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The authors

The (likely) Last Edition of Copernicus' *Libri revolutionum*

André Goddu

Review essay of Nicolas Copernic, *De revolutionibus orbium coelestium, Des révolutions des orbés célestes*. 3 volumes. Science et Humanisme, Collection published under the patronage of the Association Guillaume Budé (Paris: Les Belles Lettres, 2015).

- Vol. I:** Introduction by Michel-Pierre Lerner and Alain-Philippe Segonds with the collaboration of Concetta Luna, Isabelle Pantin, and Denis Savoie, xxviii + 859 pp.
- Vol. II:** Critical Edition and translation by Lerner, Segonds, and Jean-Pierre Verdet with the collaboration of Concetta Luna, viii + 537 pp. with French and Latin on facing pages and with the same page numbers.
- Vol. III:** Notes, appendices, iconographic dossier, and general index by Lerner, Segonds, and Verdet with the collaboration of Luna, Savoie, and Michel Toulmonde, xviii + 783 pp. and 34 plates.

THIS EDITION OF COPERNICUS'S MAJOR WORK, probably the last for the foreseeable future, represents the culmination of efforts that can be traced back to 1973. The two fundamental sources of *De revolutionibus* are Copernicus's autograph copy which survived by sheer luck and the first edition published in Nuremberg in 1543. Since the publication of the first edition in 1543 there have been several editions, notably a Polish critical edition and a German critical edition which appeared respectively in 1975 and in 1984. The Polish edition is based on Copernicus's autograph, and the German edition is based primarily on the 1543 edition. The Polish editors produced a mixed text, one that is essentially the autograph integrated with the 1543 edition, the aim of which was to produce a perfectly completed work, a goal that Copernicus and his disciple Georg Joachim Rheticus never achieved. The French editors of the volume under review, like their German colleagues, decided to adopt the 1543 edition as the basis for their critical edition, using the autograph to correct the published version only where the 1543 edition was faulty because of evident reading errors or typographical misprints. The critical edition (Volume II) is

followed (also in Volume II) by an edition of the most important passages from the autograph that were not retained in the 1543 edition. While many of the differences between the French edition and the German edition are minor, the principal difference is that the title and prefatory materials in the German edition were contaminated by elements drawn from the autograph, and that the German edition does not include a German translation. The French editors are consistent in their application of the principles and the use of the 1543 edition as the base text and more complete in the production of a translation. After so many editions, one hesitates to pronounce this one “definitive”, but it is difficult to imagine any more major revisions.

Volume I, the Introduction, is divided into six chapters: Copernicus’s biography, his minor astronomical works and the preparation of *De revolutionibus*, a summary of the content of his major work, the reception of heliocentrism between 1540 and 1616, Copernicus’s precursors, and a history of the text. The volume opens with a table of abbreviations of editions and works of Copernicus, of other texts, and of works cited often in short form. It should be noted here that there is no comprehensive bibliography, meaning that the works of many authors cited in the footnotes are not included in the list of works cited in short form; readers will find the names of authors in the indices, but they will have to consult each entry for all of the works of authors cited. The six chapters of the Introduction are followed by 17 *excursuses* (Annexes) on topics that could not have received the attention that they deserve if they were treated in the main text of the Introduction. The last excursus deals with Copernicus’s library. Volume I concludes with indices of ancient and modern names cited in the introduction and a geographical index.

Volume II contains the critical edition and French translation, followed by an index of personal and geographical names cited by Copernicus, and by an index of words that furnishes an inventory of the astronomical, mathematical, and physical lexicon of *De revolutionibus*. Volume III contains an analytical summary of *De revolutionibus*, notes on each of its six books, a series of 17 complementary notes in which the editors have grappled with more general problems and subjects concerning different sections of the work, and notes on the passages in the autograph that were suppressed. The collection of notes constitutes a textual, historical, and technical commentary, followed by an appendix on the condemnation of heliocentrism and the censorship of *De revolutionibus*, a separate section on Spanish reactions to the Roman Index and the censorship of Copernicus’s work, an iconographical Dossier (with plates collected at the end of the volume), and it is completed with an index of ancient and modern names, a geographical index, and an index of notable subjects that bear on the astronomical, mathematical, and physical topics treated in Volume I and the notes of Volume III, and is complementary to the index of words from Volume II.

