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COPERNICUS, THE ORBS, AND THE EQUANT

ABSTRACT: I argue that Copernicus accepted the reality of celestial spheres on the grounds that the equant problem is unintelligible except as a problem about real spheres. The same considerations point to a number of generally unnoticed liabilities of Copernican astronomy, especially gaps between the spheres, and the failure of some spheres to obey the principle that their natural motion is to rotate. These difficulties may be additional reasons for Copernicus's reluctance to publish, and also stand in the way of strict realism as applied to *De Revolutionibus*, although a realistic astronomy may be envisioned as a goal for Copernicus's research program.

In the previous paper André Goddu presents Duhem's views on the progress of science, connecting his views on Copernicus with some recent scholarship and examining the varieties of realism this renders possible. I want to suggest that this recent work on Copernicus misses certain important considerations by unduly emphasizing mathematical astronomy over cosmology and physics. The same issues have consequences for realism, and display valuable features of Duhem's image of science.

When Copernicus explains his motives, in the preface to *De Revolutionibus* addressed to Pope Paul III, he contrasts his views with two other schools of thought but he criticizes these alternatives on different grounds. The homocentric models revived by Amico and Fracastoro fail to show numerical agreement with positional data. On the other hand, although the eccentric models of the Ptolemaic tradition do show numerical agreement, they "contravene the first principle of regularity of motion" (Copernicus 1976, p. 25). As this objection is not raised in the case of the homocentric models, we may conclude that they do not contravene this principle, which concerns motion, the basic subject matter of physics as defined by Aristotle, not just astronomy or the motions found in astronomy.

The objection to the Ptolemaic tradition concerns the nature of the motion required by the equant. There seems to be no difficulty with the equant, regarded as a constraint on the motion of a point in a two-dimensional mathematical construction used for calculating planetary

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longitudes. If there is a difficulty it is because of the connection of the motions with physics, and the "first principle" invoked by Copernicus is an attempt to express the difficulty. Modern commentators have not quite got hold of the problem. Swerdlow for example describes the equant as an offense against "simple mechanical sense" (1972, p. 36). In the work cited by Goddu, Swerdlow and Neugebauer describe the objection to the equant as being on "physical or mechanical rather than on merely philosophical grounds" (1984, p. 290). These remarks must be understood ahistorically. There was no canonical mechanics for early modern scientists to draw upon for such judgments. To understand the equant problem, then, we need to examine the physics of motion employed by Copernicus.

Copernicus evidently expected his audience to be familiar with Aristotle's account of motion.[1] The aspects of the account relevant for understanding Copernicus's objections to Ptolemy are: all bodies move, either because they are subject to their own internal source of motion, or because they are moved by something else, which in its turn moves on account of an internal source. Further, a body may only produce motion in another body when the two are in contact. Copernicus is attempting to give an account of the motion of the planets, which are not themselves endowed with a source of motion.[2] Therefore planets are moved by something else.

In *De Revolutionibus*, Book I, Chapter Four, Copernicus discusses stations and retrogressions as apparent irregularities in the paths of the planets. Planets also appear sometimes nearer and sometimes farther from the earth. "Nevertheless", he goes on, "it must be admitted that their motions are circular, or compounded of a number of circles, because they pass through irregularities of this kind in accordance with a definite law and with fixed returns to their original positions, which would not happen if they were not circular" (1976, p. 39). But planets do not move themselves; they are moved by something else. What other object is capable of moving a planet in indefinitely repeating circles? Chapter Four begins, "The next point is that the motion of the heavenly bodies is circular. *For the movement of a sphere is a revolution in a circle*, expressing its shape by the very action, in the simplest of figures, where neither beginning nor end is to be found, nor can the one be distinguished from the other, as it moves always in the same place" (1976, p. 38, emphasis added). Hence we are to understand that the planets move in circles because they are moved by spheres. Their motion is uniform for the same reason: "Circular motion always goes round evenly, for its cause is unfailing" (1976, p. 45), this cause being for Copernicus the natural tendency of spheres.[3]

