe an ellipse about $F$ with its semiminor axis $r_1 - r_2$ on $NFQ$. The equation of center $\delta_1 = FQP$ is thus purely a function of $\alpha$ and is totally independent of $\tau^*$. It is given, just as for the superior planets, by

$$\tan \delta_1 = \frac{|(r_1 + r_2) \sin \alpha|}{R + (r_1 - r_2) \cos \alpha}.$$

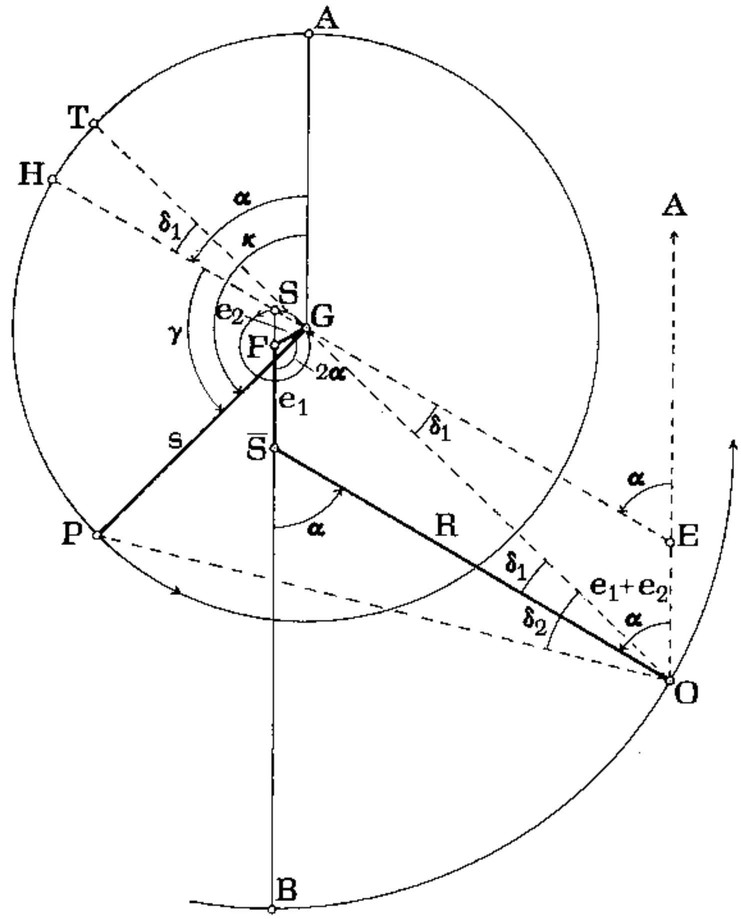


Fig. 34

We draw $OP'$ parallel to $QP$, and form

$$\bar{\lambda}'^* = \bar{\lambda}^* \pm \delta_1$$

and

$$\gamma' = \gamma \pm \delta_1,$$

using the rules for the sign of $\delta_1$ given for the superior planets. The remainder of the computation of $\lambda^*$ is also identical to that of the superior planets, and need not be repeated here. It is worth noting that here too the entire operation can be combined into one computation of $\delta = \lambda^* - \bar{\lambda}^*$ in the same way shown earlier.

It is instructive to convert the double epicycle model into the equivalent eccentric model used by Copernicus in *De revolutionibus* (fig. 34). Here the equation of center, very obviously a function of $\alpha$, is brought about by an eccentricity $e_1 = r_1$ of fixed direction and a central equivalent of the second epicycle $e_2 = r_2$. Let $e_1 + e_2$ be $\bar{S}S$, complete parallelogram $O\bar{S}SE$, and $ES$ will pass through the center $G$ of Venus's circle. Relative to the apsidal direction $OA$, $G$ will move uniformly with respect to $E$, removed from $O$ in the direction of $A$ by the distance $OE = e_1 + e_2$. If $e_1 + e_2$ is equal to the eccentricity of the sun, and the apsidal lines of Venus and the earth coincide, as is the case in the *Commentariolus*, then $S$ is in fact the true sun with the earth moving around it on an eccentric with center $\bar{S}$. This is nice because it shows clearly that the equation of center for Venus is really the equation of center for the earth moving about the true sun on an eccentric with the equivalent of a bisected eccentricity.

The sines of the equations used to derive the parameters of Venus are not given in U, which contains only the final results as in the *Commentariolus*. It is, however, trivial to reconstruct their derivation. The maximum equation of center for Venus in the *Alphonsine Tables* is

2;10°, exactly that of the sun. Since Copernicus rounded the solar eccentricity to $e = 1$ where $R = 25$, so for Venus $r_1 + r_2 = 1$. Therefore, using the 3 to 1 division to produce the equivalent of a bisected eccentricity, $r_1 = \frac{3}{4}$ and $r_2 = \frac{1}{4}$. The maximum equation of the anomaly when the epicycle is at mean distance on the eccentric is 45;59° in the *Alphonsine Tables*. Thus, in figure 33 or 34 where $R = 10000$,

$$s = \sin 45;59° = 7191,$$

and where $R = 25$

$$s = \frac{7191}{10000} \cdot 25 = 17;58,39 \approx 18.$$

DESCRIPTION OF THE SECOND ANOMALY

Venus also is sometimes seen to move retrograde, and this [happens] especially when the planet is closest to the earth for a reason to some extent the same as for the superior planets, but the other way around. For in the case of the superior planets it happens because the motion of the earth exceeds [their own motion], while in the case of Venus the motion of the earth is exceeded [by the motion of Venus], and because the sphere of the earth is surrounded [by the spheres of the superior planets], while the sphere of the earth surrounds [the sphere of Venus]. And therefore Venus is never in opposition to the sun, since the earth cannot come between them, but turns back within fixed elongations on each side of the sun, never exceeding 48° for our sight, which occur at the points of tangency of the circumference [of its sphere] with lines extending from the center of the earth. And this is all there is to the motion by which[a] Venus is carried about in longitude.

[a] 199:9 *quo* in longitudine (S)

The description of Venus's second anomaly is clear enough, requiring no explanation. The only point worthy of mention is Copernicus's statement that the maximum elongation of Venus from the true sun never exceeds 48°. The general method of computing this is given in *Epitome* XII, 12 and 13 (= *Almagest* XII, 9). Since in the *Alphonsine Tables* the eccentricities and apsidal lines of Venus and the sun are coincident, the equations of center of Venus and the sun differ by less than 0;1°, and the centers of Venus's orbit and the true sun lie in the same direction from the earth. The problem is simplified, for the maximum true elongation becomes the maximum equation of the anomaly when the earth is at perihelion. This is 46;16°, not 48°. Ptolemy's table in *Almagest* XII, 10 gives a maximum evening elongation of 47;35° in Capricorn, and the table in the 1515 *Almagest* gives by error an elongation of 47;50° in Pisces (it should be 47;7°). If this is Copernicus's source (which I doubt), it affects the dating of the *Commentariolus*, but if 48° is only a rough guess nothing should be made of it.

There is, however, another source for the 48° maximum elongation, and this is al-Farghānī's elementary textbook of astronomy which had been published in Ferara in 1493 under the title *Brevis ac perutilis compilatio Alfragani astronomorum peritissimi, totum id continens quod ad rudimenta astronomica est opportunum*. At the end of chapter 15 Farghānī gives the maximum elongation of Venus as 48° and of Mercury as 28°, both values being rounded from Ptolemy's table. It is possible that this was Copernicus's source; it is also possible that Farghānī's values had been included in some commentary on Peurbach's *Theoricae novae* (there are a good many) that Copernicus had studied. Nothing else in the *Commentariolus* explicitly shows any use of Farghānī.

MOTION IN LATITUDE

Venus also ascends in latitude for a cause of two components. Now, it keeps the axis of its sphere inclined at an angle of $2\frac{1}{2}°$, and keeps the node from which it goes to the north in its apsis. However, although the inclination in itself is the same, the digression that results from this inclination is shown to us[a] in two ways. For when the earth moves into either of the nodes of Venus, [the digressions], called *reflections*, are viewed in crosswise directions upwards and downwards, but when the earth[b] is in the quadrants [from the nodes], the natural obliquities of the sphere are seen, and these are called *inclinations*. When, however, the earth is in other positions, both latitudes are mixed together in a disorderly way,[c] and the one which is larger defeats the other, and by their sameness or difference [in direction] they increase and cancel each other.

[a] 199:14 *nobis* ostenditur (SV non)
[b] 199:17 *eadem* (SV eaedem)
[c] 199:18 *permixte* confunduntur (SV permixtae)

Copernicus says that there are two components of Venus's motion in latitude when in fact there are three. He calls them the inclination (*declinatio*), reflection (*reflexio*), and deviation (*deviatio*). When the earth is 90° from the nodal line of Venus, the apparent latitude is the inclination, but when the earth is in the nodal line, the apparent latitude is the reflection. These require two different angles of intersection of the plane of Venus's orbit with the plane of the ecliptic. But Copernicus gives only one angle, 2;30°, which belongs to the inclination. He seems to believe that the different position of the earth, 90° from the nodal line or in the nodal line, is sufficient to explain the inclination and reflection without further slanting the plane of Venus's orbit. Hence he says there are two components to Venus's latitude—the inclination and reflection together are the first, and the deviation, which requires a motion of the orbital plane, is the second. This is confirmed by the enumeration of Venus's (and Mercury's) circles at the end of the *Commentariolus* since there are not enough circles to account for a motion of the orbital plane to distinguish the inclination and reflection. Copernicus may have thought that he had effected a simplification of Ptolemy's latitude theory by introducing the motion of the earth, or he simply may not have understood Ptolemy's theory. At any rate, he corrected his error in *De revolutionibus*, and gave Venus (and Mercury) a motion in latitude of three components, slanting the orbital plane from 2;30° to 3;30° to account for the inclination and reflection as the earth moves from 90° from the nodal line to the nodal line.