As I begin with my evaluation of the edition, readers should be aware that the editors have referred to several of my publications, some approvingly and some

not, but on the whole constructively, and so it would be disingenuous of me not to acknowledge my favorable reception of their principles and their execution of such an intrinsically difficult project. They are conservative in their assessment of evidence, and on the whole I agree with their reliance on confirmable documentary evidence. That said, there are almost inevitably differences of opinion, and I shall attempt as objectively as possible to point out what I perceive to be some inconsistencies and problems. I should also add that since I received my copy of the edition, I have had only a month to examine it, so this will very likely not represent my final thoughts on these volumes. It should be noted as well that we do not know what differences of opinion there may be among the editors. This is presumably a work of consensus, and here and there one gets the sense that some carefully expressed opinions may be the result of discussions that did not lead to a complete resolution. Be that as it may, the completion of this project provides an outstanding platform for the continuation of discussion and debates on the Copernican achievement. By way of concluding these introductory remarks, I refer readers to pp. xiv-xv of Volume I for indispensable comments about the contributions of Denis Savoie, Michel Toulmonde, Isabelle Pantin, and Concetta Luna to the completion of the edition. Dedicated to the memory of Paweł Czartoryski and René Taton, the edition acknowledges its debt to predecessors, and it concludes with an appreciation for the contribution of the late Alain Segonds, who sadly did not live to see the completion of the project.

And now to matters of substance. I begin with comments on principles of interpretation. Although they do not cite an explicit formulation of a methodological principle of interpretation that they commend until p. 270, it is clear from their rejection of numerous conjectures and reconstructions and from their adoption of others that they shared a principle which they termed "methodological prudence". In evaluating suggestions about sources that Copernicus supposedly used, this principle advises: "The kind of evidence that we need in such cases includes annotations in the books that we know Copernicus owned or read, expressions in *De revolutionibus* or in *Commentariolus* that parallel *uniquely* arguments or comments in sources that he consulted, or quotations and facts that we can show he borrowed from another author (for example, Copernicus's evident reliance on Regiomontanus's *Epitome*). Other than this quality of evidence, conjecture and speculation based on similarities can often be traced back to another text common to all possible intermediaries, such as one by Aristotle, Ptolemy, *De sphaera*, or some version of *Theorica planetarum*. Such speculation leaves us with no way of knowing whether Copernicus responded to a suggestion in some commentary or reacted directly to the original text" (Goddu 2010, 187). The application of methodological prudence, however, still leaves room for disagreement, as we shall see in a number of cases discussed below.

Reviewers typically comment on topics of particular interest to them, but I have selected ones that remain controversial, and that require further comment.

Those topics include (1) Copernicus's use of the terms *petitiones* and *axioma* to characterize the postulates enumerated early in *Commentariolus*; (2) the evaluation of precursors and especially of Maragha precedents; (3) Robert Westman's thesis about astrology; (4) the chronology of the writing of *De revolutionibus*; (5) the genesis of Copernicus's heliocentrism; and (6) the Galileo affair. I will conclude with observations on the edition, emphasizing the texts that mark a major advance over previous editions.

1. Copernicus's Postulates

According to Noel Swerdlow, "The seven postulates, incorrectly called *axioms*, [. . .] are hardly self-evident" (Swerdlow 1973, 435-438; see also 423-512). In his view, one of the postulates (the first) stands by itself, and four of the propositions (2, 4, 5 and 7) are not postulates at all but rather deductions from postulates 3 and 6. In sum, the postulates are not axioms, and their logical relationship is far from clear. In distinguishing between an Aristotelian conception of axiom (according to which an axiom is a first proposition on which further demonstrations depend) and a Euclidean conception (according to which an axiom is a self-evident proposition accessible to all without instruction), the editors agree in part with Swerdlow on the first objection: Copernicus's axioms are not self-evident (I:233). The editors go further, however, and argue that there is a logical link in the succession of postulates (I:235). In enumerating the propositions, however, the editors do not explain exactly what is "logical" about the succession of the postulates. What is the *logical* connection between the denial of one center of all the celestial spheres (first postulate) and the claim that the Earth is not the center of the universe (second postulate)? And how does the third, which places the Sun near the center of the universe, follow from the first? By what reasoning does one move from the first to the second, and from the second to the third? Enumerating and paraphrasing the postulates does not explain their logical connections. The editors understood the dialectical procedure that I used to explain the connections (I:251-253), but my brief recitation was evidently not clear enough. I proposed that Copernicus literally used a method of attack in the form of questions (*petitiones*) to formulate the postulates and their sequence. In other words, the postulates are both questions and answers. Do the celestial spheres have one center or many? If many (first postulate), then Earth cannot be the center of the universe but only of gravity and of the lunar sphere (second). Why are all of the models for planetary spheres related to the position of the Sun? If their motions are relative to the Sun, then we may assume that the spheres encircle the Sun approximately in the middle of their motions (third), and so on. In other words, the postulates are not logical in a deductive way but rather logical in the manner of a Socratic dialectical inquiry.¹

