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On the Planetary Theory of Copernicus

O. NEUGEBAUER

Brown University, Providence, R. I., and
Institute for Advanced Study, Princeton, N. J., U.S.A.

IN MEMORY OF ROBERT OPPENHEIMER

In 1958 A. Koyré spoke about "l'abandon de l'équant, ce haut titre de gloire de l'astronomie copernicienne", adding that in the lunar theory "Copernic réussit la simplification la plus grande en la débarassant de l'équant (ce qui nous donne la mesure de son génie mathématique)."[1]

About three and a half centuries earlier, Vieta held a different opinion when he said:[2] "Ptolemy and Copernicus, who is always paraphrasing him, did not show themselves as good geometers at the determination of the apsides, the eccentricities, and the epicycle radii from 3 mean and 3 true positions; they assumed the problem settled and therefore solved it in an unfortunate fashion.[3] And Copernicus not only admits his unprofessional way but shows it in Chapter IX of Book III of *De Revolutionibus*, where he tries to determine the maximum equation of the equinoxes from observations of Timocharis, Ptolemy, and al-Battānī as well as the epochs of the anomaly from the limit of the slowing down.[4] More a master of the dice than of the (mathematical) profession he asks to rotate the circle until the error which admittedly comes from his ungeometrical procedure might, with good luck, be compensated."

Both pronouncements are quite characteristic of their times: on the one hand, the ever increasing modern tendency toward hero worship on the basis of "ideas" and disrespect for technicalities; on the other hand, the aggressiveness of Renaissance scholarship, which did not hesitate to point out weaknesses wherever they could be found. But the reader will notice that neither one of the above-mentioned statements is concerned with the alternative geocentric versus heliocentric universe but with the mathematical achievements and abilities of Copernicus. It is only this latter aspect which is the theme of the present paper.

I am aware of the fact that much of the following is not new, at least not to the small group of scholars who during the past decade have uncovered the Islamic antecedents of the Copernican methods[5] nor to those who are familiar with the technical procedures of

[1] In Taton, *Histoire générale des sciences*, vol. ii, p. 64.

[2] In his "Apollonius Gallus" (Paris 1600), *Opera math.*, p. 343; also in Kepler, *Werke* vol. iii, p. 464 (ad p. 156, 11).

[3] Cf. below, p. 102, referring to an iteration method of approximations.

[4] Cf. below, p. 96.

[5] Cf. V. Roberts, The solar and lunar theory of Ibn ash-Shāṭir, a pre-Copernican Copernican model, *Isis* vol. 48 (1957), pp. 428–32; E. S. Kennedy and V. Roberts, The planetary theory of Ibn al-Shāṭir, *Isis* vol. 50 (1959), pp. 227–35; Fuad Abbud, The planetary theory of Ibn al-Shāṭir: reduction of the geometric models to numerical tables, *Isis* vol. 53 (1962), pp. 492–9.

the *Almagest* which were so consistently paraphrased by Copernicus.[1] Nevertheless it seems to me useful to present some of the most central technical features of the Copernican theory of planetary motion and to look at their relation to the *Almagest* not from the viewpoint of philosophical principles but of elementary mathematics. The basic identity of the Copernican methods with the Islamic ones needs no special emphasis in each individual case. The mathematical logic of these methods is such that the purely historical problem of contact or transmission, as opposed to independent discovery, becomes a rather minor one. As I said before, all this could (or rather should) be well known. That in fact it is not needs no documentation.

1. Let me first make clear the technical basis on which we operate, a basis common to the planetary theory in the *Almagest* (about A.D. 150) and in the *De Revolutionibus*.

A practical simplification consists in the separation of the theory of longitudes from the consideration of latitudes by ignoring at first all orbital inclinations. Only after the longitudes are known were latitudes determined by tilting the respective planes into their proper positions. We shall discuss this latter part of the theory only in passing.[2] We will also not go into great detail about the theory of the Sun (including precession) and of the Moon.[3]

To simplify our presentation we speak about a "planet" when we mean the three outer planets and Venus. In this way we need not mention in every case the modifications which are required for the peculiar theory of Mercury; we shall deal with it more conveniently at the end (p. 98).

Finally I remind the reader that before Tycho Brahe a theory was considered adequate when its results agreed with observations within about 10 minutes of arc. In general we shall ignore here the problem of agreement of the ancient theories with the empirical facts as known to us and shall focus our attention almost exclusively on the relation between the mathematical methods of Ptolemy and Copernicus.

2. Let us for a moment assume that the orbit of a planet is strictly circular with respect to the Sun. Since it is our goal to predict the geocentric longitudes λ of a planet, it is convenient to transform the Earth to be at rest. Then it is trivial that the geocentric orbit of Venus is epicyclic; it requires only the construction of one parallelogram to see that the same holds also for the outer planets. The observable tracks of the planets confirm this general cinematic picture.

What is not obvious, however, and is a matter calling for much ingenuity and patient observation is the problem of determining the parameters of such a planetary model. The case of Venus should be the most simple: the radius r of the epicycle is directly obtainable from the maximum elongation θ of the planet from the (mean) Sun.[4] Assuming already the existence of a definite theory of solar motion which provides us for any given moment with the equation of center for the Sun, we know θ and hence r from $r = R \sin \theta$ where R is normed by Ptolemy as 60, by Copernicus as 10^4, representing the radius of any planetary

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[1] Cf., e.g., D. Price, Contra-Copernicus: a critical re-estimation of the mathematical theory of Ptolemy, Copernicus, and Kepler. *Critical Problems in the History of Science*, ed. M. Clagett, Madison, Univ. of Wisconsin Press, 1959, pp. 197–218.

