A Response to Peter Barker and Matjaž Vesel, 'Goddu's Copernicus'*

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The review of my study of Copernicus with extensive and careful summary by Peter Barker and Matjaž Vesel is generally constructive and edifying. By relying in part on their summary, I am optimistic that some of their questions can be answered and their criticisms met satisfactorily. Naturally, there are still other matters about which we will disagree but even in those cases we can hope for greater clarity. I am grateful to the Editor for this opportunity.

The Big Questions

I begin with the 'big questions that go unanswered' [319] just before their critical evaluation since these questions frame the criticisms that follow.

A. 'Where and why did Copernicus begin his research into heliocentrism?'

This question anticipates the outcome. The outcome followed from Copernicus' critique and rejection of geocentrism. The questions that led him to that critique had arisen already in Cracow (1491–1495); further reading and analysis brought him, probably by the end of 1509 at the latest, to the conclusion that geocentrism could not resolve the main problem that he thought astronomy should be able to resolve. I will return to this 'conclusion' later. My study [2010] addressed these issues on pages 225–229, 243–261, 285–291, 326–332, and 358–360. That my answer was not clear, however, is evident from the reviewers' question and doubts. I will summarize the argument below in the course of discussing alternative answers.

* See http://ircps.org/aestimatio/9/304-336.

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B. ‘Why are there so many similarities between his work and the work of Islamic astronomers?’

The question presupposes the answer. As far as we know, Copernicus learned first from Regiomontanus’ *Epitome* that Arabic intermediaries had made important observations different from Ptolemy’s and had expanded the observational record in ways that necessitated changes in some of Ptolemy’s sidereal, solar, and lunar models. Copernicus recognized problems with Ptolemy’s lunar model and the equant while in Italy, although his teachers in Cracow had also concluded that the equant was entirely fictional. Copernicus’ own observations and reading of the *Epitome* probably reinforced his doubts; but it is possible that he encountered Persian/Arabic critiques and perhaps even saw models that suggested alternative ways of solving these problems. Again, I will elaborate below. My study [2010] addresses this question on pages 154–156, 261–272, and 476–486; but the reviewers’ criticisms will allow me to quote my own disclaimer and to emphasize my agreement with the claim that the Maragha hypothesis provides the most complete version of the models that Copernicus could have adopted.

C. ‘Did he really select a methodology that would itself have been predictably unpersuasive to contemporaries?’

Copernicus knew that he was contradicting common sense. Without an observation to correct the perception of celestial motion and of a static Earth, and without a fully developed physical theory to support an alternative account of such perceived motion, he tried to raise questions, provoke doubts, and open minds. He had no control over openness and receptivity but he could construct an attractive alternative and he seems to have been optimistic that the vision sustaining his argument would eventually prevail. But, in the short term, he had every reason to be pessimistic. His delay and hesitation are well known and it seems that Rheticus’ enthusiasm, the support of Tiedemann Giese, and the publication of the *Narratio prima* were critical in convincing Copernicus to complete and publish his work.

D. ‘Although Goddu presents Plato as a key source of Copernicus’ dialectical method, why should Copernicus be seen as working within an Aristotelian tradition and addressing Aristotelians, rather than working within a nascent Platonic tradition and addressing Platonists?’

Here the reviewers seem to have forgotten their own summary [308] momentarily:

> For Copernicus, the Aristotelian tradition is a long way from Aristotle [Goddu 2010, 93]. According to Goddu, we should not expect to find any defining content, commentary tradition, or school at Cracow. [95]

Some of the reviewers' generalizations here require qualification. Copernicus' relation to the Aristotelian tradition, rather than being defined by a tradition of commentary and received doctrine, was rather a relation

> to his teachers and of his teachers to the 'schools' and texts on which they drew for their interpretations of natural philosophy. [Goddu 2010, 95]

Their perception [308] that I portrayed the Cracow *milieu* as 'generally hostile to Plato' is not quite accurate. I cautioned readers about how 'scholastic philosophers modified Aristotelian doctrine under the influence of Platonically inspired arguments or questions' [135]. There is need for additional clarification here.

First, in his critique, Copernicus addressed geocentrists, whatever their doctrinal allegiances. Second, the Aristotelian tradition to which he reacted and responded was an Aristotelianism often reconciled with Platonic, Middle Platonic, and Neoplatonic critiques. The dichotomy between Aristotle and Plato is anachronistic. Copernicus' own rhetorical strategy in *De rev.* 1 guided my reconstruction. In chapters 4–7, he addresses all geocentrists. But chapters 8 and 9 are directed specifically at Aristotelians by using Aristotelian categories (such as circular *versus* rectilinear, natural *versus* violent) that Copernicus had re-conceptualized. True, Copernicus makes the terrestrial celestial but he was cautious about making the celestial terrestrial.

In short, he did address Platonists but the conceptual categories were Aristotelian; yet, these were already modified by other sources on whom Copernicus relied, including Cicero, Plutarch, Pseudo-Plutarch, and Pliny, among several others. Platonism, Middle Platonism, and Neoplatonism influenced medieval scholastic Aristotelianism deeply, as my study documents [2010, 89–136]. Johannes Philoponus and Proclus, in particular, influenced medieval Aristotelianism and there is strong evidence that Copernicus relied on Philoponus (anonymously) for his doctrine of natural elemental motion [see, e.g., Knox 2005; Goddu 2010, 396, 490–491]. Given the reviewers' strong realism concerning spheres and orbs, they should have qualified their recommendation of the study by Anna de Pace [2009]. De Pace offers some potentially significant suggestions about Copernicus' sources but it comes at the cost of an exaggerated Platonic and thoroughly anti-Aristotelian reading of Copernicus [see Omodeo 2011, Goddu 2011, De Pace and Goddu 2012]. I agree, however, that we need reliable studies of Renaissance Platonism and Neopythagoreanism and their relation to Copernicus. I made some tentative suggestions along these lines in 2010, 317–320 and I will return to them below.

The Critical Evaluation

I take up and re-order the reviewers' criticisms in the following sections as follows:

(a) overreaction to my doubts about Persian/Arabic predecessors,
(b) affirmation of total spheres but silence about partial orbs,
(c) John of Glogovia and Albert of Brudzewo,
(d) Capellan and Tychonic alternatives and the origin of Copernicus' heliocentrism, and
(e) concluding reflections about texts, sources, and speculation.