The component of latitude called *declinatio* (here translated *inclination*) corresponds to Ptolemy's *inclination* of the diameter of the epicycle passing through the true apogee and perigee of the epicycle. The *reflexio* (merely transliterated here as *reflection*), which in *De revolutionibus* Copernicus usually calls *obliquatio*, corresponds to Ptolemy's *slant* of the diameter of the epicycle passing through the mean distances of the epicycle, that is, through the points 90° from true apogee and perigee. The *deviatio* (transliterated as *deviation*) is a combined rotation and tilting of Venus's orbital plane corresponding to Ptolemy's variable inclination of the plane of Venus's eccentric.

Ptolemy reports the following conditions for the motion of Venus in latitude:

1. When the true distance of the center of the epicycle from the apogee of the eccentric is ±90°, Venus has a latitude at the apogee of the epicycle of 1° in one direction, a latitude at the perigee of 6;20° in the other direction, and is in the ecliptic at mean distances. At +90° the latitude at apogee is northern and at perigee southern; at −90° it is the reverse.

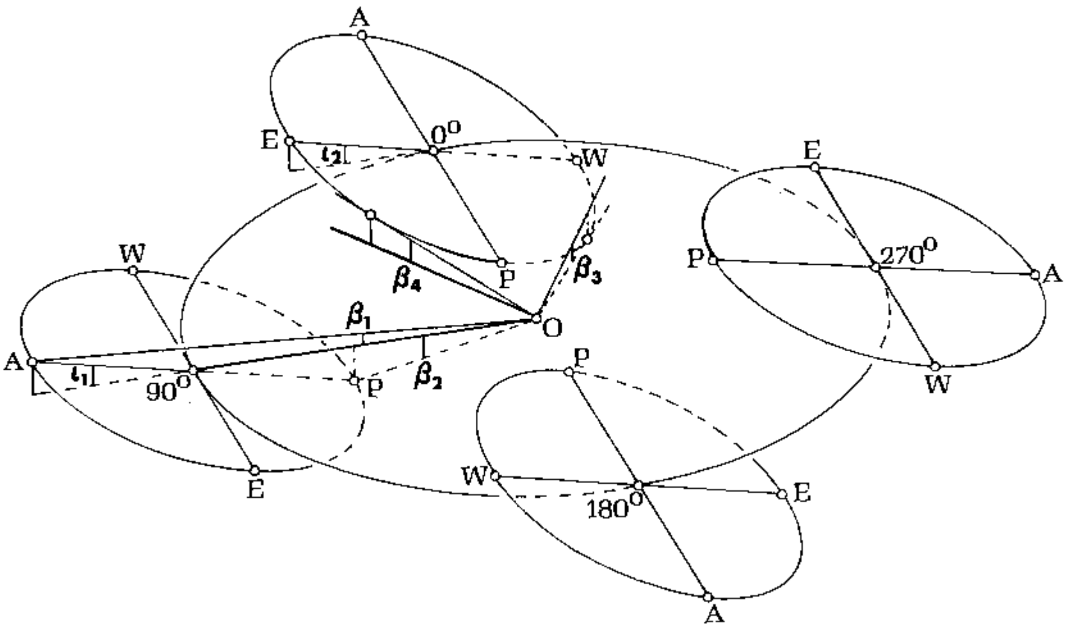


Fig. 35

2. When the center of the epicycle is at apogee or perigee of the eccentric, the planet has nearly equal maximum northern and southern latitudes of about $2\frac{1}{2}°$ at opposite maximum elongations. At apogee the eastern elongation is northern and the western is southern; at perigee it is the reverse.

3. When the center of the epicycle is at apogee or perigee of the eccentric, as in condition 2, and Venus is at apogee or perigee of the epicycle, it has a small northern latitude of 0;10°.

For the moment we shall leave out condition 3, and consider the model producing conditions 1 and 2. In figure 35 the earth is at *O*, and the center of the epicycle is shown at the apogee at 0°, and at the distances 90°, 180°, and 270° from the apogee. The eccentricity, which has no effect on the latitude, is neglected. The diameter of the epicycle passing through apogee and perigee is *AP*, and the diameter through mean distances is *EW*, *E* being east and *W* west. When the center of the epicycle is at 90° or 270° the diameter *AP* is inclined to the plane of the ecliptic by angle $\iota_1 = 2;30°$; this angle is the *inclination*. If the planet is at *A*, it will be seen at the small latitude $\beta_1$, if at *P* it will be seen at the large opposite latitude $\beta_2$, and if at *E* or *W* it will be in the plane of the ecliptic and have no latitude. Now move the center of the epicycle to 0° or 180° from the apogee. The diameter *AP* will lie in the plane of the ecliptic, and the diameter *EW* is inclined to the plane of the ecliptic by angle $\iota_2 = 3;30°$; this angle is the *slant*. When the planet is at greatest eastern or western elongation, it will have a latitude of $\beta_3 = \beta_4 = \pm 2;30°$, but when it is at *A* or *P* it will be in the plane of the ecliptic.

Now we must modify the last statement to take account of condition 3, the northern latitude of 0;10° at *A* and *P*. When the center of the epicycle is at 0°, the eccentric is inclined 0;10° such that the apogee has a northern latitude of 0;10°. This inclination decreases to zero as the center of the epicycle moves to 90° from the apogee, and then continues as the epicycle approaches 180°, where the eccentric is inclined 0;10° such that the perigee has a northern latitude of 0;10° and the apogee a southern latitude of 0;10°. Since the diameter of the epicycle *AP* lies in the plane of the eccentric when the epicycle is at 0° or 180°, the planet will have a northern latitude of 0;10° when at *A* or *P*. As the epicycle moves to 270°, the inclination again decreases to zero, and then continues, inclining the apogee of the eccentric 0;10° to the north when the epicycle arrives back at 0°.

Note that beginning at 90° from apogee with an inclination $\iota_1$, the slanting of the epicycle from $\iota_1$ to $\iota_2$ to $\iota_1$ to $\iota_2$ and back to $\iota_1$ takes one

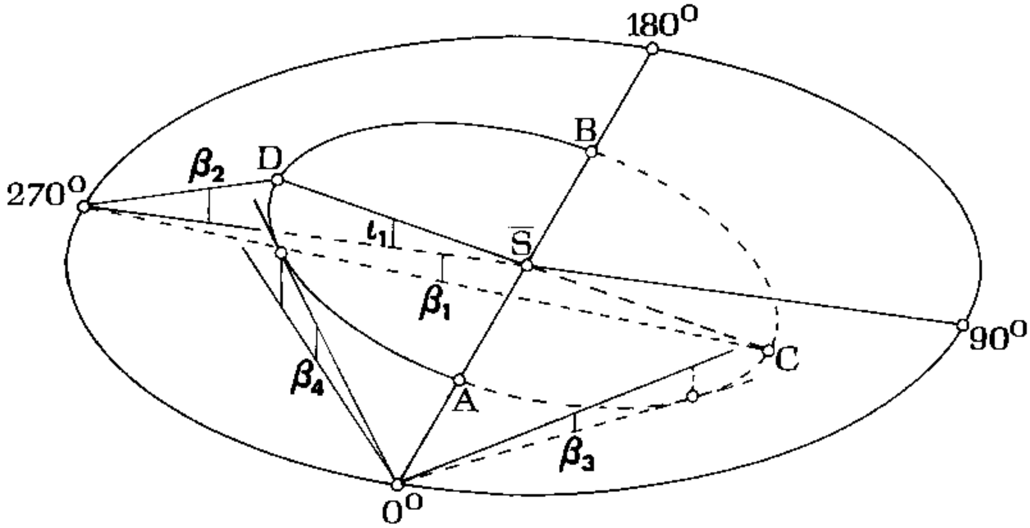


Fig. 36

year. Aside from this slant, the plane of the epicycle remains parallel to itself as it moves about the earth. Then note that the inclination of the plane of the eccentric, from 0;10° at apogee to zero at 90° to 0;10° at 180° to zero at 270°, and back to 0;10° at apogee, also takes one year. This, of course, is the longitudinal period of the center of the epicycle, so all changes of inclination are related to the same period and the model, despite its reputation, is really quite simple and logical.

That the epicycle remains more-or-less parallel to itself immediately suggests a simple heliocentric transformation. Copernicus's model, omitting for the moment the deviation, is shown in figure 36. The mean sun is at $\bar{S}$, the orbit of the earth is the outer circle, and the orbit of Venus is the inner circle which is inclined to the plane of the ecliptic. The nodal line $AB$ coincides with the apsidal line, and thus the diameter $CD$ perpendicular to the apsidal line is inclined to the ecliptic at the angle $\iota_1 = 2;30°$; this is the *inclination*. When the earth is 90° from the apsidal and nodal line, at 90° or 270° in the figure, $C$ and $D$ are seen at latitudes $\beta_1$ [$=1°$] and $\beta_2$ [$=6;20°$]. When the earth is in the apsidal and nodal line, at 0° or 180°, the greatest elongations are seen at latitudes $\beta_3 \approx \beta_4$ [$\approx 2;30°$]. Now in order to produce the proper value of $\beta_3 \approx \beta_4$, it is necessary to *slant* diameter $CD$ to $\iota_2 = 3;30°$; this is the *reflection*. Copernicus seems either to believe that the motion of the earth makes the slant unnecessary or simply to have forgotten to mention it. Note that if *eaedem* (Prowe, **2**: p. 199:17) is not emended to *eadem*, the text reads "the *same* natural obliquities," confirming the interpretation that Copernicus believed the slant unnecessary owing to the motion of the earth. With the emendation this interpretation is not as certain, leaving the possibility that he just forgot about the slant. As I mentioned before, Copernicus later caught his error, and restored the slant in *De revolutionibus* where it is called the *obliquatio*. However, whether it be through error or oversight the slant is definitely not accounted for in the *Commentariolus* since only one inclination for Venus's orbital plane is given, and there is no libration device to change the inclination $\iota_1$ to the slant $\iota_2$. This is confirmed by the number of circles given Venus (and Mercury) in the conclusion of the treatise.