[2] Cf. below, p. 103.

[3] Cf. below, p. 96 and p. 100.

[4] The modern reader should be warned that the "mean Sun" of ancient terminology moves with mean velocity *in the ecliptic.*

deferent. In the case of Venus it turned out that θ is not constant but depends largely on the solar longitude. Since it is plausible to assume that r and R are constant—in consequence of the circularity of orbits that was adopted *a priori*—one must conclude that the observer O is located eccentrically with respect to the deferent of center M. Through a systematic sequence of observations of maximum elongations, Ptolemy and his immediate predecessor Theon determined the solar positions for which θ appears as a maximum or as a minimum, hence locating the apsidal line and finding the amount of the eccentricity $e = OM$ (in terms of $R = 60$).

In the solar and lunar theory a simple eccenter (or a cinematically equivalent epicycle) seemed to suffice for the explanation of the inequality in the length of the seasons and of the intervals between lunar eclipses. But already Hipparchus (about three centuries earlier) had realized that this simple lunar model was defective in the quadratures though he was not able to bring order into the seemingly inconsistent empirical data. Here Ptolemy succeeded and established the laws which govern the inequality which is now known as "evection". It is well known that he spoiled his discovery by a hopelessly inadequate cinematic explanation.[1] But the interest paid to conditions in quadratures led to another

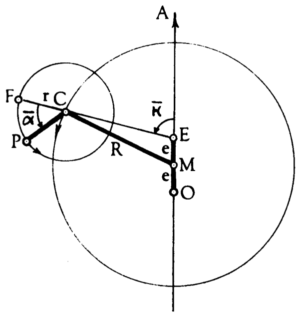

FIG. 1.

important discovery in planetary theory. Knowing for Venus the apsidal line, eccentricity and epicycle radius, one can easily predict the maximum elongation to be expected when the center C of the epicycle (or the mean Sun) is in quadrature to the apsidal line, assuming naively uniform rotation of C on the deferent, i.e. with respect to its center M. Ptolemy found, however, that the observed elongations require uniform rotation not about M but about a point E of the apsidal line located such that $EM = MO = e$. This point E is the famous *equant* (using a late mediaeval terminology), i.e. the point of the apsidal line from which the motion of C appears uniform (cf. Fig. 1).[2] For the modern reader it is not surprising that this concept played a crucial rôle in Kepler's attempts to account for the motion of Mars.

Ptolemy adopted the same cinematic principle also for the outer planets, of course combined with the trivial condition that the radius CP must always be parallel to the direction $O\bar{S}$ from the observer to the mean Sun. The eminently successful prediction of planetary positions computed on the basis of this model can rightly be considered as its justification, even if one had no longer such direct observations at one's disposal as in the case of Venus.

[1] For which cf. below, p. 100.

[2] None of our figures is drawn to scale; in particular eccentricities are greatly exaggerated.

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3. Without, for the moment, entering upon the question how to determine the parameters e and r for an outer planet (we will have to come back to this later (p. 102)), we may anticipate the results concerning the epicycle radii.[1]

As far as we know, Copernicus was the first clearly to understand that these radii are only different because the radii R of the deferents are all taken as unit. If one, however, uses the radius a of the Earth's (or Sun's) orbit as unit, then $a = R/r$ for an outer planet, and $a = r/R$ for an inner planet, provides us with the heliocentric distance of each planet. In this way we can compare both systems:

TABLE 1

r	Alm.	Cop.	mod.
Saturn	6;30	6;32	6;17
Jupiter	11;30	11;30	11;32
Mars	39;30	39;29	39;22
Venus	43;10	43;10	43;24
Mercury	22;30	22;35	23;14

TABLE 2

a	Alm.	Cop.	mod.
Saturn	9·231	9·175	9·539
Jupiter	5·217	5·219	5·203
Mars	1·519	1·520	1·524
Venus	0·719	0·719	0·723
Mercury	0·375	0·376	0·387

Table 2 represents the main contribution of Copernicus to astronomy: it opened the way to the determination of the absolute dimensions of our planetary system.

Surprisingly enough the problem of heliocentric distances is not at all emphasized by Copernicus. Of the numbers listed in Table 2 only the distance of Mars is explicitly mentioned (in V, 19); for Saturn and Jupiter one has to compute the mean distance from the extreme values. For Venus one finds only Ptolemy's value of r, and for Mercury one can compute a mean value $\bar{r}$ for the variable radius of the planet's orbit (cf. below, p. 99). A contemporary reader could scarcely get the impression that here lay the central core of the "Copernican System".

4. We now come to "l'abandon de l'équant". For the Moon one gets into trouble because no equant in the proper sense exists in Ptolemy's model. The mean motion takes place with respect to the Earth, the removal of which might be difficult, even for Copernicus.

As for the planets we shall now demonstrate that it was the goal of Copernicus' cinematic arrangements to *maintain* the equant, by no means to eliminate it.

For an outer planet Copernicus prescribes the following motion (cf. Fig. 2): the planet P moves on an epicycle of radius r' such that PC makes with CM the angle $\bar{\varkappa}$ when CM makes the same angle $\bar{\varkappa}$ with the apsidal line ($\bar{\varkappa}$ increases with the rate of the sidereal mean motion of the planet). The center M of the deferent has the eccentricity e_1 with respect to the mean Sun $\bar{S}$ about which the observer O rotates on a circle of radius r.