To understand Copernicus's position we need to concentrate not on the circles generated by the spheres, or their mathematical properties, but on the nonmathematical concepts of motion and the perfection of the heavens. If we are to locate these concerns according to the dichotomy of mathematical astronomy versus cosmology, then these are clearly cosmological concerns. We are obliged to conclude that Copernicus's main announced criticism of Ptolemy, the inadmissibility of the equant, is a cosmological objection, and in this respect it is identical to the criticisms of the homocentric theorists. The equant is only a problem because planets have to be moved by uniformly rotating spheres. Spheres may rotate uniformly only about axes which are diameters, but the motion required by the equant could be created only by a sphere rotating uniformly about an axis that is not a diameter, a condition impossible to satisfy.

It is a matter of current controversy whether Copernicus believed in the reality of the celestial spheres familiar in the Ptolemaic tradition.[4] There is admittedly no unequivocal statement in *De Revolutionibus*, a point usually counted in favor of those who deny that Copernicus accepted spheres, although the passages I have already quoted (among others) are difficult to interpret in any other way. But the most telling point is that the equant problem is not intelligible except as a problem about real spheres. General historical evidence tends to the same conclusion. Whatever his personal views, Copernicus must have expected his audience to accept the reality of the spheres. Throughout his lifetime the only major astronomical traditions (the Ptolemaic tradition as developed by Puerbach and Regiomontanus, and the less developed alternative tradition of homocentrics) assumed the existence of celestial spheres.[5] Both the text and the context of Copernicus's work suggest that he accepted the reality of the celestial spheres, and this commitment suggests additional reasons for his reluctance to publish, in addition to those adduced by Goddu.

If Copernicus's system is interpreted by means of the physical construction familiar in the Ptolemaic tradition, a major problem appears – the spherical shells are no longer in contact.[6] There are huge gaps between them. There is an even larger gap between the spherical shell of Saturn and the fixed stars. And there is a discontinuity at the center of the system. In the Ptolemaic system as developed in the *Planetary Hypotheses*, or the *Theorica* tradition, or even in the rival homocentric models of Fracastoro and his contemporaries, the innermost sphere of the heavens is continuous with the uppermost sphere of the terrestrial elements. The terrestrial elements form a further series of nesting spheres which is continuous all the way to the center of the system. There is no empty space. Once the center is moved to the Sun,[7] the first of the spheres of the heavens, counting outward from the center, is the sphere of Mercury. The size of the Sun is known to be much smaller than Mercury's sphere. What fills the region between?

Copernicus's cosmological model has gaps. These difficulties are independent of the success of the model in calculating planetary positions, and to that extent are conceptual rather than empirical. They are no different in kind from those that motivated Copernicus to seek an alternative to Ptolemy. And in fact there are other similar difficulties.[8] For Copernicus, the natural motion of a sphere is to rotate, but the Moon is a sphere, and it does not rotate.[9] The fixed stars are also confined to a sphere, and that sphere does not rotate. The same problem arises for the Sun, although whether or not it rotates may be an open question. Adding all these potential difficulties to the problem of the gaps between the spheres provides considerable additional grounds for Copernicus's reluctance to publish.[10]

Copernicus's lack of a detailed (as we would say) physical model for heavenly motion makes his realism hard to appraise. Perhaps we need to see realism not as a global requirement which must be satisfied by any theory, but rather as a goal to be reached if a series of related theories succeeds. The hope is that more and more of these difficulties will be resolved by later work, as Galileo resolved the problem of brightness variation for Venus by demonstrating the phases.[11] Sometimes, of course, the resolution takes the theory in a new direction, as in the case of Kepler's solution to the problem of the gaps between the spheres. We might say that Kepler's work made a realist interpretation of Copernicus's theory untenable.