¹ The editors cite James Evans to support their claim about the logical connections between the postulates, but Evans rather explains the content of each without addressing

The above comments apply the principle of methodological prudence advocated by the editors. Relying on a genuine Copernican annotation, I suggested how it could have motivated Copernicus to formulate questions and lead him from one postulate to the next. To sum up, we are in agreement to the following extent—the postulates are not axiomatic in the Euclidean sense, there is not a logically deductive relationship between the postulates, and the results described later in the text derive from the postulate of a moving Earth.

2. Precursors and Maragha Precedents

The editors' evaluation of the notion of "precursor" altogether (I:520-551) marks an important methodological contribution to discussion of the relation between Copernicus and his predecessors from epistemological and logical perspectives. The editors point out, however, that Copernicus himself mentioned authors who had put him on the path to heliocentrism. Indeed, Copernicus's reliance on his predecessors is evident from the sources that he owned and some (now lost) that he consulted. His dependence on authorities is undeniable. That said, we also have to take into account the extent to which his sources provided him with cover for his almost universally rejected proposals about Earth's motions. He was not a lone hero or completely original in his astronomical system, yet he was the first to propose a complete and detailed cosmological system based on Earth's motions and the Sun's stability. However much other authors may have been suggestive of a heliocentric system with Earth in motion, none of them proposed the system that he advanced. For instance, the Pythagoreans as cited by Copernicus proposed that Earth is not the center of the universe and that it moves, but none of them created a cosmological and astronomical system based on those ideas that could account for the observed celestial motions. No predecessor who held some part of an overall theory had put it all together in the way that Copernicus did. Copernicus's claims in this regard succeeded in fooling contemporaries and even some of his followers.

In other words, and to be clear as possible on the subject, yes, there were authors before Copernicus who had proposed the Earth's axial rotation, some who considered the possibility that Earth moves from its position, some who proposed that the planets move around a central fire (not identified with the Sun), but there is no one prior to Copernicus who adopted all of these ideas in the form of a heliocentric (strictly speaking, heliostatic) system with the Earth moving around the Sun with the Earth's axis tilted so as to account for the Sun's apparent motion on the ecliptic and for the change of seasons, and to account for the observed motions of the other planets. Beyond that, no one had proposed that Earth's annual motion could contribute to saving the axiom about the perfectly uniform and circular motions of the celestial spheres.

their relation. Of only the seventh does he say that "[m]uch of the remainder of the *Commentariolus* is devoted to showing how the motion of Earth affects the apparent motion of the other planets" (Evans 1998, 415-416).

That last assertion brings us, then, to the hypothesis about Copernicus's acquaintance with and dependence on Maragha theory, specifically the Tusi-couple and bi-epicyclic geometrical models.

The editors, reacting mostly to strong versions of what amounts to "blueprint copying" by Copernicus of Maragha models, cite a number of skeptics and questioners in order to reject the evidence of Copernicus's dependence on these models. The editors raise a number of issues, and point out a number of weaknesses in the assertions of supporters. Their challenge will anger supporters and perpetuate the impasse that has existed for several decades. In my view, discussion of the hypotheses about Copernicus's acquaintance with Maragha precedents has gotten entirely out of hand. Some proponents have over-reacted to those who have merely raised questions, resorting in the latest versions to, in effect, *ad hominem* arguments and ideological posturing.² On the other hand, some opponents have not considered all of the arguments and evidence in a constructive fashion, adopting an excessively narrow view of transmission, albeit in reaction to versions that amount to blueprint copying. They have also, justly, raised pertinent questions about how Copernicus, incapable on his own of constructing geometrical models and, in effect, mathematically incompetent, could have understood, however incompletely, the complex kinematic models that he supposedly saw and that he then adapted for his own ends. From such considerations, the editors conclude, in effect, that none of the evidence provided to date prevents us from thinking that Copernicus produced his models independently (I:554-555). I agree with that judgment, but it is also not entirely