A. Overreaction

Readers who know only the review of my study may be surprised to read the following sentences from my 'Excursus on Transmission' on pages 476–486 regarding the hypothesis about Islamic predecessors:

> All of that said, scholars convinced of the hypothesis should continue to search for the intermediary link. It may yet turn out to be correct, and the fact remains that the Maragha hypothesis still provides the most complete version of the models that Copernicus could have adapted both in the *Commentariolus* and *De revolutionibus*. I would welcome the discovery for it would finally put all of the speculation, including mine, to rest. [2010, 485]

Yet, in an article cited in his and Vesel's review, Barker says:

> Goddu has now followed Rosińska in emphasizing Brudzewo as a possible origin for the Tusi device in Copernicus, in part of a general attack on the plausibility of Islamic sources for Copernicus' work. [Barker 2013, 137]

In the footnote, he refers to the pages of my study and the 'Excursus'.

Did the reviewers overlook the disclaimer quoted above? Did they judge it disingenuous? Although it was not my intention to be misleading, perhaps I did not make my points clearly enough. If so, then I add the emphasis that should clarify my meaning: I too regard the hypothesis of an Islamic source as providing the most complete and concrete reconstruction. The 'attack' (not my word) was against dogmatism. My pleas were for a more concerted search for the route of transmission and for keeping an open mind while considering other options. The neglect of my disclaimer and of some other details was not intentional or motivated by hostility, I believe, though this overreaction tends to confirm some of the concerns voiced in my excursus.

I regret these comments because the reviewers have done me the generous consideration of having summarized my study so extensively and carefully. Naturally, in such a long book, it is inevitable that they mischaracterize some of my views and beliefs. I did talk about the alleged source as possibly a 'ghost' but even I consider that possibility as unlikely. Perhaps more effort has been made to find the route of transmission than has appeared in print so far. What we need is communication about such efforts and a complete report of failures as well as of promising paths to pursue. Why has so little been reported about the provenance of MS Vat. Gr. 211? What inventories, catalogues, and collections have been examined thus far and what remains to be examined? In 1973, Swerdlow [426] asked similar questions and he seemed confident that something would eventually turn up.

In the same spirit of discussion, there are additional remaining questions about the sources that have been proposed and their similarity with Copernicus' models. I address a few of them again below.

The reviewers' criticism [327–329] of Mario di Bono's claims [1995] about the figure in the*Tadhkira* I will leave for Di Bono to answer and note only the reviewers' assumption that, whatever source Copernicus saw, he would have recognized both versions of the Tusi couple. Without knowing what Copernicus saw, read, or heard, how do we know his interpretation without assuming what we need to prove? This seems to me to be the kernel of Di Bono's analysis. Copernicus applied and elaborated the devices in ways that no other predecessor did.

I agree that there are important similarities between the models and the lettering. I also note, however, some questions. The lettering in Copernicus' autograph is similar but the figure that he drew there is less similar than the one from the 1543 edition of his work [Goddu 2010, 268 Figure 4; Barker and Vesel, 328 Figure 2]. Copernicus drew the figure in the autograph [fol. 75r] on Paper C, which he may have exhausted as early as 1525 [see Zathey 1972, 2–7; Birkenmajer 1900b, 368] but probably no later than 1528 [see Swerdlow and Neugebauer 1984, 1.88]. He or Rheticus drew the figure that appears in the edition possibly no earlier than 1542. Why is there a difference between the figure in the autograph and the one in the edition of 1543? Why was it changed and what was the source for the second one? It would appear that Copernicus may have relied on more than one source and that the second figure may have been derived second-hand from another source.

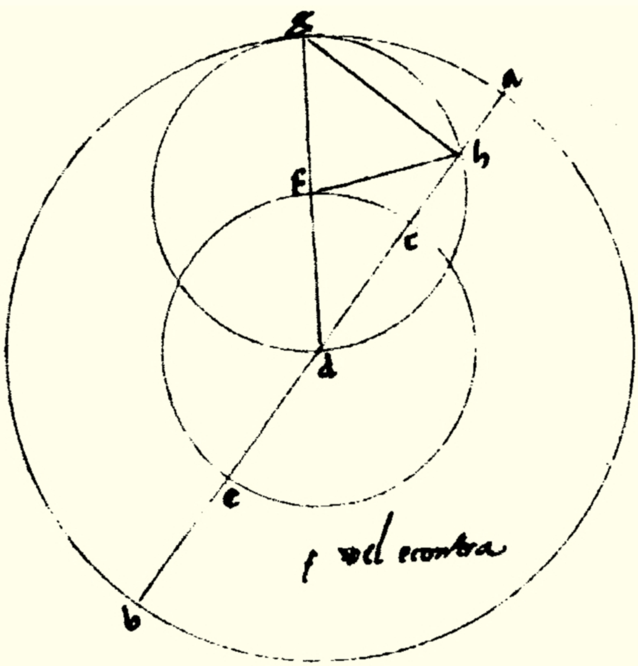

Figure from Copernicus' autograph, fol. 75r

The reviewers argue for Brudzewo's influence on Copernicus. Why, then, do they ignore Brudzewo's version of the reciprocation device that Birkenmajer, the editor of the text [1900a, 120], connected with Copernicus' so-called 'libration mechanism'? I will return to this question in the section on John of Glogovia and Albert of Brudzewo.

The reviewers rightly cite articles on Moses Galeano by Langermann [2007] and Morrison [2011] that propose another avenue of research. The Hebrew manuscript reporting the system of Ibn-al-Shatir, cited by Langermann, is from 1539 but the text itself was composed around 1500 when Galeano visited Venice; and Langermann suggests personal contact through Hieronymus Soncino's network as possible. Together with Morrison's analysis of an astronomical text by Galeano, this gives reason to search for other avenues of transmission in northern Italy connected with the circle of Regiomontanus. Aside from the additional evidence that Arabic texts or ideas in some form were in Italy, scholars may find more concrete evidence by examining early inventories, catalogues, and, best of all, by actually visiting archives and libraries and examining the contents of codices, which often contain surprises. We must also consider networks of communication, for which Domenico Maria Novara may have been helpful and which Copernicus may have utilized while in Bologna or Padua. In short, we still need more information about sources or, at least, more concrete evidence of their existence, provenance, and Copernicus' access to them. The recent efforts provide grounds for optimism. My excursus was, contrary to scepticism, a plea for more research and a plea to keep an open mind by considering alternatives.