[1] For sexagesimal numbers I use a semicolon to separate integers from fractions, a comma for the separation of sexagesimal digits.

In order to relate this model to the Ptolemaic one, we transfer O to be at rest, using the familiar parallelogram construction, repeatedly applied by Copernicus and, of course, well known to all astronomers at least since Apollonius. Figure 3 shows in heavy solid lines the resulting structure, which differs from the Ptolemaic model only insofar as the planetary epicycle of radius r does not move with its center C_2 on the deferent and as the equant seems to be missing. In fact, however, the rule for the motion of C_2 is such that a point E on the apsidal line (cf. Fig. 4) at a distance r' from M' will always see C_2 at an angle $\bar{\varkappa}$ from the apsidal line. Hence E is the equant for C_2. Since C_2 is the center of the planet's epicycle the Copernican model would be *identical* with Ptolemy's if the path of C_2 were a circle.

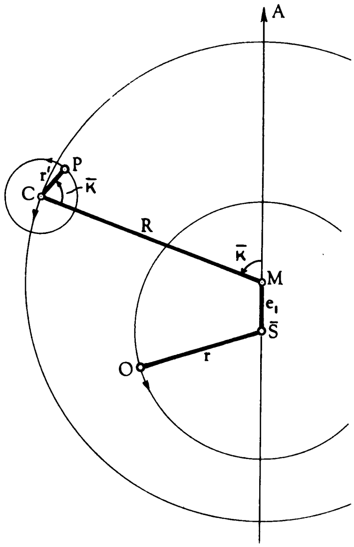

FIG. 2.

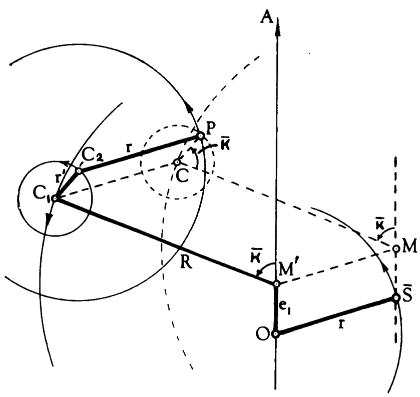

FIG. 3.

Copernicus proves that this is not the case,[1] as is easy to see if one considers, e.g., the situation at quadrature. But he does his very best (without saying so) to make the orbit of C_2 agree as closely as possible with the Ptolemaic deferent. To this end one must obviously require that

$$OE = e_1 + r' = 2e \tag{1}$$

where e is the Ptolemaic eccentricity $OM = ME = e$. Furthermore one will have exact agreement with Ptolemy's deferent in the apsidal line if (cf. Fig. 5)

$$(R - r') + (e - r') = R$$

i.e. if

$$r' = \tfrac{1}{2} e \tag{2}$$

and hence, because of (1),

$$e_1 = \tfrac{3}{2} e. \tag{3}$$

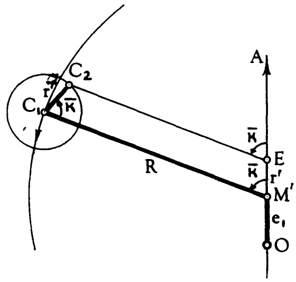

Fig. 4.

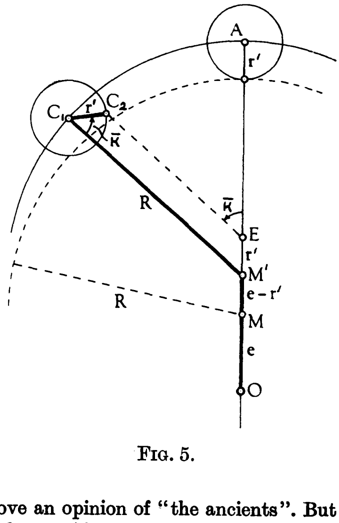

Fig. 5.

[1] Ostensibly in order to disprove an opinion of "the ancients". But in antiquity no such model was proposed and the only persons who would have been interested in this problem before Copernicus are the Muslim astronomers who invented this model. Indeed one finds in aṭ-Ṭūṣī (about A.D. 1270) the same proof as with Copernicus (cf. Tannery (below, p. 99 note 4), p. 351).

The relations (2) and (3) are indeed the relations which Copernicus (as well as aṭ-Ṭūsī and ash-Shāṭir) prescribes in relation to Ptolemy's eccentricities.

It is easy to see that this Copernican orbit of C_2 is in quadratures only about $e^2/2R$ wider than Ptolemy's deferent.[1] The angular displacement of C_2 as seen from O remains well below one minute for all planets. When Copernicus' recomputations of Ptolemaic longitudes occasionally result in differences of more than one minute, then the cause lies in the inaccuracy of the trigonometric procedures, not in the principle of the models.