The simple variable inclination of the plane of the eccentric in Ptolemy's model becomes the exceedingly curious *deviation* in Copernicus's heliocentric representation. Since the eccentric in Ptolemy's model becomes the orbit of the earth, which defines the plane of the ecliptic and therefore cannot be inclined, Copernicus must transfer this variable inclination to the orbital plane of Venus, with astonishing results. Here is Copernicus's description:

The inclination of the axis is as follows: It has a movable libration suspended, not at the nodes as in the case of the superior planets, but from certain other points that can revolve and make their own annual revolutions with respect to the planet. Consequently, whenever the earth is located facing the apsis of Venus, the maximum inclination due to the libration occurs, and this [maximum inclination occurs] in the planet itself whatever part of its sphere it is in at that time. Therefore, if the planet is then in the apsis or diametrically opposite to the apsis, it will not be entirely without latitude even though it is then in the nodes. From here, since this inclination decreases until the earth moves away from the apsidal line through a quadrant of a circle and, because the motions are equal, the point of maximum[a] deviation of this [inclination] has become distant[b] just as far from the planet, absolutely no trace of this deviation is found anywhere.[c] And since the downward motion of the deviation is continued[d] without interruption, and the initial [point of maximum deviation] inclines from the north to the south and continuously increases its distance from the planet by an amount equal to the motion of the earth away from the apsis, the planet is carried into the part [of its sphere] which previously had been south, but which now,[e] by the rule of opposition, has become north, until once again, upon completion of a semicircle[f] of the libration, [the planet] reaches the highest point, where the deviation again becomes maximum and at the same time equal to the original deviation. Then at last in the same way [the deviation] continues through the remaining semicircle. Therefore, this latitude, which is commonly called the *deviation*, never becomes southern. And in this case also, it seems suitable that these things are brought about by two concentric spheres with oblique axes, just as we explained for the superior planets.

[a] 200:1 *maximae* illius (SV maxime)
[b] 200:1 *distiterit* (S)
[c] 200:2 vestigium *usquam* (V usque, S omit)
[d] 200:3 deviationis libramento *continuato* (SV)
[e] 200:6 fuerat, nunc autem
[f] 200:8 librationis *semicirculo* (SV circulo)

The inclination of the plane of the eccentric in Ptolemy's model reaches a maximum of 0;10° north when the center of the epicycle is in the apsidal line. Since the plane of the eccentric passes through the center of the earth, this inclination will raise Venus 0;10° to the north as seen from the earth no matter where the planet is on the epicycle. For example, it may be at greatest elongation, which is either 2;30° north or 2;30° south; the inclination will change this to 2;40° north or 2;20° south. Therefore, in the heliocentric arrangement, when the earth is in Venus's apsidal line, Copernicus must tilt the orbital plane of Venus such that the *exact point* Venus is in will be raised 0;10° to the north *as seen from the earth*. But Venus may be any place in its orbit, and its distance from the earth varies from a maximum of about $25 + 18 = 43$ to a minimum of about $25 - 18 = 7$. Obviously if the orbit is tilted to give 0;10° of lati-

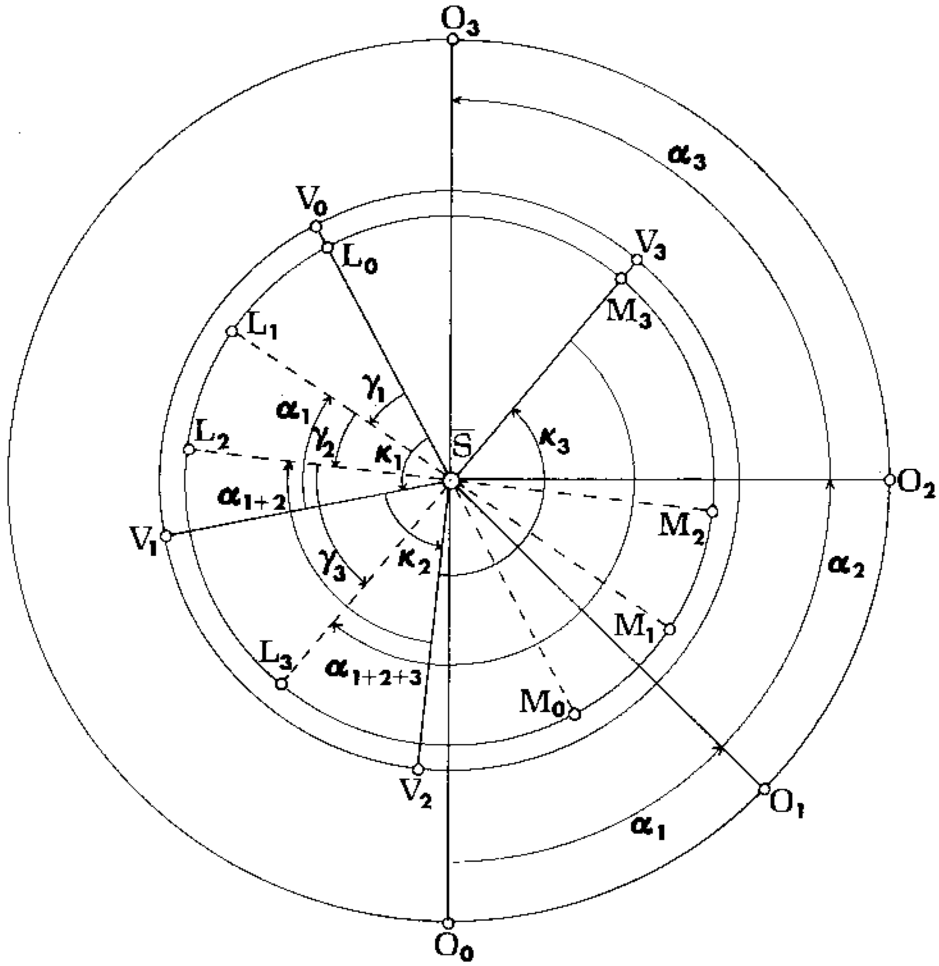


Fig. 37

tude to the north when the planet is farthest from the earth, it must be tilted considerably less to produce the same 0;10° when the planet is closest to the earth. In fact, at maximum distance an inclination of about 0;24° is required, but at minimum an inclination of only 0;4°. Copernicus seems ignorant of this, for he describes a model that inclines the plane to a single maximum value, although he does not specify the angle. Indeed, he does little better in *De rev.* VI, 8, where first he demonstrates that he does not understand Ptolemy's model, and then conveniently miscomputes the apparent latitudes of the deviation when Venus is farthest from and closest to the earth in order to make his single value for the maximum inclination appear plausible. All in all, it is one of the more shoddy performances in *De revolutionibus*.

As a result of neglecting the enormous effect of distance on the apparent latitude, Copernicus's model of the deviation really does not work at all. It is supposed to produce a latitude of 0;10° to the north (he forgets to mention this value in the *Commentariolus* although he does mention the 0;45° deviation for Mercury), but it will do nothing of the kind. The model works in this way:

In figure 37, the mean sun $\bar{S}$ is the center of the orbit of Venus $V_0V_1$ and of the earth $O_0O_1$. The circle $L_0M_0$ shows the motion of the point of maximum deviation. We let $O_0\bar{S}O_3$ be the apsidal line, and begin with the earth at aphelion $O_0$ and Venus at $V_0$. Since the earth is in the apsidal line, the point of maximum deviation $L$ coincides with Venus at $L_0$. Venus's orbital plane is inclined to the north in the direction of