I sympathize with the frustration expressed by Ragep [2007] over the delay in accepting the role of medieval Islamic astronomy in the European Renaissance of astronomy. But here it is important, perhaps, to recall how long it has taken for the story to develop. Some information about the Maragha achievements was known already in the 19th century. It took nearly another 100 years until the 1950s for more details to emerge and even within the last decade there have been more discoveries that hold out promise for a resolution. It was not my intention to denigrate any such hypothesis because it is not European. I am not satisfied that enough has been done to confirm the route of transmission and, however aggravating that may be to proponents of a non-European origin of Copernicus' models, I think that we should keep an open mind and consider a variety of options and multiple sources.

B. Affirmation of total spheres but silence about partial orbs

The reviewers' reconstruction [321–327] of my argument is flawed. Edward Rosen's earlier polemic was directed mostly at materially solid spheres, not spheres altogether, and it was motivated by Copernicus' silence about the penetration of spheres. Perhaps because some of his later comments are buried in footnotes, it seems that many have overlooked them. In any case, Rosen qualified his earlier assertions about the reality of spheres and orbs. In his revised translation of the *Commentariolus* [1985], he cited his earlier version and in a very long footnote [1939, 122–126n326] emphasized the problems with materially solid spheres; Copernicus' reliance on circles (*circuli*) not *sphaerae*; the misrepresentation of his [Rosen's] views about spheres as fictions with no physical existence; and the uncertainty that Copernicus found in his sources, especially Georgio Valla [1501], about eccentric and epicycle orbs. Even in his earlier version, Rosen acknowledged:

> From Copernicus' language it sometimes appears that he regarded the planet as attached to a three-dimensional sphere; but more often a two-dimensional great circle of the sphere was the geometrical figure to which he affixed the planet. [1939, 11–12]

Notice that even the second clause implies the existence of the total sphere. Likewise, in translating *De rev.* 1.4.15–16,

> Several motions are discerned herein, because a simple heavenly body cannot be moved by a single sphere non-uniformly.

Rosen commented: 'This pronouncement makes unmistakably clear Copernicus' adherence to a form of the traditional doctrine of the spheres' [1978, 348]. Rosen later again commented:

> The spheres intended by Copernicus were invisible carriers of the visible planets...in the time of Copernicus (and long before him) a visible planet was thought to be attached to an invisible sphere (*orbis*) that transported the visible planet. These invisible spheres performed the revolutions mentioned by Copernicus in the title of his *Revolutions*. [1984, 62]

It is clear, then, that Rosen did not deny spheres altogether but concluded that Copernicus' spheres were invisible and immaterial. Rosen did not elaborate further but he was evidently referring to the real total sphere in which other orbs and circles are contained. Copernicus' predecessors and contemporaries disputed the reality of the partial orbs. Copernicus himself neither affirmed nor denied the existence of partial orbs. Rosen rejected the material solidity of spheres on the ground that the descriptions would attribute terrestrial qualities to celestial entities. Later Copernicans or interpreters of Copernicus have adopted a variety of views about the celestial spheres. Some attribute solidity to them but others suggest that they are fluid or air-like and penetrable in principle.

In his article on Brudzewo's *Commentariolum*, which is mentioned in the review, Barker [2013] cites the work of Michel-Pierre Lerner [2008]; but because some of Lerner's distinctions have also been overlooked, I cite the most relevant passages here. After defining 'planetary sphere', Lerner says:

> Defined in these terms, the total spheres with their contents appear to have been considered real by a large number of astronomers, including Copernicus and Tycho Brahe (at least up to 1573). [2008, 2:3–4]

While Lerner implies the reality of the partial orbs here as well, he immediately sounds a cautionary note and adds that even those, like Albert of Brudzewo, who denied the reality of the partial orbs, affirmed the reality of the total sphere. In his first brief chapter on Copernicus [2008, 1:131–138], Lerner grants to Swerdlow the plausibility of his reconstruction of Copernicus' view and path to heliocentrism but points out several unresolved problems. For example, the Earth would have to be attached to the sphere; but air, a rare and fluid body by definition, occupies the higher region of Earth, so how could it be attached or fixed to a solid body? In traditional celestial theory, the natures of the spheres and of the celestial bodies are substantially homogeneous with the orbs moved by Intelligences. All such questions and theories do, of course, belong in the realm of natural philosophy. The point is that Copernicus did not answer questions about the nature of the spheres. He evidently made a conscious decision to leave such questions aside. Lerner returns to the problem of Copernicus' reticence and the contradictory and irreconcilable interpretations of Copernicus' spheres among 16th-century and recent interpreters [2008, 2:67–73], and adopts a different strategy in asking 'What, in the face of Copernicus' silence, is logically consistent with his new cosmological configuration?' The reviewers refer selectively to Magini, about whom more below, and Mästlin but ignore Mästlin's doubts about the nature of the orbs and neglect altogether Rothmann's unequivocal denial of the solidity of the orbs.

The reviewers acknowledge Copernicus' 'inability to choose between mathematically equivalent models' and concede that 'there was no obvious way of choosing between' alternative orb-models. They also acknowledge the incompatibility of Peurbach's reframing of the Ptolemaic system and Copernicus' dimensions. In Peurbach's version of the system,

> [E]ach set of partial orbs formed the total orb for a single planet. The total orb for one planet fitted perfectly inside the total orb for the next planet out, with the fixed stars forming a boundary to the whole system. [326–327]

But they overlook the first problem. In both Capellan and Tychonic arrangements, the orbs for Mercury and Venus are enclosed in the Sun's orb. In addition to the gaps between the orbs and the enormous gap between Saturn and the fixed stars, Copernicus' system requires abandoning the principle that each distinct motion requires a separate orb and supposing, in the case of the Moon and Earth, that the two share a total orb. In other words, for Copernicus, the Earth's total sphere carries the Moon's orbs with it around the Sun.

Lerner also points to several consequences of Copernicus' cosmological vision that are clearly inconsistent with the traditional theory of spheres and orbs. I take Lerner's point to be that Copernicus set in motion a process that contributed to questions about spheres and orbs, and I concluded [2010, 370–380, 384–386] that his retention of the total spheres to which planets are attached provides the most conspicuous evidence of his adherence to the Aristotelian tradition while otherwise remaining silent about the nature and movers of the spheres.