² Barker and Heidarzadeh accuse opponents, some of whom have searched arduously for Copernicus's sources and have emphasized his debt to Arabic sources and critiques of Ptolemy that we know were available to him, of clinging to an out-dated 'lone-genius' view, of ignoring Copernicus's historical context, and of valorizing European exceptionalism (Barker and Heidarzadeh 2016, 19-57, esp. 21, 42, and 57). For his part, F. Jamil Ragep has focused on the evidence constructively. His devastating, if somewhat inflammatory criticism of my reliance on a reconstruction of Nicole Oresme's models by Garrett Droppers was entirely just, although embedded in comments that overlooked the inquiring nature of my suggestions. Be that as it may, Ragep went on to show how Oresme did, it seems, describe a Tusi-couple model, although we continue to disagree on the question of whether and to what extent Oresme was dependent on predecessors (see Ragep 2014). In the relevant statement from *Questiones de spera*, Q. 13, Oresme says in response to objections by Aristotle and Averroes the following: "It is possible for some planet to be moved, according to something in its nature, perpetually in a rectilinear motion [that is] a composite of several circular motions, so that this motion proceeds from several intelligences, any of which intends to move by a circular motion and is not frustrated in this intention" (ed. Garrett Droppers 1966, pp. 283-285 [translation slightly modified]). There is no indication in that description that Oresme relied on a source. His subsequent proof is difficult to follow, but Ragep has supplied a reconstruction that seems to fit. As for Ragep's protestations about conflation of a Tusi-couple with a Eudoxan-couple, even Ragep reports that Tusi himself presented the curvilinear version of his couple as an outgrowth of al-Haytham's Eudoxan couple model, that is, that he used "it as a starting point with which to deal with the difficulties of Ptolemy's latitude theory" (Ragep 1993, 2:453).

satisfactory.

As readers may surmise from the section above on postulates, I am looking for matters on which we agree or, at least, may be able to agree. It is probably hoping for too much to propose calm, reasonable, and fair discussion of the arguments and evidence, but I proceed here with a brief discussion of the main strengths and weaknesses of both sides, and conclude with a suggestion about a middle ground between blueprint copying and independent development. The editors cite the literature, excluding items that appeared after the edition went to press, but they do not consider adequately the strongest arguments, in my view, made in support of Copernicus's acquaintance with Maragha precedents. In his 1973 edition and commentary on *Commentariolus*, Noel Swerdlow presented an argument that has the character of a "consilience of inductions" (469). After raising questions about Copernicus's understanding of the fundamental properties of the first anomaly, and whether as a consequence his model was his own invention or something he learned from a still undiscovered transmission to the west of a description of Ibn ash-Shatir's planetary theory, Swerdlow argues as follows:

My own inclination is to suspect the latter, not because I think Copernicus incapable of carrying out such an analysis of the first anomaly in Ptolemy's model (he certainly shows considerable ingenuity in deriving the heliocentric representation of the second anomaly), but rather because the identity with the earlier planetary theory of Copernicus's models for the moon *and* the first anomaly of the planets *and* the variation of the radius of Mercury's orbit *and* the generation of rectilinear motion by two circular motions seems too remarkable a series of coincidences to admit the possibility of independent discovery.

(Swerdlow 1973, 469)

The strength of the argument rests on the convergence of four independent assertions and on the improbability of a coincidence. The second strong argument derives from Copernicus himself. In the autograph of *De rev.* III, 4 and III, 5, passages that were deleted in the final version, he indicates that others had called the model of reciprocal motion "motion along the diameter of the circle." The editors discuss these passages (III:257-259), but in questioning Copernicus's dependence on the Tusi-couple, perhaps preferring a dependence on Proclus, they are too hasty in dismissing the implication that versions of these mechanisms were known in the 16th century. I shall return to this issue below, but let us consider first Swerdlow's claim about the improbability of coincidence among four independent items.