$L_0$ and to the south in the direction of the opposite point $M_0$. Now, the earth moves through $\alpha_1$ to $O_1$, Venus moves through $\kappa_1$ to $V_1$, and $L$ moves back from $V_1$ through $\alpha_1$ to $L_1$. This is how Copernicus describes the motion of $L$. But as figure 33 shows

$$\kappa - \alpha = \tau^* - \bar{\lambda}^*_\odot = \gamma;$$

thus $L$ moves through $\gamma_1$ from $L_0$ to $L_1$, and the opposite point moves from $M_0$ to $M_1$. As $L$ moves through $\gamma_1$, a libration device like that for the superior planets causes its inclination to change, by $\iota_0 \cos\alpha$, where $\iota_0$ is the maximum inclination causing the deviation. Then the earth $O_2$ moves through $\alpha_{1+2} = 90°$, Venus $V_2$ moves through $\kappa_{1+2}$, and $L_2$, having moved $\gamma_{1+2}$, trails $V_2$ by

$$\kappa_{1+2} - \gamma_{1+2} = \alpha_{1+2} = 90°.$$

At this point $\iota_0 \cos\alpha = 0°$, so $L_2$ and the opposite point $M_2$ have no latitude in addition to Venus's *inclination* when the earth is 90° from the apsidal line. Thus, the deviation, which was present when the earth was in the apsidal line and Venus displayed the *reflection*, has now disappeared. The earth then reaches the apsidal line again at $O_3$ where $\alpha_{1+2+3} = 180°$, Venus moves through $\kappa_{1+2+3}$ to $V_3$, and $L$ moves through $\gamma_{1+2+3}$ to $L_3$. Since

$$\kappa_{1+2+3} - \gamma_{1+2+3} = \alpha_{1+2+3} = 180°,$$

$V_3$ coincides with $M_3$, and since $\iota_0 \cos\alpha = -\iota_0$, $L_3$ has moved south by $\iota_0$ and the opposite point $M_3$ has moved north, thereby raising Venus to the north by the maximum deviation. This is half the cycle. The motion of $L$ from north to south and of $M$ from south to north has completed a semicircle of the libration as $V$ has moved a semicircle from $L$, and $O$ a semicircle from $O_0$. The whole thing continues in the same way through the remaining semicircle, $L$ moving back to the north and $M$ to the south. So, as Copernicus says, when the earth is in the apsidal line Venus is only moved north by the deviation, never south. But that is almost the only thing he can claim for the model with any truth. In other respects it is a mess, and I am disappointed that he did not have the sense to get rid of it in *De revolutionibus*.

## 8. MERCURY

### MOTION IN LONGITUDE

But of all things in the heavens the most remarkable is the motion of Mercury which passes through nearly untraceable paths so that it cannot easily be investigated. There is in addition a further difficulty in that most of the time it remains in invisible passages under the rays of the sun, and becomes visible for only a few days. Nevertheless if someone apply himself, only with greater ingenuity, Mercury will also be understood. Just as for Venus, two epicycles that can revolve in its sphere will also be proper to Mercury. Now, as in the case of Venus, the larger epicycle completes revolutions in the same time as its sphere, and fixes the position of Mercury's apsis $14\frac{1}{2}°$ east of *Spica Virginis*. However, by a rule opposite to [the rule governing the motion of] Venus, the smaller epicycle turns back through two revolutions such that in every position of the earth in which it overlooks[a] the apsis of Mercury or looks back at the apsis from the opposite position, the planet is farthest from the center of the larger epicycle, and when the earth is in quadrants [from the apsis] the planet is closest. We have said that the sphere of Mercury completes a revolution in the third month, specifically, in 88 days. The semidiameter of the sphere receives $9\frac{2}{5}$ parts where we have assumed the semidiameter of the great sphere to be 25 parts. Of these parts, the first epicycle receives 1;41 parts, and the second epicycle one-third of the first, that is, about 0;34 part.

[a] 200:25 absidem huius *supervidet* (S superaverit, V supervenit)

Ptolemy's model for Mercury differs from the model for the other planets in two particulars:

1. The center about which the epicycle moves uniformly is closer to the earth than the center from which it maintains a constant distance. In fact, the equant point bisects the mean distance of the center of the eccentric.[1]

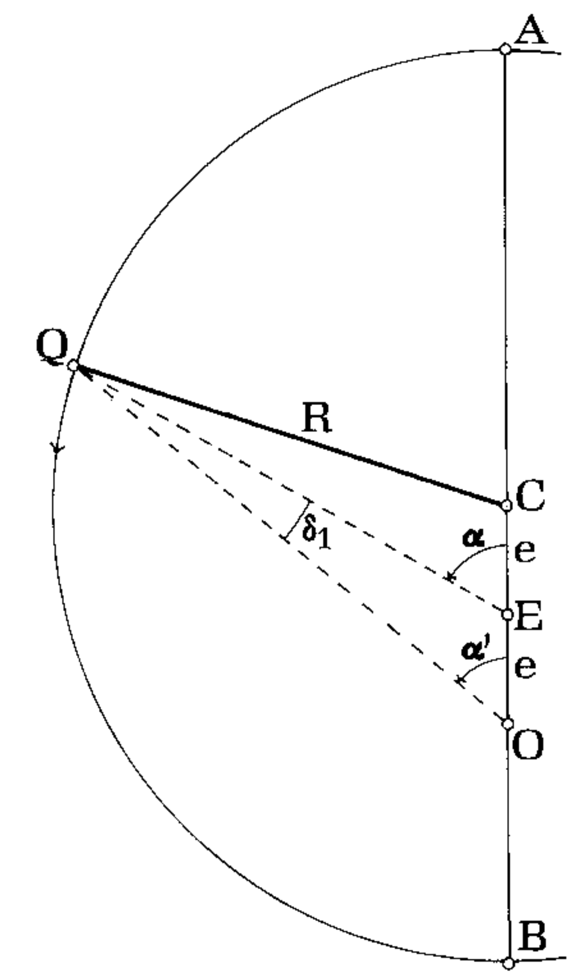


Fig. 38

[1] This is true in the *Almagest*, but not in the *Planetary Hypotheses* where Ptolemy reduces the distance from the equant to the center of the small circle and the radius of the small circle from 3;0 to 2;30 while leaving the distance from the earth to the equant at 3;0. Some of the following analysis will not apply correctly to the model in the *Planetary Hypotheses*. Hartner, 1955, an extraordinary study, should be consulted on Ptolemy's model for Mercury, but with a certain amount of caution.

2. The center of the eccentric moves on a small circle in the direction opposite to the motion of the center of the epicycle and with equal velocity. The purpose of this is to increase the equation of the anomaly by bringing the center of the epicycle closer to the earth at ±120° than at 180° from the longitude of the apogee of the eccentric. This was done because Ptolemy observed that Mercury has its least elongations in Libra, and its greatest elongations four signs away in Aquarius and Gemini rather than directly opposite in Aries.

The motion of the center of the eccentric does affect the equation of center, but within reasonable limits of accuracy the equation of center and the variation of distance can be separated, leading to a far simpler equation of center. Copernicus's model for Mercury which, like his other planetary models, is identical to Ibn ash-Shāṭir's model except for the heliocentric representation of the second anomaly, is based on exactly this separation of the equation of center from the motion of the center of the eccentric in Ptolemy's model.[2] The order of presentation in the *Commentariolus* clearly reflects this distinction, and we shall proceed in the same way in our analysis.

Figure 38 shows the simplified version of the first anomaly in Ptolemy's model. The earth is at $O$ on the sidereally fixed apsidal line $AB$, and the center of the eccentric $C$ is removed from $O$ by $2e$. Bisect $OC$ at $E$, and $E$ will be the center with respect to which the center $Q$ of the epicycle moves uniformly while maintaining a constant distance $R$ from $C$. Letting the distance of $Q$ from the apogee be $\alpha$, the equation of center $\delta_1$ is

$$\tan \delta_1 = \frac{|e \sin \alpha|}{\sqrt{R^2 - e^2 \sin^2 \alpha} + 2e \cos \alpha}.$$

The Copernican model, with the mechanism to vary the radius of Mercury's orbit separated from the rest of the model, is shown in figure 39.

[2] Kennedy and Roberts, 1959. The equation of center in Ibn ash-Shāṭir's solar model (Roberts, 1957) is the same as the Mercury model in that the two epicycles produce uniform motion about a point bisecting the distance to the center of the eccentric in Ptolemy's model. The motivation for this model is not clear, and it causes an unnaturally large variation in the distance and hence the apparent diameter of the sun.