In the case of Venus, Copernicus assumes that $\bar{S}$ is the mean Sun (cf. Fig. 6), C_1 at a distance e_1 from $\bar{S}$ is the center of a circle of radius r' which carries the center C_2 of the

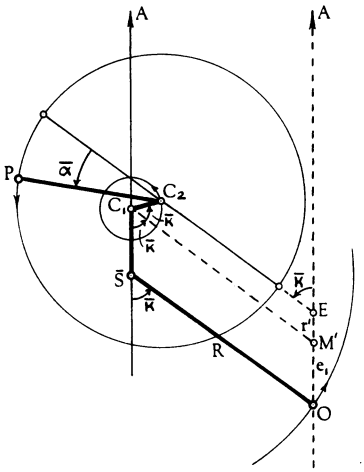

FIG. 6.

planet's orbit; all angles $\bar{\varkappa}$ increase proportional with time. Since $\bar{S}C_1C_2 = 2\bar{\varkappa}$ we see as before that E, at the observer's apsidal line OA, is the equant which controls the motion of the planet's orbital center C_2. This point, in turn, moves practically on the Ptolemaic deferent because

$$r' = 104 \quad e_1 = 312 \quad e \approx 208{\cdot}3 \text{ [2]} \quad \text{for} \quad R = 10^4$$

satisfy the conditions (2) and (3).

Since we shall find (cf. p. 98) that Copernicus preserved the equant also for Mercury we can now say that his aim was by no means to abolish the concept of equant, but, exactly as his Islamic predecessors, to demonstrate that a secondary epicycle is capable of producing practically the same results (thanks to the smallness of the eccentricities) as Ptolemy's equant. Though the resultant deferent is unfortunately not a circle, each component motion is uniform and circular. Both ash-Shāṭir and Copernicus considered this as their main achievement, even if the model had become more complicated than Ptolemy's.

Kepler was less philosophically prejudiced, and he not only reintroduced the Ptolemaic equants in the planetary theory but took the heliocentric approach seriously and hence

[1] Kepler remarked (*Werke*, vol. iii, p. 75) that he would not mind this construction if it only would make the deferent narrower, not wider, than a circle.

[2] For $R = 60$ $e = 1;15$ (*Alm*, x, 3).

provided also the (circular) orbit of the Earth with an equant,[1] an improvement which increased the accuracy in his determinations of the positions of Mars. Ptolemy's discovery of the "equant" was not only never abandoned but proved of the greatest importance for the construction of the "oval" orbit of Mars and hence of the Kepler ellipse.

5. The identity of the Copernican cinematic model with the Ptolemaic is obscured by many secondary features. Instead of using tropical longitudes, Copernicus always operates sidereally, with γ Ari as zero point, simply because this star is the first zodiacal star listed in the star catalogue of the *Almagest* (with $\lambda = 6;40$ $\beta = +7;20$). Hence all Ptolemaic longitudes are reduced by 6;40 and augmented by precession, which, however, is vitiated by a trepidation term. Furthermore the "mean Sun" (our $\bar{S}$) in the center of the Earth's circular orbit is not quite the mean Sun but rotates slowly about another center which, finally, has a fixed distance from the real Sun.[2] The motion of $\bar{S}$, which modifies the solar eccentricity and apogee, is unfortunately regulated by the same parameter which controls the oscillations of the true vernal point and simultaneously the obliquity of the ecliptic. The two latter motions are supposedly the consequence of a motion of the celestial poles along a figure-eight-curve made of two small contacting circles, the point of contact being the mean pole. In fact, however, this supposed motion of the polar axis does not induce the simple harmonic motion of the vernal point which Copernicus finally assumed (and which made Vieta so angry). Hence it is not at all simple to find out what the Copernican equivalent of a Ptolemaic coordinate should be and such a transformation is made still more arduous by the countless small computing errors, inaccuracies, and inconsistencies which mar all discussions in *De Revolutionibus*. Frequent shifts from sexagesimal parameters to decimals and back again do not increase accuracy. Finally angles which have a simple geometric significance are moved to $\bar{S}$ or C (of course simply parallel) and given new names. Hence it is not surprising that it is not at all apparent that the Copernican planetary tables are the direct equivalent of the Ptolemaic ones. But it is geometrically clear that this must be the case since we know that the Copernican model preserves the equant of the Ptolemaic theory (cf. Fig. 7).

In order to find the longitude λ of a planet P as seen from O (with respect to γ Ari or to ♈ 0°) one needs the angle η which appears at C as well as at $\bar{S}$. The position of the planet on its epicycle is defined by its "mean anomaly" $\bar{\alpha}$ or its "true anomaly" $\alpha = \alpha + \eta$, which are called "parallactic anomalies" when counted at $\bar{S}$. The equant guarantees that $OCP\bar{S}$ always forms a parallelogram and hence we have the same angle θ at O as well as at P. Hence λ will be given in both versions by

$$\lambda = \lambda_A + \bar{\varkappa} + \eta + \theta,$$

of course with proper signs for η and θ.

The tables in the *Almagest* (xi, 11) contain 8 columns, the first two for the arguments, the remaining six for functions which we denote as c_3 to c_8. The corresponding tables of Copernicus (v, 33) give four functions C_3 to C_6. Between these functions c and C there exist simple correspondences. Already Ptolemy abolished in this *Handy Tables* the tabulation

[1] Incidentally, Kepler was not the first to design a solar theory with an equant. Ibn ash-Shāṭir (around 1350) assumed a secondary epicycle which carries the Sun at an angular distance $2\bar{\varkappa}$ from the direction of the apsidal line (cf. Roberts, *Isis*, vol. 48, p. 429, Fig. 1). This is exactly the same device used in the theory of Mercury for the motion of the center C_2 of the planet's orbit (cf. below, p. 99, Fig. 9) and produces the same result, i.e. an equant located between the observer and the center of the deferent.