The reviewers criticized my claim that Sacrobosco's *Sphere* is 'of almost no practical use' and ask 'no use to whom? Practical for what?' [321]. The reviewers answer their own objection: 'It does not really teach astronomical calculations'. In other words, it is of no practical use to astronomers for calculating past or future positions. As for the *Theoricae novae* and their criticism of my claim, 'The traditional accounts of orbs never make it clear how the orbs are consistent with the mathematical models' [2010, 378], the reviewers' own illustration from Magini [322, Figure 1] highlights the problem.

This figure is clearly not constructed to scale and requires the interpretation of circles that are not spheres. Those problems to the contrary notwithstanding, I was mistaken in claiming that the mathematical models entail the penetration of orbs. A combination of orbs with the mathematical models is possible without penetration. Magini's 'Scheme of orbs, and of the centers of the sphere of the Moon' illustrates such a possibility. The assumption that Magini's scheme is compatible with the Copernican theory, however, involves a sleight of hand.

To reconcile Magini's scheme with Copernicus' lunar model [*De rev.* 4.3], we have to assume that the entire orb centered on epicycle orb E rotates counterclockwise on circle D. Epicycle orb F, however, rotates clockwise. Now, in Copernicus' scheme the epicyclet (the upper epicycle) rotates clockwise because the center of the epicyclet is on the circumference of the epicycle whose radius vector rotates clockwise. In other words, the epicyclet rotates clockwise because the radius of the lower epicycle is rotating clockwise as the deferent radius rotates counterclockwise.

So, what in Magini's scheme causes epicycle orb F to rotate clockwise around epicycle orb E? Suppose that the circumference of epicycle orb E rotates clockwise. How, if at all, can it cause epicycle orb F to rotate unless they are connected somehow? Here is the sleight of hand to which I referred earlier. Copernicus' epicyclet radius rotates counterclockwise, entailing that the radius of Magini's epicycle orb F carrying the Moon is also rotating in the same direction contrary to the clockwise rotation of its center.

There cannot be a real physical connection between the geometry and the orbs, so what causes the orbs to move in the way that they do? A mathematical description provides an account, not an explanation. An explanation requires some real connection between the orbs or some altogether extraneous explanation. Geocentrists could appeal to celestial Intelligences or angels as the movers. To what can a heliocentrist appeal? The supposed compatibility between Magini's scheme and Copernicus' mathematical models is illusory and, accordingly, the assumption of real partial orbs is unwarranted.

There are, to be sure, similar problems with Copernicus' assumption of total spheres moving the visible bodies; but we have noted his explicit testimony about their existence and that he was silent about the partial orbs. He was content to describe the models and the circles needed to calculate mean longitude, the angle of anomaly (epicycle), and the additional angle of anomaly (epicyclet), from which the astronomer can calculate the Moon's true longitude and account for the appearance of the Moon at quadrature better than Ptolemy could.

Magini's orb version raises precisely the sorts of problems that Copernicus could have seen in Giorgio Valla's translation of Proclus' *Hypotyposis* [1501, XVIII, fol. sig. gg7]. Proclus objected that the astronomers make the eccentric and epicycle circles as well as their spheres move independently and that even the circles do not move like one another but in opposite directions.

Copernicus says that rotation is natural to spheres as following their form. But why do some orbs rotate clockwise and others counterclockwise? Must we also conclude that Copernicus retained Intelligences as the movers of the spheres and orbs or perhaps that he believed that the cosmos possessed a world-soul that directs the motions of the heavenly bodies? Where does speculation end?

Of course, Copernicus says nothing about any of this except to adopt the total spheres as the movers of the planets. The entire discussion hinges on Copernicus' view of partial orbs about which he said nothing. As for the claimed connection between partial orbs and Swerdlow's reconstruction of Copernicus' path to heliocentrism, except for the question of impenetrability, there is none. I acknowledge Copernicus' adoption of the total sphere and grant that this alone suffices for Swerdlow's reconstruction. I will return to such speculation, however, subsequently.

The reviewers' argument rests in part on the assumption that Copernicus would not have been exceptional in his view of partial orbs [325–327]. Why not? What could be more exceptional than his rejection of geocentrism and adoption of Earth's motions?

Exceptionalism on some questions is characteristic of Copernicus and is neither surprising nor striking. His silence here seems consistent with his silence on other matters about which there was controversy. These were the sorts of problems that Copernicus left his followers and successors to resolve. The reviewers select only evidence of followers who adopted partial impenetrable orbs and proposed alternative theories about the fluid or air-like qualities of spheres that could penetrate, and ignore those followers who either rejected partial impenetrable orbs or were also silent about them. Why give preference to those who adopted impenetrable partial orbs over those who did not?

This is not to say that Copernicus rejected the real existence of partial orbs—he does not say that either. Here are the most general alternatives:

(1) He adopted partial orbs as real without explaining their nature or the causes of their motions. This would be consistent with his adoption of real total spheres.
(2) He rejected the reality of partial orbs because exceptions to the principles were required to accommodate a body moving around

another body that was itself moving and resulted in gaps contrary to the traditional theory. This would be consistent with his adoption of real total spheres as necessary and sufficient to account for the circular motions of bodies attached to or fixed in them.

(3) He said nothing about the reality of partial orbs precisely because there were disagreements about their reality and their relation to the geometrical models. Again, this would be consistent with Copernicus' assumption and belief that he needed real total spheres to account for the regular, circular motions of bodies attached to or fixed in them.

The first two are possible but it is the third that fits best with what Copernicus has written.

C. John of Glogovia and Albert of Brudzewo

Barker [2013] makes some questionable assumptions about John of Glogovia, Albert of Brudzewo, and the curriculum at Cracow.