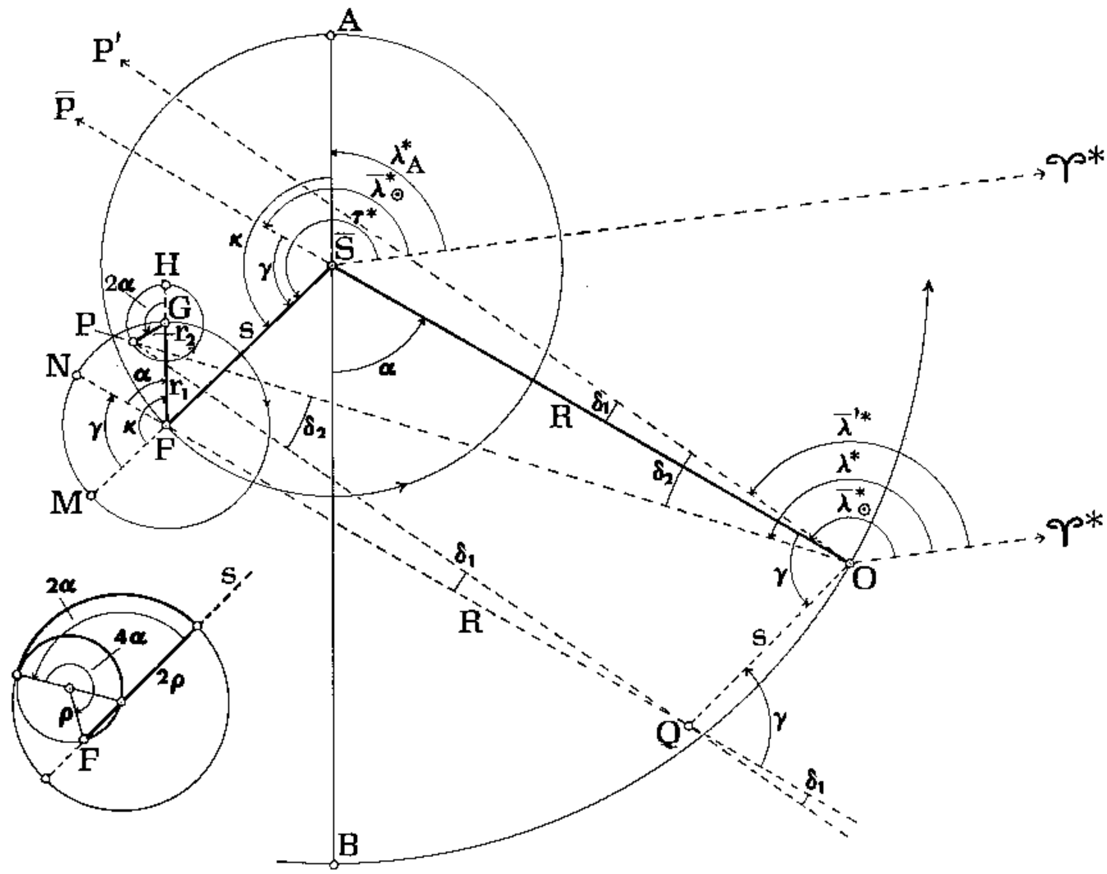


Fig. 39

Everything is exactly the same as the model for Venus in figure 33 with the exception of the motion of $P$ on the second epicycle. As in the case of Venus, $r_1$ rotates through $\kappa$ when measured from the direction $\bar{S}FM$, but through $\alpha = \kappa - \gamma$ when measured from $QFN$ which is parallel to $O\bar{S}\bar{P}$. But the motion of $P$ is $2\alpha$ such that when $\alpha = 0°$, $P$ lies at $H$, the farthest point on $r_2$ from $F$. The equation of center, again purely a function of $\alpha$, the distance of the earth from Mercury's apsidal line, is given by

$$\tan \delta_1 = \frac{|(r_1 - r_2) \sin \alpha|}{R + (r_1 + r_2) \cos \alpha}.$$

If

$$r_1 + r_2 = 2e \quad \text{and} \quad r_1 - r_2 = e$$

the lower term will differ from the lower term in the simplified version of Ptolemy's model by

$$\frac{e^2 \sin^2 \alpha}{2R},$$

so the equations for Mercury have exactly the same equivalence as the equations for the other planets. The motion of $P$ with respect to $F$ will again generate an ellipse; this time with semi-major axis $r_1 + r_2$ lying on line $QFN$ and semi-minor axis $r_1 - r_2$ meeting $QFN$ at $F$. Viète uses this ellipse directly in his model for Mercury.

The equivalent model in *De revolutionibus* is shown in figure 40. The epicycles $r_1$ and $r_2$ have been replaced by the fixed and rotating eccentricities $e_1$ and $e_2$. The equation of center is, of course, unchanged, and it can be seen that relative to a fixed $O$, the motion of the center $G$ of Mercury's orbit is uniform with respect to $E$ removed from $O$ in the direction of $A$ at the distance $e_1 - e_2$. In Ptolemy's model this distance is $e$, and since in principle

$$e_1 - e_2 = r_1 - r_2 = e,$$

we see clearly why all three models are equivalent.

The second special condition in Ptolemy's model, the motion of the center of the eccentric, is shown in figure 41. Instead of having $Q$ remain a fixed distance $R$ from $C$, we describe a circle about $C$ of radius $e$, and let $Q$ remain at the distance $R$ from a point $G$ that moves about

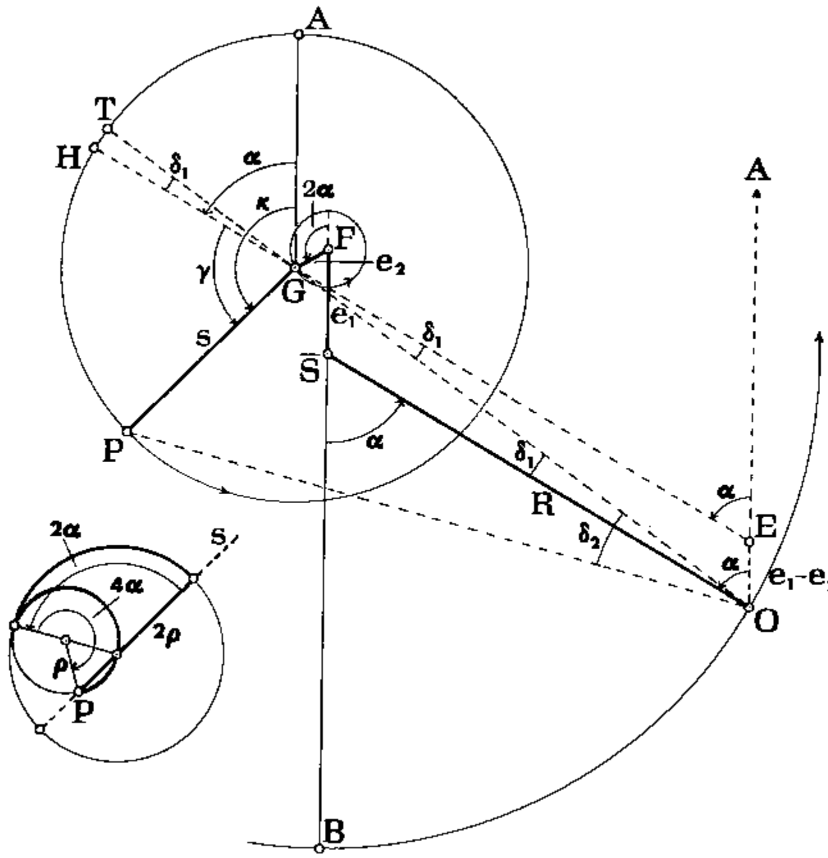

Fig. 40

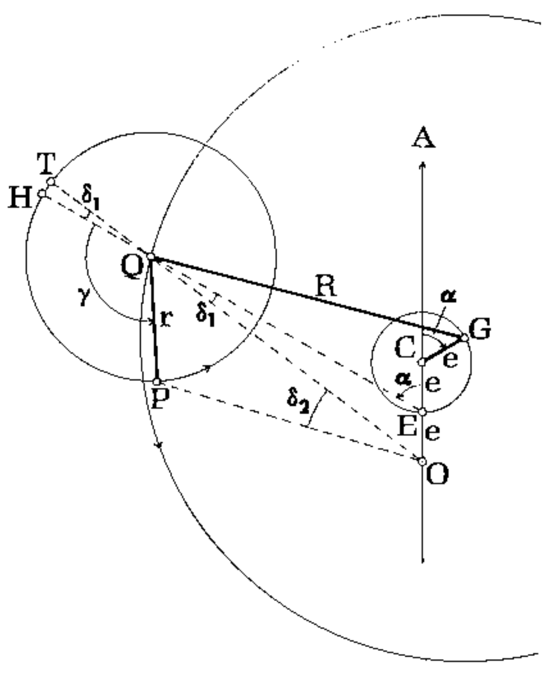

Fig. 41

$C$ through $\alpha$ in the direction opposite to the uniform motion of $Q$ about $E$. $G$ is then the center of the eccentric on which $Q$ moves. The distance of $G$ from $O$ will vary from $3e$ at $\alpha = 0°$ to $e$ at $\alpha = 180°$. The distance $OQ$ will be $R + 3e$ at $\alpha = 0°$, $R - e$ at $\alpha = 180°$, and will reach its least distance at $\alpha \approx \pm 120°$. In the *Almagest*, where $R = 60$, $e = 3;0$, so $R + 3e = 69$ and $R - e = 57$. The least distance of $Q$ is about 55;34. The equation of center is made considerably more complex by the motion of the center of the eccentric, although this was not an intentional result of the model. Determination of the distance $OQ$ for arbitrary values of $\alpha$ is also very laborious.

The principal effect of the model is to move $Q$ closer to $O$ at $\alpha \approx \pm 120°$ than at $\alpha = 180°$, and thereby optically magnify the radius of the epicycle, increasing the equation of the anomaly $\delta_2$ as required. This is just what happens in the lunar model at mean quadrature. The lunar model of Ibn ash-Shāṭir and Copernicus replaced this optical magnification by a real increase in the effective radius of the epicycle while holding the distance of the center of the epicycle constant. The same principal is used for Mercury—the effective radius of its orbit is made to vary—but the variation is brought about by a rectilinear motion along the radius itself. There is also a variation in the distance of the planet from the mean sun introduced by the two epicycles, and it must be taken into account in deriving the quantity of the variation of the radius of the orbit. We shall defer consideration of the parameters until after the complete model has been examined.

Copernicus's description of the variation of the radius of Mercury's sphere is as follows:

> But this combination of circles, although adequate to the other planets, is not adequate to Mercury because, when the earth is in the views of the apsis mentioned above, the planet appears to move by traversing a far smaller circumference, and on the other hand, when the earth is at quadratures [to the apsis], by traversing a far larger circumference than the proportion of the circles just given permits. Since, however, no other anomaly in longitude is seen to arise from this, it seems suitable that it take place on account of some kind of approach [toward] and withdrawal from the center[a] of the sphere on a straight line. This is necessarily brought about by two small nested spheres with axes parallel to the axis of the sphere provided: that the center of the larger epicycle or of the whole of this[b] (*sic*) be distant[c] as far from the center of the small sphere directly connecting [with it] as the center of this small sphere is from the center of the outer small sphere (it has been found that each is distant 0;14½[d] part of the 25 parts by which we have measured the proportion of all the circles), and that the motion of the outer small sphere complete two revolutions in a tropical (*sic!*) year while the inner small sphere, which has a motion in the opposite direction with twice the number of returns, turn around four times in the same period. For by this composite motion, the center[e] of the larger epicycle is carried[f] on a straight line, just as we have explained concerning latitudes that are librated. Thus, when the earth is in the positions noted with respect to the apsis of Mercury, the center of the larger epicycle is closest to the center of the sphere, and when the earth is at quadratures [to the apsis], the center of the larger epicycle is farthest from the center of sphere. But when the earth is in positions falling halfway between, that is, 45° from the apsis and from the quadratures to the apsis, the center of the larger epicycle joins the center of the outer small sphere and both centers coincide. The length of this withdrawal and approach is 0;29 of the aforesaid parts. This completes the explanation of Mercury's motion in longitude.