The independence of the items, when examined more closely and individually, exposes a weakness. Let us take them in order, referring also to arguments of other proponents that reveal the problem in its clearest form. First is the Tusi-couple, a version of which Copernicus used in *De rev.* III, 4. Proponents describe Copernicus's version, sometimes explicitly but at least implicitly, as an example of blueprint copying. Some, referring to the Tusi-couple, also claim

that he copied it from some unknown source. Furthermore, their language is puzzling. Although the models are very similar, they are not identical, yet some of them continue to insist on their identity. Accordingly, we encounter nonsense such as the following: the versions are not perfectly identical but are said to be identical nonetheless.³ In fact, even allowing for the fact that Copernicus saw a copy of al-Tusi's version, they are not identical, meaning that if he saw the earlier version, he changed it. In the autograph version, routinely ignored by proponents, Copernicus rotated the outer circle 45° clockwise, and added another circle. The differences may be trivial, but they require explanation. Instead, proponents ignore them, almost always referring to the version that appears in the 1543 edition (also not completely identical with the earlier versions), which raises the additional question of why Copernicus and/or Rheticus altered the figure as it appeared in the autograph. Were they relying on some more recent source, or was the reason itself a matter of conformity with the way other similar figures are presented in the edition? To my knowledge, there has been no examination of this matter. Instead, we get the following sort of explanation:

This is not to say that there are no remaining difficulties with attributing knowledge of Islamic astronomical models to Copernicus. We do not know in detail where, when or how Copernicus acquired this knowledge. We do not know how he bridged the gap between the original language and the Latin in which he himself worked. [. . .] In this paper we will suggest that the answers to the open questions outlined above are becoming more secure, but are still not firm.

(Barker and Heidarzadeh, 22)

After admitting and conceding these difficulties and others, they jump to a conclusion that we can describe only as a textbook example of a non sequitur:

However, the basis for attributing knowledge of Islamic astronomical models to Copernicus is so firm that it cannot be seriously challenged.

(ibid.)

The paragraph should have ended with the sentence preceding the conclusion. The facts stated there represent the state of our knowledge and our ignorance of Copernicus's precise hypothetical source.

The second issue concerns the double-epicycle model and Copernicus's reliance on Ibn ash-Shatir for his version. Swerdlow (456) says that Copernicus's lunar model, except for its parameters, is identical to the model of Ibn ash-Shatir. Later (504) he says that he "copied" ash-Shatir's model for Mercury without fully understanding it. What in fact are compared are modern versions of the models, not the ones found in *De rev.* IV, 3 and V, 4 (where Earth's annual motion is included). Now, the descriptions indeed show that

³ For example, see Barker and Vesel 2012, 329. For my response see Goddu 2013, 252-253.

these are bi-epicyclic and epicyclet models respectively, but where are the versions that Copernicus supposedly copied? As far as I know, there is no example of a model in the sources that Copernicus could have copied, so how could this possibly be a case of blueprint copying? What original did Copernicus copy, and where is it? Now, even if my remarks expose a weakness in Swerdlow's "consilience argument", namely the implication that Copernicus merely copied models that he had seen, they do not touch on the apparent fact that there was acquaintance with, if not models, then at least descriptions of or, even more generally, ideas about reciprocation and bi-epicyclic models in the 15th and 16th centuries, and possibly even as early as the 14th century in Western Europe.

Taken together, these enigmas argue against blueprint copying, although they are not proof of independent development either. Here is where, in my view, some opponents have made a mistake. On the other hand, mere questioning of Copernicus's reliance on Maragha precedents has evoked a disproportionate response from supporters, especially Ragep and Barker and Vesel. For example, in reaction to my first effort in this regard, they either ignored the tone of questioning that I clearly established, or they ignored my plea for additional archival research to discover the source that Copernicus used. The latter in particular has been greeted with a flood of more speculation, missing the point altogether. Ragep justly and correctly demolished my reliance on a flawed reconstruction by the late Garrett Droppers of an account by Nicole Oresme, but to his credit Ragep went on to propose a better interpretation that does indeed suggest a version of the Tusi-couple. My reconstruction was based to a large extent on the possibility that Oresme's version could have been transmitted indirectly to Copernicus, but that would clearly mean that Copernicus did rely on predecessors, and so his version could not have been a completely independent development on his part. That admission seems to have eluded Ragep and Barker and Vesel.