Scholastic commentators distinguished between introductory summaries representing what the commentators believed the author to have meant and more advanced commentaries. The introductory summaries *might* represent the commentator's own views but we need to compare them with their more advanced treatises to confirm such an interpretation. John of Glogovia's more advanced interpretations of some questions on logical issues, for example, contradict his own commentaries on introductory texts [see Goddu 1995, 152–163]. Glogovia's advanced questions on natural philosophy often present a variety of solutions to problems, leaving readers to sort out the issues for themselves. We can usually determine Glogovia's genuine opinion but doing so requires painstaking comparison of a variety of sources. The editor of Glogovia's *Questions on the Physics*, Marian Zwiercan [1973, 98–108], concluded that Glogovia's philosophical works 'contain ideas drawn from Averroes and Averroists'. This does not mean that Glogovia was an Averroist but it does caution us to be careful about issuing definitive declarations about his doctrines. Glogovia relied on a large number of authors, among them Albert the Great, Thomas Aquinas, John Versoris, and the 13th-century Latin Averroist John of Jandun. In fact, Glogovia relied heavily on John of Jandun for his commentary on Aristotle's *De anima* [see Kuksewicz 1962]. Glogovia did lecture on Gerard of Cremona's *Theorica planetarum* but Polish experts cite only a fragment of his comments in manuscript, now lost [see Seńko 1964, 36; Birkenmajer 1900b, xxv]. In his treatise on the *Sphere*, it appears that Glogovia did accept the reality of partial orbs [see Barker 2013, 127–130] but we do not possess a more advanced text by him on the subject. In his *Quaestiones de motu*, however, he 'pondered' the Averroist rejection of epicycles and eccentrics without stating a conclusion [see Zwiercan 1973, 107–108; Markowski 1975c, 110]. Finally, a point to which I will return below, it was not only Averroists who expressed doubts about the reality of eccentrics and epicycles.

We know very little about Glogovia's relationship with Albert of Brudzewo. Glogovia taught Brudzewo [see Zwiercan 1973, 108]. They were both associated with the same student hostel. Historians of the University of Cracow and of medieval Polish philosophy have portrayed John as a typical author of the scholastic commentary tradition. According to some sources, he was a critic of such modernizing humanists as Conrad Celtes [see Morawski 1900, 2:155–158], although some contemporaneous humanists praised him [see Zwiercan 1963, 452]. Brudzewo taught Celtes and even referred to him in one letter as 'son' [see Morawski 1900, 2:177]. Whether Glogovia's criticism of Celtes, however, put any strain on his relationship with Brudzewo is unknown. Brudzewo has been linked with a humanist circle in Cracow, the *Sodalitas Litteraria Vistulana*, which supported both Renaissance Humanism and Neoplatonic philosophy. Brudzewo's adoption of Peurbach's *Theoricae* suggests that he belonged, unlike Glogovia, to the humanist circle of astronomers. The point is that Glogovia's and Brudzewo's views may have been compatible on some issues and not on others.

Indeed, aside from more advanced treatises sometimes contradicting the same author's introductory comments, Barker's assumption [2013, 129, 135] that university masters would not have disagreed with one another in introductory undergraduate courses overlooks the dialectical and disputatious nature of medieval pedagogy. Students expected their teachers to disagree and criticize one another as part of the dialectical nature of the enterprise. In general, there is a better fit between the approach adopted by Brudzewo and his predecessors in the astronomical school at Cracow than with Glogovia [see Rosińska 1973a, 1973b; Markowski 1975a, 1975b; Dobrzycki 1975].

The reviewers [320] express surprise, indeed, they even call it 'bizarre', that I would doubt Copernicus' direct knowledge of Albert of Brudzewo's *Commen-*

tariolum, as if this were not a question of fact. But we have to distinguish between Brudzewo's lectures, the manuscript copies of his book, and the published version. I agree that Copernicus must have heard lectures on the *Commentariolum* and we may presume that he took notes. But to answer questions about the manuscript copies requires our knowing more about the number of manuscripts and how they were distributed. The important point is that Copernicus knew its content. The echoes alleged by Ludwik Birkenmajer [1924, 83–98] between the *Commentariolum* and Copernicus' *De rev.* are faint and unpersuasive. The *Commentariolum* is an exceedingly rare book. There is no evidence that Copernicus owned a copy or of its having been in a Varmian library in the 16th century [see Hipler 1874], meaning that after 1502 it would have been very difficult for Copernicus to consult it. These are questions of provenance. Direct textual evidence is not necessary for acquaintance and influence unless we are looking for proof of a unique source for a specific fact (such as Birkenmajer's 'echoes') or assertion in Copernicus' texts.

Barker's account [2013, 130–139] of Brudzewo's *Commentariolum* requires four emendations concerning:

(1) Brudzewo's definition of 'sphere' or 'orb',
(2) Barker's description of the marginal annotations regarding the lunar model,
(3) his neglect of Brudzewo's description of a reciprocation or libration mechanism, and finally, the most serious,
(4) Brudzewo's comments about the solar orbs and his reflections on the reality of eccentric and epicycle orbs.

(1) Under the third way of understanding 'orb', namely, as the orb concentric to the Earth or the aggregate of all orbs necessary and sufficient to save the motions of a planet in longitude and latitude, Barker [2013, 130] omits Brudzewo's comment that the third, that is, the aggregate both with respect to its convex and concave surfaces, is the sense appropriate here, which I take to mean the 'principal subject' of the treatise. In other words, 'sphere' or 'orb' refers primarily to the total or complete sphere.

(2) Birkenmajer included some marginal annotations from manuscript copies of Brudzewo's text. In his edition of the lunar model, Birkenmajer cited figures from two manuscript versions, L and C, and the edition of 1495 E [see 1900b, 68–69]. John of Crobya copied the part of manuscript

L containing a figure with two concentric orbs and a marginal annotation in 1488 [Birkenmajer 1900b, xlvi–xlix]. Manuscript C is dated to 1493 and contains a figure perhaps copied from a lecture of Albert of Pniewy [see Birkenmajer 1900b, xlix–li]. It is possible that Copernicus attended Albert of Pniewy's lectures. The edition of 1495 also includes the figure with two concentric orbs. In combining these sources, Birkenmajer believed them to be representative of the comments made by lecturers and he emphasizes the point that they reflect the views of Brudzewo himself. In other words, these are early witnesses to Brudzewo's own interpretation and annotations. As Barker [2013, 138] explains, the motions described refer to πρόcνευcιc, what Toomer [1998, 226–227] translates as the 'direction in which the epicycle points' or what Pedersen [1974, 192] calls 'inclination'. The figure from manuscript L depicts the Moon in an epicyclic orb inside a second epicyclic orb. The outer epicycle accounts for the change of direction and the additional figures depict the Moon in all of its phases. Barker objects to Birkenmajer's claim that the motion is related to the explanation of the spots on the Moon. In fact, however, that is exactly what the annotation in manuscript L, as quoted by Barker, says.

The issue, then, is not whether Brudzewo and his students understood the relation between πρόcνευcιc, lunar phases, and the spots on the Moon correctly but rather that they believed that there was such a connection. What could be the source for their interpretation and, above all, the suggestion that a double-epicycle model could account for the phenomenon?