Contemporary philosophers and historians are too likely to treat science as an activity in which most of the time things go right, as the paradigm of rationality or an activity so successful that its success needs explaining. Much contemporary concern with realism in science is, I think, a consequence of this underlying conception, as is the persistent tendency to ignore or underemphasize the anomalies which are as much a part of the scientist's working environment as the positive evidence for the current theory. It would be as great a mistake to describe science, as a historical phenomenon, without anomalies, as it would be to describe monarchies, as an historical phenomenon, without succession problems, or democracy, as an institution, without faction fights. The assumption of success is fatal as an approach to understanding the history of science.

Duhem saw that the history of science is largely the history of error, of failed theories and abandoned positions. The history of science is like the history of life on earth – extinct theories outnumber the survivors. Duhem accommodated scientific failure by relegating realism to the realm of metaphysics. As Goddu shows, Duhem allowed realism only when science and metaphysics coincide at a theoretical endpoint. Interestingly this coincidence is to be recognized by 'bon sens' a nonlogical faculty of judgment. It is salutary to be reminded by Duhem that there are sources of knowledge that resist logical analysis. I would even locate some of them inside science itself (e.g., tacit knowledge). If we are unable to endorse Duhem's solution to the problem of scientific failure, the response need not be a global realism, but a historically relativized realism – the realism of medium term theoretical success, which may be withdrawn in the long term. But even in locating the problem, Duhem's history and philosophy of science is more sophisticated than much of what has passed for historical and philosophical analysis of science during the twentieth century.

NOTES

[1] He refers, for example, to Aristotle's threefold division of simple motions as if he expects his audience to already understand the doctrine (Copernicus 1976, p. 45).

[2] Although ancient Stoics took the contrary view, and there was new interest in their scientific ideas during the sixteenth century (Barker 1985; Barker and Goldstein 1984, 1988). The possibility that each sphere was endowed with an intelligence capable of self-movement was also debated in the Middle Ages (Weisheiphl 1985, chap. 7).

[3] For a very different reading of these passages, see Jardine (1982).

[4] Swerdlow (1973, 1976) had very much the best of an exchange with Rosen (e.g., 1975), in which he affirmed the reality of Copernicus' spheres against Rosen's denials. Other important points appear in Jardine (1982) and Westman (1980), the latter taking a middle position. I propose modifying Westman's position in the direction of Swerdlow.

[5] For Puerbach see Aiton (1987); on the homocentric theorists see Swerdlow (1972).

[6] For the original Ptolemaic 'nested sphere' model see Goldstein (1967). On later knowledge of the model see Van Helden (1985). On Copernicus's calculations of planetary distances compare Van Helden (1985) with Neugebauer (1968).
[7] Strictly speaking the center is moved to the *mean* Sun.
[8] Other difficulties are more plainly empirical. Copernicus's theory accounts for the observed pattern of variation in brightness for Mars, but also requires a similar variation in brightness for Venus – and this is not apparent to a naked eye observer. This problem was eliminated by Galileo's 'discovery' of the phases of Venus (Ariew 1987). Similarly, although Copernicus avoids the dramatic variation in the apparent size of the Moon predicted by Ptolemy's theory, his own theory employs not one but two epicyclic motions and will not keep the same face of the Moon always turned towards the earth.
[9] This seems to have been the majority position prior to Copernicus. A minority of cosmological commentators recognized that a rotation of the moon might compensate for the kind of epicyclic motion mentioned in the previous note (Gabbey (forthcoming); Grant 1987).
[10] Although these defects of the Copernican theory seem compelling to the modern reader, considerably more argument would be needed to establish that Copernicus himself would have seen either these difficulties or the empirical problems mentioned in note 8 as major defects. In particular, it would be important to consider whether Copernicus saw his astronomy as a system in the modern sense. For more on this problem see Barker and Goldstein (1988).
[11] See note 8 above.

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