[a] 201:7 recessum *a centro* orbis (S)
[b] 201:10 totius illius . . .-asse? (SV asse, here omitted)
[c] 201:10 tantum *distet* (V)
[d] 201:12 *14* et medio (S)
[e] 201:16 *centrum* maioris (SV centro)
[f] 201:15 *Praefertur* enim (SV Praeferuntur)

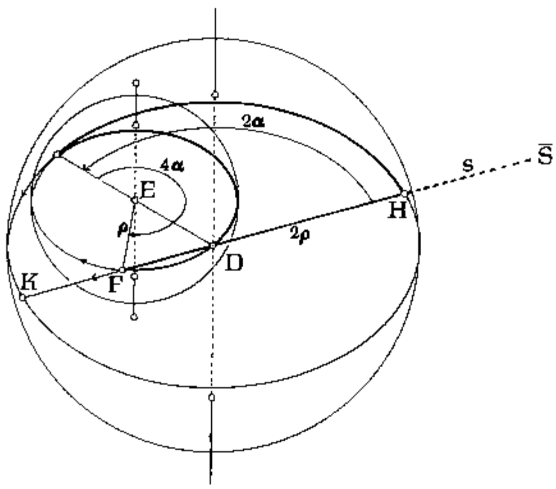


Fig. 42

There is something very curious about Copernicus's description. The principal effect of Ptolemy's model is to produce the greatest elongations at ±120° from apogee. This is also true of Copernicus's model, as he demonstrates in *De rev.* V, 28, but he says nothing about it here. Instead he describes a totally fictitious apparent motion of Mercury that is really only a description of the expanding and contracting radius of its orbit in the model. The statement that Mercury "appears" to move in a smaller orbit when the earth is in the apsidal line and in a larger orbit when the earth is 90° from the apsidal line is utter nonsense as a description of the apparent motion of Mercury. No one—not Ptolemy, not Regiomontanus, not even Copernicus in *De revolutionibus*—gives such a description of Mercury's apparent motion because this is *not* Mercury's apparent motion. But it *is* a description of the motion of Mercury in the model. Copernicus apparently does not realize that the model was designed, not to give Mercury a larger orbit (read *epicycle*) when the earth (read *center of the epicycle*) is 90° from the apsidal line, but to produce the greatest elongations when the earth (*center of the epicycle*) is ±120° from the aphelion (*apogee*).

This misunderstanding must mean that Copernicus did not know the relation of the model to Mercury's apparent motion. Thus it could hardly be his own invention for, if it were, he would certainly have described its fundamental purpose rather than write the absurd statement that Mercury "appears" to move in a larger orbit when the earth is 90° from the apsidal line. The only alternative, therefore, is that he copied it without fully understanding what it was really about. Since it is Ibn ash-Shāṭir's model, this is further evidence, and perhaps the best evidence, that Copernicus was in fact copying without full understanding from some other source, and this source would be an as yet unknown transmission to the west of Ibn ash-Shāṭir's planetary theory.

When he wrote *De revolutionibus* Copernicus did describe the effect of the model properly. Indeed the first thing he says about Mercury, in V, 25, is that it has its least elongations in Libra, but its greatest elongations in Gemini and Aquarius rather than in Aries. However, the absence of any such description of Mercury's motion in the *Commentariolus*, and the substitution of the erroneous description of its apparent motion, certainly makes it appear that Copernicus did not yet know that the model is equivalent to Ptolemy's in its effect on the location of the greatest elongations. Since the maximum elongations in Aquarius and Gemini, ±120° from the least elongations in Libra, are the real purpose of the variation of the radius of Mercury's orbit, it is most unlikely that Copernicus would fail to mention this unless he was unaware of it.

The device for varying the radius of Mercury's orbit is not the same as the libration mechanism used for the latitudes. The description of the motions of an *inner* and an *outer* small sphere only makes sense as a description of the other method for generating a rectilinear motion from two circular motions used by the Marāgha astronomers. This is, to describe it in the plane, a circle rolling on the internal circumference of a circle of twice its radius. Copernicus, as usual, envisions this as the motion of spheres, so the two circles are the great circles in the planes perpendicular to the axes of the spheres. The device is shown in figure 42 and inset in figures 39 and 40. The center of Mercury's orbit is $\bar{S}$, and the broken line $\bar{S}H$ is the smallest radius of the orbit. Point $D$, lying on an axis attached to the inner surface of Mercury's sphere at a distance from $\bar{S}$ equal to the mean radius of the orbit, is the center of a sphere of radius $2\rho$ which revolves in the direction shown through $2\alpha$ where $\alpha$ is the distance of the earth from Mercury's apsidal line. Internally tangent to the outer sphere is the inner sphere with center $E$ lying on an axis parallel to the axis through $D$. The inner sphere is of radius $\rho$, and revolves in the opposite direction through $4\alpha$. Point $F$, the center of the larger epicycle in figure 39, is on the inner sphere, so $EF = DE = \rho$, and $F$ will be carried back and forth on $HK = 4\rho$. When the earth is in Mercury's apsidal line, $2\alpha = 0°$, $F$ is at $H$, and Mercury has its smallest orbit of radius $\bar{S}H$. When the earth is 90° from the apsidal line, $2\alpha = 180°$, $F$ is at $K$, and Mercury has its largest orbit, $4\rho$ larger than its smallest. And, as Copernicus says, when the earth is 45° from the apsidal line, $2\alpha = 90°$, and $F$ coincides with $D$. For any value of $\alpha$, the position of $F$ is given by

$$DF = 2\rho \cos 2\alpha$$

or

$$HF = 2\rho(1 - \cos 2\alpha).$$

Thus the radius of Mercury's orbit is

$$\bar{S}F = \bar{S}D - 2\rho \cos 2\alpha = \bar{S}H + 2\rho(1 - \cos 2\alpha).$$

Two further points in the text require comment. The phrase "or of the whole of this" (*sive totius illius*), which I do not understand, is followed by the word *asse*, which I also do not understand and have omitted. A conjecture that seems plausible is that *sive totius illius* is the beginning of a phrase saying something like "or the whole of this if the two small nested spheres are located at the center of Mercury's sphere" since it is possible to place this mechanism at the other end of the radius, at the center of Mercury's sphere. Copernicus describes this sort of model in *De rev.* V, 32.[8] This would take a line or so that dropped out of the text, leaving the otherwise meaningless *asse*, which was perhaps the end of a divided word and could as well have been -*isse* or -*esse*. The other oddity in the description is Copernicus's slipping and referring to the outer small sphere's completing two revolutions in a tropical year (*in anno vertente*) when of course he means a sidereal year, the period in which the earth completes a revolution with respect to Mercury's sidereally fixed apsidal line.

The true longitude of Mercury may be computed from the model in the same way as for the other planets except that it is also necessary to adjust the radius of the orbit as a function of $\alpha$, the distance of the earth from the apsidal line. Letting $s$ be the mean radius of the orbit and $r_3 = 2\rho$ be half the difference between the maximum and minimum radius, that is,

$$r_3 = 2\rho = \tfrac{1}{2}(s_{\max} - s_{\min}),$$

then the corrected radius $s'$ is

$$s' = s - r_3 \cos 2\alpha.$$

Then $\delta$, the difference between $\bar{\lambda}^*$ and $\lambda^*$, is given by

$$\tan \delta = \frac{|-(r_1 - r_2)\sin\alpha + (s - r_3\cos 2\alpha)\sin\gamma|}{R + (r_1 + r_2)\cos\alpha + (s - r_3\cos 2\alpha)\cos\gamma}.$$

Call the higher term $\Delta$, then,

$$\lambda^* = \bar{\lambda}^* \pm \delta. \quad \begin{array}{l} + \text{ for } \Delta \geq 0 \\ - \text{ for } \Delta \leq 0 \end{array}$$

THE DERIVATION OF THE PARAMETERS

The derivation of the parameters for Mercury requires considerably more effort than the simple extraction of sines used for the other planets. Again, the higher part of U gives the evidence for reconstructing Copernicus's procedure. It reads:

376 Mercurii ecce[ntricitas] 2256 Ep[icyclus] a
cum b ·10·6··/ 100
diversitas diametri 1151 5 ʼ9ʹ 19

The number 2256 was possibly changed from 2259, and the 10·6 looks as if it may have originally been 10·0. The meaning of the numbers is as follows: 2256, called the *eccentricitas* because in the eccentric model of the second anomaly it is the eccentricity, corresponds to the radius of Mercury's sphere where the distance from the earth to the mean sun is 6000. This indicates that Copernicus was using sine tables normed to a radius of 6000 or 60000. Regiomontanus's *Tabulae directionum*, which are bound in with U, contain sine tables normed to 60000, and we shall use them in recomputing Copernicus's parameters. 376 is a conversion of 2256 for a radius of 1000, that is, $376 = \frac{1}{6} \cdot 2256$. It is possible that Copernicus used sines normed to 60000 for all the planets, and then divided by 6 to produce the numbers in U. Since this makes no difference in the final results, I did the previous derivations with sines normed to 10000. Here it is necessary to use sines normed to 60000 to pro-