The likely source, as proposed by Rosińska [1974], is Sandivogius of Czechel. Indeed, the description provided by Sandivogius as quoted by Rosińska [1974, 241–242nn11–13.] is almost identical to the marginal annotation in manuscript L of Brudzewo's commentary. While it is true that Rosińska questioned the Islamic route of transmission, she also acknowledged the greater similarity between the role of the models in Islamic sources and Copernicus than between Sandivogius and Copernicus. In fairness to Rosińska, her doubts about Sandivogius' originality [1974, 243] should also be emphasized.

There is, however, a misunderstanding here. It is clear from the figures described that the solutions are represented as epicyclic orbs, as is appropriate for the *Theoricae*. It is also clear that the orbs are depicted concentrically. The differences from the Copernican model are clear. I did use the word 'device' [2010, 156–157] and so contributed to the misunderstanding; yet my claim is not, and never was, that Copernicus derived his version directly from the Cracow models but rather that the idea of a double epicycle may have impressed him. He could perhaps have developed his model independently but a Cracow source does not exclude the Islamic route. That is to say, the Cracow models may explain his receptivity to other solutions.

(3) Brudzewo's description of a reciprocation mechanism requires some comment. Without a great deal of explanation, as if it were well known, Brudzewo [Birkenmajer 1900b, 120] describes the composition of a rectilinear motion by means of several circular motions. Birkenmajer does not indicate any marginal figure but it would be prudent to consult the manuscripts. In any case, Birkenmajer does not hesitate to compare it with the so-called motion of libration in *De rev.* 3.4. Birkenmajer added:

> We do not know whether it was his own or another's creation, but even more striking is the ingenious method of Brudzewo for the kinematic elaboration of a rectilinear motion from several circular motions. [1924, 95]

In fact, I did not exclude the possibility that 15th-century Latin authors may have relied on a description of Maragha planetary theory [2010, 478]. My complaint here is not about the questions that I posed and the alternative sketch that I provided but that I am accused of an absolute rejection of the Islamic route.

(4) Finally, a similar selectivity characterizes Barker's account [2013, 132] of the solar orbs. Brudzewo [Birkenmajer 1900b, 19] asserts that the ancients understood the Sun to move not on a circle but in an orb, which is a solid and spherical body. Over the next few pages, Brudzewo continues his recitation of the contents of the *Theoricae* with comments about the relation between geometrical models and spherical orbs, mentioning objections, some deriving from Averroes about the penetration of spheres or the introduction of a void [Birkenmajer 1900b, 25]. The resolution of these difficulties, he says [1900b, 25–26], was achieved by dividing each total concentric sphere into partial orbs to account for the observations and the diverse motions of the planets. Barker [2013, 134–135] claims that Brudzewo pits the philosophers against the astronomers here. Yet it is also astronomers who divided the total orb into partial orbs, the purpose of which was to account for the observed positions and motions. Immediately after that explanation comes the comment that nearly everyone had previously interpreted as representing Brudzewo's genuine view [1900b, 26–27]. In Rosen's translation:

> No mortal man knows whether these eccentrics really exist in the spheres of the planets, unless we admit (as some people claim) that the eccentrics, like the epicycles, are made manifest by the revelation of spirits. If we reject this claim, then the eccentrics are devised solely by the imagination of the astronomer. [1939, 123]

Brudzewo follows with a quotation from Richard of Wallingford, who denies the reality of eccentrics and epicycles as fictions, products of the mathematical imagination. Indeed, Richard, as quoted by Albert, says that no one trained in this discipline could truly believe that eccentrics and epicycles exist as imagined. According to Barker, Brudzewo was quoting the views of those he rejected. Brudzewo completes his comments, omitted by Barker, however, with the following assertion:

> So says the author. We should therefore be content with these means, for through them we achieve a perfect science of the moving stars.

Is this the voice of someone who rejected the agnostic judgment expressed in the paragraph cited above? Barker [2013, 135] suggests somewhat fancifully that the reference to 'revelations of spirits' refers to a 'spirit located in the celestial regions'. It is likelier that Brudzewo was questioning the certainty expressed by realists. How do they know whether eccentrics and epicycles really exist?

Barker has quite rightly pointed out problems with my interpretation of Brudzewo's text. There are passages where Brudzewo asserts the real existence of partial orbs; but it was a standard technique in a commentary to cite the views of the author, Peurbach in this case, especially in the comments following *lemmata* or quotations of the first words of a section or paragraph. Some previous interpreters have concluded that Brudzewo was in fact a fictionalist about eccentrics and epicycles. Even I used that word [2010, 148, 158] and stated elsewhere [376] that he rejected the reality of epicycle spheres. But in my brief description of his text [2010, 164], I described him more cautiously as 'agnostic' about the existence of eccentrics. Since then, I have become more convinced that the correct word to describe Brudzewo's view is 'agnostic'. In other words, Brudzewo did not affirm their real existence but he did not categorically deny their existence either.

Such cautious reading prompts me to add that we should reconsider how the motions of the planets can be described as real. Clearly, the paths or orbits are not real entities distinguishable from the moving planets. On the other hand, the planets do not move just anywhere. What a partial orb really does describe is the area in which the visible body moves. In responding to Averroist objections, Brudzewo affirmed the necessity of the mathematical models. But their necessity does not entail the real physical existence of partial orbs.

In my view, the main flaw in Barker's analysis [2013] is the lumping together of opponents of eccentric and epicycle orbs as all Averroists. Barker ignores ancient and medieval opponents who did not follow Averroes but who raised doubts about the reality of eccentrics and epicycles. Proclus, Richard of Wallingford, John Buridan, and Henry of Hesse were at best agnostic about the existence of eccentrics and epicycles. The assumption that only Averroists adopted such a view is mistaken. Non-Averroist followers of Aristotle also raised objections based on Aristotle's conception of spherical motion around bodies. In the section quoted [Birkenmajer 1900b, 122–123], Brudzewo refers to those who support partial orbs against the Averroists and then adds the comment about their existence. He volunteers the objection, suggesting that he had doubts, but which he expresses cautiously and without commitment.

Finally, as confirmation of that reading, I quote Birkenmajer's own judgment after noting the similarity in Brudzewo's and Copernicus' scepticism about the equant:

> The same skepticism in Brudzewo and Copernicus with respect to the real existence of other circles was, however, rather moderate. We can see this in parallel passages here and there, which also testifies to the similarity in the thinking of both scholars. [1924, 91–92]

Birkenmajer follows that comment with quotations from both Brudzewo and Copernicus on eccentrics and epicycles. He calls their view of other circles 'skepticism' but moderate in comparison with their scepticism about the equant.