Further reflection on these circumstances has also led me to re-evaluate my criticism of the discovery by Jerzy Dobrzycki and Richard Kremer of a source that was available to Copernicus around 1510 (Dobrzycki and Kremer 1996, 187-237). Their focus was on the double-epicycle model. At first, I saw this as just another example of the same sort of argument advanced heretofore, but I overlooked their explicit assertions about a more widespread and diverse transmission of sources and models, which at the time I did not distinguish from the blueprint-copying versions. In his own inimitable and straightforward way, Owen Gingerich concluded that Copernicus may have learned of the double-epicycle model without any direct knowledge of Maragha precedents (Gingerich 2004, 264). What is especially intriguing about Dobrzycki's and Kremer's argument and Gingerich's suggestion is that it leaves room open for a middle between blueprint copying and independent development. They are perhaps not as explicit or clear about this as they could have been, but in any case their version suggests the need for another category. Equally important is

the possibility that Ragep might be open to the suggestion that follows.⁴

This brings me at last to my suggestion. Drawing on examples from the history of technology and anthropology, I suggest that we introduce into this discussion a middle category called “idea diffusion”.⁵ Idea diffusion is more nebulous than blueprint copying, but it also undermines independent development. In this case, we would be looking for sources in the 15th century, derived from Georg Peurbach, evidently transmitted to Albert of Brudzewo, Johannes Angelus, and probably others, and these are sources that we can at least place closer to Copernicus. For instance, in Brudzewo’s *Commentariolum* we find a spherical version of a double-epicycle model. Barker and Vesel have rejected this suggestion as too different from the kinematic version developed by Copernicus, but this objection clearly reveals the circularity of their argument.⁶ Copernicus, they claim in effect, must have copied a version that he saw because that was all that he was capable of doing.

On the contrary, the version by Brudzewo and the reliance by Angelus on versions of the model may have given Copernicus sufficient information for him to develop his own version, and, in any case, he was thoroughly original in applying the solution to the motions of the planets in a heliocentric framework (in *Commentariolus*), and then adapting them for his mature version in *De revolutionibus*. In other words, he could have relied on suggestions or verbal descriptions that derived ultimately from Maragha sources without the slightest awareness of their origin.⁷

Earlier I had referred to the principle of methodological prudence. Here again in the case of Copernicus’s acquaintance with Maragha precedents, there are reasonable doubts and questions. It seems likely that the Maragha models or, at least, their ideas were known in western Europe in the 15th and 16th centuries, in which case we would be talking about idea diffusion rather than blueprint copying or independent development. It seems to me that such a middle category fits better with the evidence provided by Dobrzycki and Kremer, Robert Morrison (2014, 32-57) and even Barker and Heidarzadeh. The interpretation of the editors of the French edition, recognizing European sources avail-

⁴ See Ragep’s concluding remarks in Ragep 1997, 46-47.

⁵ For a readable, jargon-free, example, see Diamond 1999, where he shows how re-invention of details of porcelain production depended on descriptions or models of earlier versions, the exact details of which were vague.

⁶ Barker and Vesel, 319-332. In 1973, Swerdlow did not share the assumption that Copernicus was incapable of constructing the figure (469). Ragep called my suggestion “bizarre in the extreme,” yet in his conclusion says: “Given what we know, it would seem that one possible scenario is that Copernicus was indeed influenced by Brudzewo’s comments to pursue the problem of the moon’s epicyclic apogee. And perhaps he realized at some point that what was needed was a curvilinear oscillation on the epicycle’s circumference, as Tusi had before him” (1997, 33). He also refers to Dobrzycki and Kremer in his concluding paragraph.

⁷ I have not included “models” in the sentence because the hypothetical source, as the comments above indicate, is not as specific as Copernicus’s known acquaintance with Brudzewo’s commentary or possible acquaintance with Angelus’s ephemerides.

able to Copernicus, also fits better with idea diffusion than with independent development.

3. Robert Westman's Thesis about Astrology and Astrological Culture

The next two issues also involve evaluation of evidence, and here I begin with the editors' rejection of Robert Westman's thesis about the role of astrology and astrological culture in motivating Copernicus to develop his theory.

The editors have rejected Robert Westman's account of the origin of Copernicus's heliocentrism (Westman 2011) on the grounds that they find it insufficiently supported by the sources (for example: I:90n3; I:388n2; I:391-395). As readers of my work know, I myself prefer an account that focuses on the problems that Copernicus recognized with Ptolemaic astronomy, and although I consider contextual matters seriously throughout, especially Copernicus's reliance on predecessors and early printed books, my reconstruction is closer to those presented by Noel Swerdlow and Bernard Goldstein. Recently, I have published an article commending the reconstructions of Ludwik Antoni Birkenmajer and Curtis Wilson, with which I am very sympathetic, all of which is to say that my proclivities lean towards accounts that begin with the technical problems (qualitatively considered) that bothered Copernicus and the solutions that he proposed for solving them (Goddu 2016, 225-253).