[2] It may be remarked that here Copernicus introduced into the solar theory exactly the same mechanism against which he polemicized on philosophical grounds in Ptolemy's lunar theory.

of two separate components c_3 and c_4 which together give the angle η as function of $\bar{\kappa}$. Copernicus follows, of course, the same practice, thus

$$\eta(\bar{\kappa}) = c_3(\bar{\kappa}) + c_4(\bar{\kappa}) \approx C_3(\kappa).$$

With $\alpha = \bar{\alpha} + \eta$ as argument Ptolemy forms

$$\theta(\alpha, \bar{\kappa}) = c_6(\alpha) + c_8(\bar{\kappa})\, c_5(\alpha) \quad \text{if} \quad c_8 \leqq 0$$

or

$$\theta(\alpha, \bar{\kappa}) = c_6(\alpha) + c_8(\bar{\kappa})\, c_7(\alpha) \quad \text{if} \quad c_8 \geqq 0.$$

Here $c_8(\bar{\kappa})$ is a coefficient of interpolation which increases in nearly sinusoidal fashion from -1 at $\bar{\kappa} = 0$ to $+1$ at $\bar{\kappa} = 180$; $c_6(\alpha)$ gives the angle θ when the epicycle is at mean distance, $c_6(\alpha) - c_5(\alpha)$ at maximum distance, $c_6(\alpha) + c_7(\alpha)$ at minimum distance. The above formulae indicate how θ is found for intermediary distances.

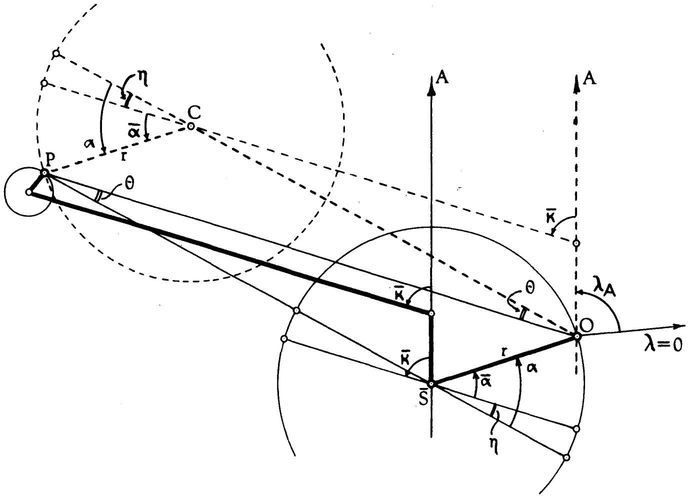

FIG. 7.

Copernicus modified this procedure by adopting a principle developed for Islamic tables, only to use positive corrections. Hence he finds θ from

$$\theta(\alpha, \bar{\kappa}) = C_5(\alpha) + C_4(\bar{\kappa})\, C_6(\alpha)$$

where $C_4(\bar{\kappa})$ increases from 0 at $\bar{\kappa} = 0$ to $+1$ at $\bar{\kappa} = 180$ because $C_5(\alpha) = \theta$ at maximum distance, $C_5(\alpha) + C_6(\alpha) = \theta$ at minimum distance. Hence

$$c_6(\alpha) - c_5(\alpha) \approx C_5(\alpha)$$
$$c_5(\alpha) + c_7(\alpha) \approx C_6(\alpha).$$

Table 3, excerpted from the tables for Saturn, shows how closely these relations are satisfied.

Obviously the Copernican tables will produce practically the same results as the Ptolemaic ones. Also the number of steps in computing a planetary longitude is the same in both systems.

TABLE 3

	Alm. xi, 11			*Revol.* v, 33		
	$c_3 + c_4$	$c_6 - c_5$	$c_5 + c_7$	C_3	C_5	C_6
30	3;6	2;42	0;19	3;6	2;42	0;19
60	5;29	4;49	0;35	5;29	4;49	0;35
90	6;31	5;53	0;41	6;31	5;52	0;42
120	5;49	5;21	0;42	5;49	5;22	0;42
150	3;24	3;13	0;26	3;24	3;13	0;26

6. Ptolemy's model for Mercury (cf. Fig. 8) assumes that the center N of the deferent rotates uniformly about a point M of the apsidal line whereas the center C of the epicycle is seen moving uniformly from an equant E which lies halfway between O and M. The resulting path of C brings the epicycle for $\bar{\varkappa} = \pm 120°30'$ (or a value very close to it[1]) nearer to O than at the "perigee" Π at $\bar{\varkappa} = 180$. In order to preserve these features Copernicus first sets out to keep the equant in its proper position between O and M. For this reason he has now to let the center C_2 of the planet's orbit (cf. Fig. 9) rotate with a phase 180° different from the other cases. Applying the same type of argument which we used before (p. 94) it can be shown that again the conditions (2) and (3)

$$r' = \tfrac{1}{2} e \qquad e_1 = \tfrac{3}{2} e = 3r' \tag{4}$$

should be satisfied if not only E but also A and Π should be kept in place. And indeed it is this relation (4) which Copernicus prescribes in *Revol.* v, 25, for his model of Mercury.