[8] This model works on a somewhat different principle. The radius of Mercury's orbit remains constant, but the distance of the center of the orbit from point $G$ in figure 40 varies. Copernicus says the model will be of use for the theory of latitude, and then points out in VI, 2 *ad fin.* that it is associated with the *deviation* component. A study of this model will appear in a forthcoming volume of *Studia Copernicana*

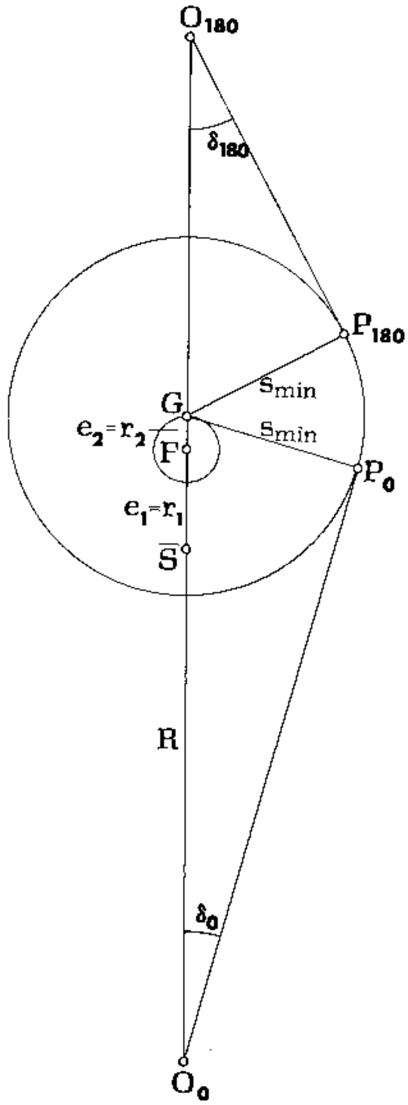


Fig. 43

duce the numbers given to this norm in U. Now

$$\frac{2256}{6000} = \frac{376}{1000}$$

corresponds to the *mean* radius of Mercury's orbit. The number 100 is the sum of the radii of the two epicycles where $R = 1000$; it is thus related to the 376. It was derived from $\frac{1}{6}(600)$, and 600 or the equivalent must be the meaning of the 6···. The *diversitas diametri* 1151 is the radius of the outer sphere of figure 42; it is thus the difference between the mean radius of Mercury's orbit 2256[0] and the minimum and maximum radii. Then note that $\frac{1}{6}(1151) = 191\frac{5}{6}$ $\approx 190$; this is the origin of the 19, which is the radius of the outer small sphere where $R = 1000$. The term *diversitas diametri*, equation or variation of the diameter, is taken from the *Epitome* and the *Alphonsine Tables* where it refers to the addition to the lunar equation of anomaly resulting from the variation of the distance of the epicycle. In Copernicus's model this is accounted for by varying the effective radius of the lunar epicycle so it must have seemed appropriate to use it here for the quantity of the variation of the radius of Mercury's orbit.

The method Copernicus used to derive the parameters is identical to the derivation in *De rev.* V, 27. The results are slightly different because he used a different angle for Mercury's least elongation, when the earth is farthest from the center of Mercury's orbit. An interesting property of the procedure is that it makes no use of Mercury's greatest elongation at $\alpha = \pm 120°$ so it can be carried out without even realizing that the model produces the greatest elongation at $\alpha = \pm 120°$. In *De rev.* V, 28, Copernicus shows that the greatest elongations do in fact occur at $\alpha = \pm 120°$, but in the *Commentariolus* he does not so much as mention it, leading me to believe, as I said before, that he did not yet understand that the model produced this result. The procedure itself depends specifically on the fallacious observation that Mercury moves in a larger orbit when $\alpha = \pm 90°$ which, as I also said, is really just a description of the model, not of the apparent motion of Mercury. That such a procedure will give the required parameters is a consequence of the separation of the equation of center from the mechanism to produce greatest elongations at $\alpha = \pm 120°$. I suppose that Copernicus found this model someplace, and after studying it realized that it was only necessary to use elongations at $\alpha = 0°$, 180°, and 90° to determine fully the radius of the orbit, the radii of the two epicycles, and the quantity of the variation of the radius of the orbit. The elongations at $\alpha = 0°$ and 180° give the minimum radius of the orbit and the sum of the radii of the two epicycles. The elongations at $\alpha = 90°$ give the maximum radius of the orbit, and the difference between the maximum and minimum radii immediately gives the amount of the variation which is twice the radius of the outer small sphere or four times the radius of the inner small sphere in figure 42. In *De rev.* V, 27 Copernicus also uses the elongation at $\pm 90°$ to find the radius of each epicycle such that $r_1 \neq 3r_2$, but he does not do so here as the radii of the epicycles in the lower part of U and in the *Commentariolus* are in the proportion of 3 to 1. We shall consider each derivation separately.

1. The conditions for the derivation of the minimum radius of the orbit and the sum of the radii of the epicycles are shown in figure 43. In order to make the derivation clearer I have made two alterations in what must have been Copernicus's original procedure. I have substituted the finished heliocentric model for the geocentric model using the eccentric representation of the second anomaly, and replaced the two epicycles of the *Commentariolus* with the two eccentricities used in *De revolutionibus*. Thus the radius of the orbit $s_{min}$ is equivalent to $e$, the eccentricity in figure 21, and $e_1$ and $e_2$ are equivalent to $r_1$ and $r_2$ in figure 39. The mean sun is $\bar{S}$, the center of the rotating eccentricity is $F$, and the center of Mercury's smallest orbit $s_{min}$ is $G$, which is farthest from $\bar{S}$ at $\alpha = (0°,180°)$. The earth is shown in the apsidal line at its greatest distance from $G$ in $O_0$ where $\alpha = 0°$, and at its least distance from $G$ in $O_{180}$ where $\alpha = 180°$. The distance $O\bar{S} = R$. When $\alpha = 0°$, the planet $P_0$ has a maximum elongation $\delta_0$, and when $\alpha = 180°$, the planet $P_{180}$ has a maximum elongation $\delta_{180}$. We wish to find $s_{min}$ and $\bar{S}G$ where $O\bar{S} = R$.

The elongations are derived from the *Alphonsine Tables.*

$$\delta_0 = (c_6 - c_5)_{max(\alpha=0°)} = 22;2° - 3;2° = 19;0°$$

$$\delta_{180} = (c_6 + c_7)_{max(\alpha=180°)} = 22;2° + 1;14° = 23;16° \approx 23;15°$$

The latter is identical to Ptolemy's value (*Almagest* IX, 8 = *Epitome* IX, 15 = *De rev.* V, 27), while the former is 0;3° less than the elongation reported by Ptolemy. This makes a small difference in the results. In all that follows, we use the sine tables in Regiomontanus's *Tabulae directionum* which are normed to a radius of 60000.

Now

$$\frac{GP_0}{GO_0} = \frac{\sin 19;0°}{60000} = \frac{19534}{60000}$$

and

$$\frac{GP_{180}}{GO_{180}} = \frac{\sin 23;15°}{60000} = \frac{23684}{60000}.$$

Thus where $GO_0 = 60000$,

$$GO_{180} = \frac{19534}{23684} \cdot 60000 = 49486\tfrac{1}{2} \approx 49487,$$

so that

$$O_0GO_{180} = 2\bar{S}O = 60000 + 49487 = 109487,$$

and

$$\bar{S}O = \tfrac{1}{2}(109487) = 54743\tfrac{1}{2} \approx 54744,$$

and

$$\bar{S}G = GO_0 - \bar{S}O = 60000 - 54744 = 5256.$$

Letting

$$\bar{S}O = 60000,$$

$$s_{min} = GP_0 = GP_{180} = \frac{19534}{54744} \cdot 60000 = 21409,$$

and $s_{min} = 21409$ is the least radius of the orbit where $R = 60000$. Likewise

$$\bar{S}G = \frac{5256}{54774} \cdot 60000 = 5761 \approx 5760,$$

so

$$r_1 + r_2 = e_1 + e_2 = 5760$$

$$r_1 - r_2 = \tfrac{1}{2}(5760) = 2880$$

and since $r_1 = 3r_2$

$$r_1 = 4320 \quad \text{and} \quad r_2 = 1440.$$

2. The maximum radius of the orbit occurs when $\alpha = 90°$. The conditions for its derivation are shown in figure 44. The mean sun is $\bar{S}$, the earth $O_{90}$ has moved $\alpha = 90°$ from the apsidal line, and the center of Mercury's orbit $G$ has moved through $2\alpha = 180°$ so it is now closest to $\bar{S}$. $\bar{S}G = r_1 - r_2 = e_1 - e_2$. Mercury is seen at its greatest morning elongation $P_M$ and its greatest evening elongation $P_E$. It is reported in *Epitome* IX, 17 (= *Almagest* IX, 9) that

$$P_EO_{90}\bar{S} = 26;15°$$

and

$$P_MO_{90}\bar{S} = 20;15°.$$

Therefore

$$P_EO_{90}P_M = 46;30°$$

and

$$\delta_{90} = \tfrac{1}{2}(46;30°) = 23;15°.$$

Here Copernicus could derive a new value for $\bar{S}G$ as he does in *De rev.* V, 27. That is