We may object that editors are not infallible interpreters of the text that they edit. But, if Birkenmajer had reason to believe that Brudzewo was expressing a view that he rejected, then surely Birkenmajer would have indicated a problem. What Birkenmajer does express is the contrast between the definite rejection of the equant as fictitious and Brudzewo's more cautious comments about eccentrics and epicycles. This is why, in my view, we are justified in concluding that Brudzewo was agnostic about their existence.

Such agnosticism may have influenced Copernicus, who also knew of Bessarion's doubts about the reality of mathematical models in astronomy [Goddu 2010, 223–224].

As for my neglect of contextual issues, I claim that the reviewers have not adequately reported evidence that contradicts their interpretation. They have neglected alternative interpretations of spheres and orbs and, with respect to the literature on *De sphera* and *Theorica planetarum*, they have neglected the survey by Christe McMenomy [1984], which is the most thorough study of the different ways in which these texts were interpreted and of the trends that emerged in the 15th and 16th centuries.

D. Capellan and Tychonic alternatives and the origin of Copernicus' heliocentrism

In reference to the explication of Swerdlow's hypothesis [1973] and the issue of orbs and spheres, my reasoning, as it stands, was incomplete and incorrect. I contributed to confusion here by not distinguishing clearly between mathematical models and orb or sphere models. The issue of the reality of partial orbs, however, is irrelevant. The real total sphere is sufficient to create the sort of problem proposed by Swerdlow. If, in either the Capellan or the Tychonic arrangement, the solar orb includes the epicycle orbs of Mercury and Venus, as in Theon of Smyrna's interpretation [Dreyer 1906, 127], then the physical principles for their motions differ from those for the superior planets each of which has its own set of partial orbs inside its own proper total sphere, which is, therefore, different from the total sphere carrying the Sun, Mercury, and Venus.

Already in the *Commentariolus*, after pointing out that there is no one center of all the celestial orbs or spheres [Dobrzycki 2007a, 10 *prima petitio*], Copernicus committed himself to the idea that all the spheres encircle the Sun [*tertia petitio*]. Notice the problem that arises for all orb arrangements as soon as we begin to consider the circular motion of one body around another. In the *Theoricae*, this problem does not arise because all bodies move on epicycles around empty geometrical points. Each body can be treated separately with its own set of partial orbs. In the Ptolemaic system, the celestial spheres are nested and contiguous. Because they have bodies orbiting other bodies, the Capellan and Copernican systems introduce complications with the nesting and contiguity of the total spheres, while the Tychonic system eliminates the celestial spheres altogether. We must revise that feature of the *Theoricae*, however, if some bodies move around another body that is itself moving. This is the problem that arises in the Capellan arrangement, the Tychonic arrangement, and with Copernicus' Earth in motion. Now, suddenly, we must suppose that the Sun does not have a proper total sphere but rather shares it with Mercury and Venus. It is possible that reasoning of this kind influenced Copernicus in the following way. Because he realized that Mars at opposition is closer to Earth than the Sun is, he could not, according to Rheticus [Hugonnard-Roche and Verdet 1982, 55], include the total sphere of Mars inside the sphere of the Sun, which, in turn, necessitated placing Earth's total sphere between the spheres of Venus and Mars and arranged around the Sun.

As we all know, Copernicus says nothing about this problem. As Swerdlow [1973, 478] acknowledged, the elimination of a Tychonic alternative is pure speculation. To my knowledge, Schiaparelli was the first to speculate about a Tychonic intermediary to explain Aristarchus' heliocentric hypothesis [Dreyer 1906, 143–148]. There is no mention of a Tychonic alternative by the ancients but Dreyer adds:

> [W]e can only conclude, that it was never proposed as a way of 'saving the phenomena', though Aristarchus may have first been led to it, and then immediately afterwards may have been struck by the still greater simplicity and beauty of the heliocentric system, which alone he therefore considered worth proposing publicly. [1906, 147]

Dreyer [1906, 364] also suggests that Copernicus may have considered the Tychonic alternative; but if so, 'he did not rest content with it, but proceeded at once to its logical sequel, the heliocentric system'.

Further, we know that Copernicus rejected the Capellan arrangement silently. On the assumption that Copernicus adopted Theon of Smyrna's description, the Capellan arrangement did not involve any penetration of spheres. So, why did he reject it? One of his criticisms of Ptolemy was that his system was like a mosaic, the result of which is the depiction of a monster. The Capellan arrangement is less of a mosaic but, because it proposes two centers and two principles of arrangement, it may have appeared to Copernicus to be like a two-headed monster. Copernicus rejected it because he had already come to the conclusion that the universe should have one center and one principle of arrangement of spheres around it. Dreyer does not clarify what he means by 'greater simplicity' and 'logical sequel' but I suggest that the contrast between the first and third *petitiones* contains the answer. If Copernicus ever considered or anticipated a Tychonic arrangement, he would have rejected it for the same reason.

The supposed unambiguous reference in the 'Letter Against Werner' [Dobrzycki 2007b, 32; Clutton-Brock 2005, 211] to the existence of eccentrics and epicycles uses the word 'circles' (*circuli*).

> What might have been is an abstraction
> Remaining a perpetual possibility
> Only in a world of speculation. [Eliot 1943, 3]

All of this speculation, though not logically impossible, is superfluous. Copernicus does not answer our questions as explicitly as we would like but he does express his reasons sufficiently enough for us to reconstruct his path to the heliocentric theory on the basis of his own words. He enumerates the problems with geocentrism in both the *Commentariolus* and *De revolutionibus*. He adds three further explicit comments in the *De revolutionibus* that are not in the *Commentariolus*, although they are not inconsistent with anything in the *Commentariolus* and, I claim, are already implicit in his early remarks.

The *Commentariolus* and *De revolutionibus* agree on a number of problems with geocentrism. Geocentrists disagree about how to preserve the uniform motions of the celestial spheres and to account for the planets' apparent motions. Homocentric hypotheses cannot account for the observed motions without eccentrics and/or epicycles. Ptolemy and his followers, however, could not preserve uniform motion by means of eccentrics and epicycles without adding equant circles that violate the planets' uniform motions around the center of the deferent sphere or the centers of their epicycles. These difficulties led him, Copernicus says, to search for a more reasonable arrangement of circles (*modum circulorum*) that would preserve uniform motion and account for the observations.