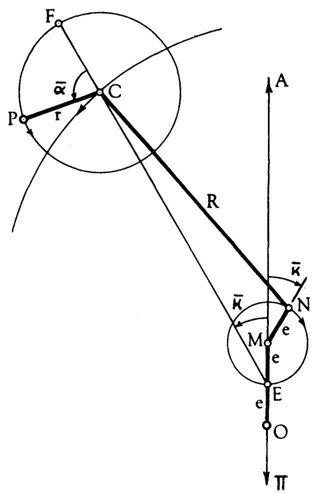

FIG. 8.

[1] W. Hartner has shown that this path is nearly elliptical; cf. *Vistas in Astronomy*, ed. A. Beer, vol. 1 (1955), p. 109. See also Hartner's article on "Mediaeval views on cosmic dimensions" in *Mélanges Alexandre Koyré*, vol. II, p. 268, footnote 25.

But if one looks at the subsequent calculations one finds that Copernicus uses not (4) but[1]

$$r' = 212 \quad e_1 = 736 \quad \text{Ptolemy: } e = 500.$$

It is possible to detect the reason for this change of parameters. In order to determine the remaining parameters of the model one could require the preservation of additional geocentric distances of the Ptolemaic model, e.g. at quadratures or at $\bar{\varkappa} = \pm 120$. It is not difficult to see, however, that this leads to unpleasant conditions for the motion of the planet with respect to the center of its orbit. Hence Copernicus abolished his original

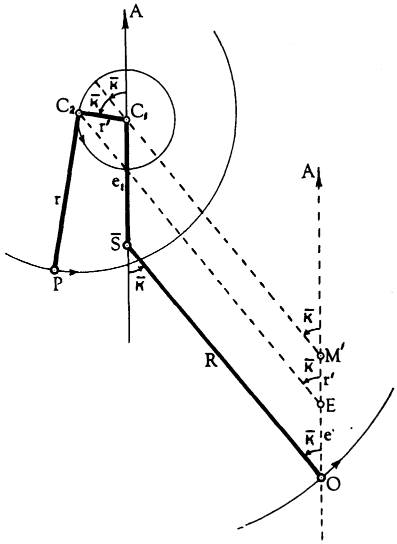

FIG. 9.

procedure (without saying so) and adopted an approach which is preferable also in principle. Ptolemy determined the parameters of his model from observations of maximum elongations, much in the same fashion as with Venus. What Copernicus now required for his model was that it should produce for $\bar{\varkappa} = 0, 180$, and ± 120 the same maximum elongations as Ptolemy's theory. Of course, Ptolemy's rotating deferent had to be replaced by a variation of the radius of the planetary orbit, or, in Copernicus' terminology, the planet had to perform a motion of libration along the radius of its orbit between fixed limits $\bar{r} + \tau$ and $\bar{r} - \tau$. For the mean radius $\bar{r}$ Copernicus took a close approximation of Ptolemy's epicycle radius r; then it is easy to show that the maximum elongations taken from the *Almagest* for the above-mentioned values of $\bar{\varkappa}$ determine the amplitude τ of libration. The motion of libration itself is again simple harmonic (as for the vernal point),[2] supposed to be generated by uniform rotations in the form devised by Proclus[3] or aṭ-Ṭūsī.[4]

[1] For unknown reasons sometimes also 211½ and 736½ respectively.

[2] Cf. above, p. 96.

[3] Commentary to *Euclid* I, Defin. IV. Cf. also transl. *Ver Eecke*, p. 96, n. 4.

[4] Cf. P. Tannery, *Recherches sur l'astronomie ancienne* (Paris 1893), p. 348. Also Kennedy-Roberts, *loc. cit.*, p. 231f.

Neither Ptolemy's nor Copernicus' machinery for the motion of Mercury could be considered plausible representations of physical facts; Copernicus himself had speculated in a loose fashion about an alternative mechanism.[1] It is difficult to see how devices of this type ever could have been taken for more than mathematical similes of no other significance than to guide the computations. I realize that one is supposed to be disgusted with Osiander's preface which he added to the *De Revolutionibus* (in keen anticipation of the struggle of the next generations) in which he, in the traditional fashion of the ancients, speaks about mere "hypotheses" represented by the cinematic models adopted in this work. It is hard for me to imagine how a careful reader could reach a different conclusion.

7. Ptolemy's model for Mercury is obviously inspired by the mechanism which he had invented in order to explain for the Moon in quadrature the increased equation of center (cf. above, p. 91). But the crank mechanism which properly enlarged the effect of the epicyclic anomaly also increased the lunar parallax and the Moon's apparent diameter to almost twice its actual value. Ptolemy kept silent about this obvious deficiency of his theory which accounted so nicely for the longitudes. But in late Islamic astronomy this defect was no longer accepted without question, and we now know that almost two centuries before Copernicus the device of a secondary epicycle was used by Ibn ash-Shāṭir.[2] The determination of the corresponding radius is trivial[3] since one has to do nothing more than to make the diameter of the second epicycle so large that the maximum equation increases from about 5° at syzygies to about 7;40 at quadratures, again simply accepting Ptolemy's data. Copernicus, operating on the same premises, reached of course the same result as the Muslim astronomers.[4]