$$\bar{S}O_{90}G = 26;15° - 23;15° = 3;0°$$

so that

$$e_1 - e_2 = \bar{S}G = \tan 3;0° = \frac{3145}{60000},$$

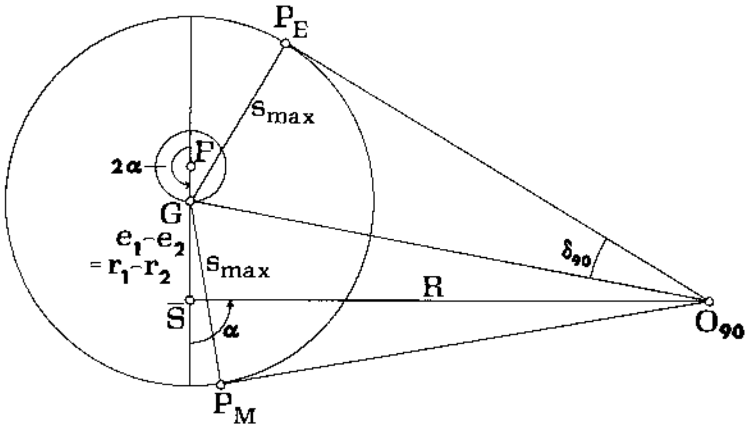


Fig. 44

but apparently he did not do this, or at least he retained the previous value for the rest of this computation. The new value for $\bar{S}G$ is used in *De rev.* to set $e_1 \neq 3e_2$, but nothing of the kind is done here. Now

$$OG^2 = O\bar{S}^2 + \bar{S}G^2 = R^2 + (e_1 - e_2)^2$$

and where $R = 6000$,

$$e_1 - e_2 = 4320 - 1440 = 2880.$$

Thus

$$OG^2 = 60000^2 + 2880^2$$

and

$$OG \approx 60000 + \frac{2880^2}{2 \cdot 60000} \approx 60069.$$

Since

$$\sin \delta_{90} = \sin 23;15° = \frac{23684}{60000},$$

thus

$$s_{max} = GP_E = \frac{23684}{60000} \cdot 60069 = 23711,$$

and $s_{max} = 23711$ is the maximum radius of the orbit.

Therefore, the mean radius of the orbit $s$ is

$$s = \tfrac{1}{2}(s_{max} + s_{min}) = \tfrac{1}{2}(23711 + 21409) = 22560,$$

and this is the *eccentricitas*, that is, the radius of Mercury's sphere given in U where the zero is omitted. Then

$$s_{max} - s = 23711 - 22560 = 1151$$

is the *diversitas diametri* in U, that is, the radius of the outer small sphere controlling the variation of the radius of the orbit.[4] These are then normed to $R = 1000$ by

$$\frac{1}{60}(22560) = 376$$

and

$$\frac{1}{60}(1151) = 19\frac{11}{60} \approx 19.$$

The crossed-out *5`9'* are remainders in the computation of 19. The sum of the radii of the epicycles was 5760; thus

$$\frac{1}{60}(5760) = 96 \approx 100.$$

In converting these numbers to the parameters in the lower part of U, Copernicus seems to have had some difficulty with the radii of the epicycles since he crossed out some numbers and still came up with an incorrect result. He first wrote ☿ *orbis* 9·24, which is correct, then wrote and crossed out

Epi[cyclus] a 1·44¾· 1·42¾· b 0 34¼

and in the next line settled on

Epi[cyclus] a·1·41¼· b 0·33¾
coll[igunt?] 1·7·½,

and then wrote *diversitas diamet[ri]* 0·29 which is correct. The semidiameter of the sphere is given by

$$25 \cdot \frac{376}{1000} = 25 \cdot \frac{6,16}{16,40} = 9;24$$

[4] Si te hujus laboriosae methodi pertaesum fuerit, jure mei te misereat . . .

and the *diversitas diametri* by

$$25\cdot\frac{19}{1000} = 25\cdot\frac{19}{16,40} = 0;28,30 \approx 0;29.$$

Both of these numbers are carried over unchanged to the *Commentariolus*. The distance between the centers of the small spheres is therefore $\frac{1}{2}\cdot 0;29 = 0;14,30$, as in the text.

If the sum of the radii of the epicycles is 100, then

$$r_1 + r_2 = 25\cdot\frac{100}{1000} = 25\cdot\frac{1,40}{16,40} = 2;30$$

so that

$$r_1 = 1;52,30 \quad r_2 = 0;37,30 \quad r_1 - r_2 = 1;15.$$

Or if one uses in place of 100 the unrounded 96

$$r_1 + r_2 = 25\cdot\frac{96}{1000} = 25\cdot\frac{1,36}{16,40} = 2;24,$$

and thus

$$r_1 = 1;48 \quad r_2 = 0;36 \quad r_1 - r_2 = 1;12.$$

For some reason Copernicus had difficulty with this. First he wrote $r_1 = 1;44\frac{3}{4}$ with no value for $r_2$. Then he wrote

$$r_1 = 1;42\tfrac{3}{4} \qquad r_2 = 0;34\tfrac{1}{4}$$

so that

$$r_1 + r_2 = 2;17 \quad r_1 - r_2 = 1;8,30,$$

and finally

$$r_1 = 1;41\tfrac{1}{4} \qquad r_2 = 0;33\tfrac{3}{4}$$

leading to

$$r_1 + r_2 = 2;15 \quad r_1 - r_2 = 1;7,30.$$

These are incorrect, for if $r_1 + r_2 = 2;15$ where $R = 25$, then where $R = 1000$, $r_1 + r_2 = 90$ instead of 100 and $r_1 - r_2 = 45$. This would give an equation of center of 2;35°, which is absurd, compared with 2;52° for $r_1 - r_2 = 50$, which is closer to the correct equation of 3;2°. I do not know why Copernicus had these problems. In the *Commentariolus* he rounded the numbers to $r_1 \approx 1;41$ and $r_2 \approx 0;34$.

The apsidal longitude $\lambda^*_A$ is given as $14\frac{1}{2}$° east of Spica. Using the *Alphonsine Tables* as before

$$\lambda_A - \text{Spica} = 3,27;55^\circ - 3,13;48^\circ = 14;7^\circ \text{ east},$$

so Copernicus's value is 0;23° east of the Alphonsine. This is the smallest difference of any of the apsidal longitudes.

### MOTION IN LATITUDE

Mercury performs a [digression in] latitude no different from that of Venus, but always in the opposite part of its sphere, for where Venus becomes northern Mercury goes to the south. Its sphere is inclined to the ecliptic by an angle of 7°. The deviation, which in the case of Mercury is also always southern, never exceeds $\frac{3}{4}$°. Otherwise, what has been explained concerning the latitude[a] of Venus may properly be called to mind here also so that the same things are not repeated all over again.

[a] 202:2 circa *latitudinem* (S)

The latitude model for Mercury is the same as that for Venus except:

1. The descending rather than the ascending node is located at the aphelion. Thus when the earth is in the apsidal line at $\alpha = (0°, 180°)$ and the reflections are visible, eastern (evening) elongations will be southern and western (morning) elongations northern at $\alpha = 0°$, and it will be the reverse at $\alpha = 180°$. When the earth is ±90° from the apsidal line, the perigee of Mercury's orbit is inclined to the north and the apogee to the south at +90°, and the reverse at −90°.

2. The deviation always inclines the planet to the south. Otherwise it is the same as Venus's deviation.

Copernicus gives two parameters for Mercury's latitude; both are simply taken from Ptolemy (*Epitome* XIII, 3 and 12) as was the one parameter given for Venus. The angle of 7° is the *slant* of Mercury's orbital plane. The *inclination*, not given, is 6;15°. Note that for Venus Copernicus gave the inclination 2;30, but not the slant 3;30°. That he gives different components for each planet indicates that, even though he believed the motion of the earth removed one of the components of latitude, he was not sure which one to retain as the inclination of the orbital plane in his new latitude theory, or perhaps he was just confused. Naturally, all of this is corrected in *De revolutionibus* when he restores both components of latitude.

The deviation of 0;45° is affected by the same problem as the 0;10° deviation of Venus. That is, Copernicus's model for the deviation, unlike Ptolemy's variable inclination of the plane of the eccentric, cannot give the planet the same apparent deviation every place in its orbit. The error, however, is not as serious in Mercury as it is in Venus because Mercury has a smaller variation in its distance from the earth.

## 9. [CONCLUSION]

And so altogether, Mercury moves on seven circles, Venus on five, the earth on three and the moon moves about it on four, and finally Mars, Jupiter, and Saturn on five each. Therefore, taken as a whole, 34 circles are sufficient to represent the entire structure of the heavens and the entire choric dance of the planets.

The enumeration of the circles is as follows: The earth requires three—one for the annual revolution, one for the daily rotation, and one for the motion of the inclination or precession. The moon has its sphere, two epicycles, and an outer sphere causing the nodes to move along the ecliptic. The five for Mars, Jupiter, and Saturn are the sphere, the two epicycles, and the two higher spheres causing the orbital plane to librate. The five for Venus are the sphere, the two epicycles, and the two spheres producing the deviation. Note that, if Copernicus intended the orbital plane to slant further in the reflections, two more spheres would be necessary. Mercury's seven are the sphere, the two epicycles, the two spheres varying the radius of the orbit, and the two for the deviation. Again, two more spheres would be required to distinguish the inclination and reflection.

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