The preface of the *De revolutionibus* adds several other criticisms of which three are most notable:

(1) geocentrists could not derive the principal consideration, the structure of the universe and the harmonious arrangement of its parts, resulting in a mosaic that resembles a monster;

(2) there is a flaw in their method; and
(3) Copernicus' frustration that the motions of the world machine, created for our sake by the best and most systematic Artisan, were not understood with greater certainty.

The disagreement over structure relates principally to lack of consensus among geocentrists about the ordering of Mercury and Venus [*De rev.* 1.10] and to Copernicus' distance-period principle [Goldstein 2002, 220–222], already implicit in the *Uppsala Notebook* and *Commentariolus* [Goddu 2006, 39–46]. In other words, nothing in geocentrism compelled a choice among the alternatives.

The failure to reach agreement on the planetary order was the result of a flaw in their method. Copernicus admits obscurity here, saying that it will become clear in the proper place. Where else does he discuss these issues other than in chapters 4–10 of *De rev.* 1, which he summarized very briefly in the *Commentariolus* [Dobrzycki 2007a, 11.19–21]? In fact, he presents the solution in the *Commentariolus* along with the observation that the arguments in support of Earth's immobility rest on appearances.

What, then, was the flaw? Following Aristotle, geocentric astronomers had inferred the structure of the whole from the observation of one part. We see heavy bodies fall in straight lines towards the center of Earth. The motion of the whole must be the same as the motion of the part; therefore, Earth, if it moved, could move only in a straight line toward the center. We see celestial bodies, however, move in circles; hence, they do not possess the same tendency as heavy bodies. From those premises, geocentrists concluded that Earth must be immovable at the center of the universe. The flaw is the logical fallacy of arguing from part to whole, the result of which is that geocentrists cannot agree on a unique principle for ordering the spheres. That result frustrated Copernicus. Had the most perfect Artisan constructed the universe arbitrarily, not in the best way possible and with no principle of arrangement as suited to a harmonious structure, so unknowable by us? Impossible, thought Copernicus. As he considered alternatives, he realized or hypothesized that by having all planetary spheres arranged around a stationary Sun, a unique principle for their harmonious arrangement emerged. In other words, we must begin with the whole organized in a harmonious fashion to infer the ordering of the parts and come, then, to a decision about the places and motions of the parts [Goddu 2009].

I return, as promised, to Copernicus' reliance on the Platonic tradition. Where did Copernicus get the idea that the universe must be organized harmoniously according to a unique mathematical principle? These are ideas that we associate with Platonism, Neoplatonism, Neopythagoreanism, and even with scholastic or Christian Neoplatonism. The authors and predecessors that he cites provide us some clues. We need to consider a variety of possibilities here, including his reading of Cardinal Bessarion's *In calumniatorem Platonis* [1503] and of Ficino's translation of, and commentary on, Plato's dialogues [1484], his references to authors who are associated with Pythagorean views about harmony and mathematics, and, lest we forget, Ptolemy himself. After all, Copernicus suggests that he adopted Ptolemy's program and his promise of progress, and eventually concluded that because of the flaw in their method summarized above, Ptolemaic astronomers had failed to achieve the hoped for results.

E. Concluding reflections on the summary of my study, textual evidence, and speculation guided by contextual considerations.

If I have expressed frustration with the oversight concerning the principal results of my study [2010, 285–291, 358–360, 384–386, 425–427], I have to conclude that, because I buried them in such a long book and did not summarize them clearly enough at the end, the reviewers overlooked my claims. It is my good fortune that they have given me the opportunity to summarize what I thought I had made clear.

The reviewers were right to complain about lapses of clarity, reluctance to speculate without textual foundations, and the incompleteness of some arguments. In my defense, I refer above to the passages that address the first, remind readers here of my speculative reconstruction of Copernicus' education at Cracow [2010, 5–167], and call upon my references to supporting evidence in my other publications. The questions and criticisms that they raise have allowed me to respond directly to problems that I perhaps should have anticipated but which I thought I had addressed. In retrospect, it is clear that I did not do so adequately.

My chief complaint about the speculation concerning partial orbs is that it adds nothing to our understanding of why or how Copernicus proposed a heliocentric system. If Swerdlow is right, it suffices to affirm the existence of the total spheres and their impenetrability, regardless of whether partial orbs exist or not. In my view, Swerdlow's speculation, unsupported by any text in Copernicus, is superfluous. We can reconstruct his path to heliocentrism by relying on his words, his criticisms of geocentrism, and his arguments in support of Earth's motions.

To focus this conclusion and my response to the reviewers' main criticism, I must ask the reader to decide whether we should base a reconstruction of Copernicus' achievement on what he wrote or on what we suppose he may have thought about matters on which he chose to remain silent. It seems to me that we should resort to the second only when we have exhausted his words and a close study of the sources that we know he used. We are not finished studying his sources. For example, no one to my knowledge has examined carefully Copernicus' copy of Bessarion, *In calumniatorem Platonis* [1503]. My brief summary of that text [2010, 220–225] had as its primary goal to confirm that Bessarion's defense of Plato did not entail a rejection of Aristotle. I did, however, suggest even then that Bessarion influenced Copernicus to be cautious about realist interpretations of astronomical models. As models for this sort of reading of Copernicus' books and his annotations, I commend the studies of Dilwyn Knox [2005, 2012], who has taken the considerable trouble to identify carefully Copernicus' doctrines and then to search the sources which we know he used for the best fit.

When we do resort to contextual considerations, we should be comprehensive in laying out the alternatives, and, if we choose one over another, in explaining why we have eliminated the others. Even in cases where we may disagree with the reconstruction, the consolation will be in knowing that the evidence has been presented fully. In the end, I do not think that we can eliminate subjectivity from the conclusions that we prefer. Although I criticize the reviewers for their selectivity, their criticisms are clearly motivated by what they perceive to be a far too narrow reliance on texts. I have explained my shortcomings here as due to the conviction that Copernicus made the conscious decision not to express himself on some topics that were controversial or unsettled among the experts. We can sometimes reconstruct his genuine view from other assertions that he makes but, in some crucial cases, we must consider the possibility that he did not know the answer and adopted the sort of agnosticism that I claim he learned from Albert of Brudzewo (or one of his students) and perhaps Bessarion.

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