The new lunar model had the great advantage over Ptolemy's that it kept the parallax under all conditions nearly within the limits prevailing at syzygies. Copernicus confirmed the new parallaxes by showing[5] that an occultation of Aldebaran by the Moon, observed in Bologna in 1497, was accounted for by his parallax. If one checks, however, Copernicus' computations, one finds errors in practically every step, even such obvious ones as a total of less than 180° for the angles of a spherical triangle. Fortunately the number of steps is large enough to make the total error insignificant. But the ancient lunar theory, assuming a fixed maximum latitude of 5°, also accepted by Copernicus, produced in the present case a latitude of only about −4;35, instead of −4;47. For the latitudinal parallax Copernicus found about −0;30 which moved the Moon down to Aldebaran, which he placed at a latitude of −5;10, a coordinate taken right out of Ptolemy's catalogue of stars. Had he checked it by observation he would have found the star at about −5;30 and his theory would have made the lower rim of the Moon pass almost 10 minutes above the star. Even worse, Ptolemy's latitudinal parallax would have moved the Moon from Copernicus' position right down to the star, hence supporting the ancient theory against the Copernican. In fact it was only a wrong Copernican lunar latitude in combination with a wrong Ptolemaic stellar position which "confirmed" the (essentially correct) Copernican parallax.

As is well known, Ptolemy's solar parallax was wrong by a factor of about 20. Since no direct measurement could possibly be made with the instruments of antiquity one followed a method, invented by Hipparchus, based on eclipses. Ptolemy had assumed that the Moon at maximum distance covers the Sun (at mean distance) exactly, both appearing under an

[1] *Revol.* v, 32.
[2] Cf. V. Roberts, quoted above, p. 89, note 4.
[3] Hence no "*mesure de... génie mathématique*". Cf. also the very simple discussion in *Revol.* iv, 8.
[4] Roberts, *loc. cit.*, p. 431.
[5] *Revol.* iv, 27.

angle of 0;31,20°. From a careful discussion of lunar eclipses he had derived for the diameter of the shadow a ratio 13/5 to the diameter of the Moon at maximum distance, the latter being at 64;10 earth radii.

The parameters accepted by Copernicus required some changes insofar as a common tangent to Moon and Sun would occur at a distance of the Moon of only 62 earth radii instead of Ptolemy's 64;10. For the shadow he considered a ratio 403/150 as more "convenient" than 13/5. Naturally one will ask: "convenient" for what? And why these special numbers? It is abundantly clear that Copernicus had no eclipse observation at his disposal giving him new information about the diameter of the shadow cone. Not only does he not adduce any such evidence but he gives in iv, 18 a very sketchy summary of Ptolemy's method how to deduce from eclipse magnitudes data for the shadow's diameter. The numerical data which Copernicus mentions are round numbers chosen *ad hoc* and so carelessly that they are excluded by Copernicus' own theory. Hence the ratio 403/150 must come from somewhere else. Indeed, since the distance of the Moon was changed from 64;10 to 62 Copernicus simply multiplied 13/5 by $\frac{64;10}{62} \approx \frac{31}{30}$ to get a "convenient" shadow. And the convenience of this transformation lies in the fact that this change is required to obtain practically the same distance for the Sun as before with Ptolemy's parameters. No wonder that Copernicus' conveniently doctored data produced a solar distance of 1179 Earth radii as compared with Ptolemy's 1210. Had he used his parameters without corrections he could have easily ended up with a considerably greater distance for the Sun than Ptolemy had found and this would have been rather unpleasant for a heliocentric system which had to face the absence of any fixed-star parallax.

Having obtained the conventional order of magnitude for the solar distance Copernicus happily reverted to the classical ratio 13/5 for the shadow. One can hardly think of a greater contrast in methodology as between Copernicus and Tycho Brahe, only one generation later.

8. Vieta, in his criticism quoted at the beginning, referred to the determination of planetary parameters from 3 mean and 3 true positions. Indeed all the astronomical models between Apollonius and Kepler had to solve a problem of this type. Since the circularity of the basic motion was taken for granted three points had to be determined to characterize a circle. Three observations provided two time differences which furnish two angles ($\bar{\delta}_1$ and $\bar{\delta}_2$) of mean motions, whereas the observer recorded two angles of true motion (δ_1 and δ_2). Then one faces the following problem: find the position of an observer who sees three points on a circle under angular differences δ_1 and δ_2 while he knows that they would appear from the center of mean motion under the angles $\bar{\delta}_1$ and $\bar{\delta}_2$. If the latter center coincides with the center of the circular orbit the problem has a unique solution obtainable by straightforward trigonometric operations. This is the case for the Sun (where δ_1 and δ_2 are right angles if one observes the equinoxes and a solstice) and for the Moon at lunar eclipses where the mean anomalies provide $\bar{\delta}_1$ and $\bar{\delta}_2$ at the center of the epicycle.

This simple situation no longer holds for a model with an eccentric equant of unknown eccentricity. Then it is easy to see that the four above-mentioned angles alone do not determine the problem. For the inner planets one need not worry, because the size of their orbits is directly observable at maximum elongation. This does not hold, however, for the epicycles of the outer planets. Only their centers are in principle observable at the moments of opposition of the planet to the mean Sun (cf. Fig. 10a). Unfortunately the mean distance $\bar{\delta}$ between two centers is measured at the equant E and $OE = 2e$ is one of the unknown quantities one wishes to determine.

The Copernican arrangement (Fig. 10b) faces exactly the same difficulty. The observations give only the angle δ between the directions $\bar{S}O_1P_1$ and $\bar{S}O_2P_2$. But $\bar{\delta}$ is measured at M' and we do not know $e_1 = \bar{S}M'$ or $r' = \frac{1}{3} e_1 = CP$.[1]

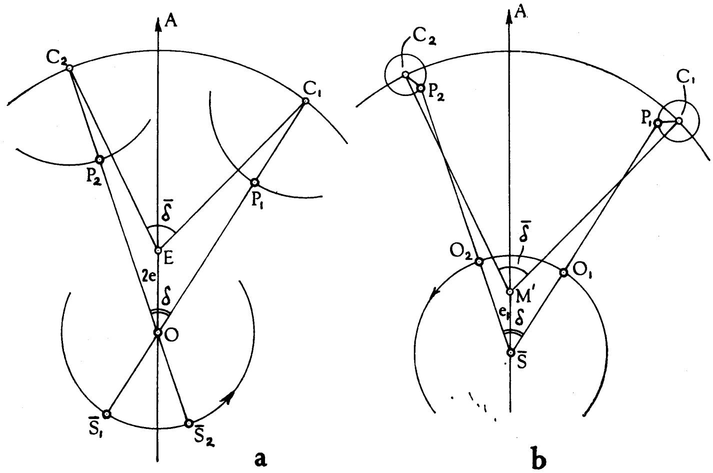

FIG. 10.

Ptolemy solves his problem by an iteration process. He assumes as first approximation that the equant coincides with the center M of the deferent and thus returns to the mathematical problem familiar from the lunar theory. This leads to an approximately correct position of the apsidal line and an approximate eccentricity. Now one can find by how much (ε) the observed angle δ had been falsified because of the identification of E and M. Hence a second approximation can be computed with $\bar{\delta}$ and $\delta + \varepsilon$ as given angles, and so forth until the results become stable. Ptolemy did not compute many steps: three for Mars, only two each for Saturn and Jupiter. He gives, of course, no proof of convergence and is satisfied to show that the last obtained parameters explain the observational data.

Copernicus admittedly did not bother to understand this iteration method which he simply characterizes as a "multitudo numerorum". All that he does is to repeat Ptolemy's final test with the parameters of his own model, obtaining exactly the same results (as was to be expected from the beginning).

A different situation arises when Copernicus sets out to repeat the determination of the parameters of his model on the basis of observations in his own time. As we have said before, he would have to face exactly the same mathematical difficulty as did Ptolemy. But such a systematic approach is foreign to him and hence he maintains as much as possible of the Ptolemaic parameters and modifies the position of the apsidal line by trial and error, knowing of course from contemporary astronomy how much displacement could be expected.

9. The rigid adherence to Ptolemaic methods deprived Copernicus of one advantage where the heliocentric approach is definitely superior to the geocentric one, i.e. in the

[1] It should be noted that the relative positions of the points $\bar{S}$, O, P are exactly the same in Figs. 10a and 10b.

theory of latitudes. Since the orbital planes go through the Sun, the assumption of deferent planes which go through the Earth produces inconvenient effects. Without eccentricities it would be correct to move the plane of the epicycle parallel to itself along the deferent at fixed inclination. In fact, however, a Ptolemaic eccentricity is the vector sum of the eccentricities of Earth and planet, and this vector lies neither in the ecliptic nor in the orbital plane. It is not difficult to see that this situation, unknown to Ptolemy, is the cause of the vibrations which he was forced to introduce into his theory of planetary latitudes in order to account for the observations. Since the Copernican theory is only a formal transformation of the Ptolemaic theory, Copernicus ends up with the same secondary vibrations of the orbital planes which he assumed to go through the mean Sun. As Kepler put it,[1] Copernicus did not know how rich he was.

It is surprising to see that it did not disturb the protagonist of a Universe in which the earth was only one of six planets that five of them entered in an "agreement with the center of the earth"[2] to nod with the frequency of the Earth's rotation. And because every celestial motion had to be mechanized by means of uniformly rotating circles, Copernicus attached to each orbital plane a perpendicular little circle inside of which rolled a second circle such that the orbits would move up and down in simple harmonic motion.

10. If one reads Copernicus only superficially and with the conviction that he had abolished, or at least greatly simplified, the Ptolemaic system, one will not be tempted to study the *Almagest* in any detail. Vieta, of course, still knew better. He must have been fully aware of the fact that there was not a single proof or mathematical procedure in the *De Revolutionibus* which did not have its exact replica in the *Almagest.* To Vieta as one of the leaders in the new trend of mathematics it must have appeared rather antiquated when Copernicus again and again demonstrated by numerical computation that his model agreed with Ptolemy's.

Modern historians, making ample use of the advantage of hindsight, stress the revolutionary significance of the heliocentric system and the simplifications it had introduced. In fact, the actual computation of planetary positions follows exactly the ancient pattern and the results are the same. The Copernican solar theory is definitely a step in the wrong direction for the actual computation as well as for the underlying cinematic concepts. The cinematically elegant idea of secondary epicycles for the lunar theory and as substitute for the equant—as we now know, methods familiar to a school of Islamic astronomers—does not contribute to make the planetary phenomena easier to visualize. Had it not been for Tycho Brahe and Kepler, the Copernican system would have contributed to the perpetuation of the Ptolemaic system in a slightly more complicated form but more pleasing to philosophical minds.

[1] *Werke,* vol. iii, p. 141, 3.

[2] Rheticus in the *Narratio Prima;* text, e.g., Kepler, *Werke,* vol.i, p. 125, 2f.; translation in Rosen, *Three Copernican Treatises* (1959), p. 183.