That said, I have also acknowledged the under-determination of Copernicus's theory, by which I understand the absence of strict proof for his heliocentric cosmology. Such under-determination has been one of the main motivations for Robert Westman's reconstruction, leading him to seek more contextualist and practical reasons that persuaded Copernicus to adopt a heliocentric theory. On this account, Copernicus recognized problems with the theories underlying the practice of astrology, with the relation between astrology and natural philosophy, and with the reliability of astrological forecasts. Because Copernicus says little about astrology, Westman's account emphasizes the culture of astrology during the long sixteenth century, and the reasons why it is unlikely that Copernicus was exceptional in his regard for astrology. The strongest textual evidence comes from Copernicus's reference to Pico della Mirandola's *Disputationes adversus astrologiam divinatricem* (1496), Rheticus's enthusiastic comments about astrology in *Narratio prima*, and indirect evidence (a letter by Bernard Wapowski) of Copernicus's having produced an ephemerides (the last overlooked by Westman). Westman, like Karl Burmeister, believes that Copernicus worked closely with Rheticus in the composition of the *Narratio*. Now, reviewers have pointed out problems with this evidence, and I have to admit that I share some of these doubts. First, Rheticus worked his way through Copernicus's manuscript quickly, and most experts concede that he did not master all of the details. In fact, there are errors, and it is curious that Copernicus did not correct them. On the other

hand, there are passages in the *Narratio* where Rheticus expresses a feature of the theory more clearly than Copernicus himself does (for example, III:412n7). Second, Rheticus presents his astrological interpretation as if it were his own idea and contribution. Third, his citation and “refutation” of Pico does not refer to the passage that Westman believes influenced Copernicus. To the last objection Westman responds that they were in Löbau, not Frombork, when they composed *Narratio*, and did not have the sources in front of them to check. Are we to assume that Copernicus forgot the source that supposedly inspired him to undertake the reform of astrology and astronomy? Fourth, we presume that Copernicus owned a copy of *Narratio*, but it has evidently disappeared, leaving us without a source that could possibly answer the relevant questions about his relationship with Rheticus and the writing of *Narratio*. Fifth, in the dedication to Pope Paul III, known for his encouragement of astrological practice, and whom Copernicus praises for his love of mathematics (II:9; III:59-60), it is curious that Copernicus passes up an opportunity to even suggest how his new theory will contribute to the reform of astrology.

All of that said, the dismissal of Westman’s thesis has been hasty in my view. One of the issues that deserves closer examination is the relation between the ordering of the planets and the effect that this would have on astrological prognostications. Westman thinks that determining the order was subservient to the aim of reforming astrology, but one could just as well argue that the relationship to astrology provided an additional reason for Copernicus to resolve one of his primary concerns, the unique ordering of the planets. He was, after all, explicit about the disagreements among geocentrists over the ordering of the planets, concluding that geocentrism did not possess the resources to settle the question. To Copernicus it was scandalous to suggest that the most perfect artisan had not ordered the planets according to a definite and knowable plan. Copernicus may have been more concerned, still, with the relation between astrology and natural philosophy than with astrological prognostication where not just the order of the planets but also the motions of the planets in longitude ought to be known with greater certainty. Indeed, in Wapowski’s letter he refers explicitly to modifications of the atmosphere. In other words, astrology provided another reason to settle definitely questions about the motions of the planets in longitude, the ordering of the planets, and the consequences for natural phenomena in the sub-lunar world.

The different propensities or inclinations that authors have towards different kinds of solutions leave us at an impasse. There is no objective way to resolve these differences. They are more than just matters of taste, but rather testify to deep-seated beliefs that cannot be analysed any further or adjudicated in a definitive way. Any continued argument over them is repetitive and circular. Where one finds the technical analyses deficient and even shallow, the other finds the contextual unpersuasive and trivial. Such differences cannot be settled by amassing more evidence — the differences prescribe the weight assigned to different kinds of evidence, and so the conclusion reached comes down to dif-

ferent philosophical and historical dispositions. My comments above suggest steps towards a compromise, but for those already committed to one or the other approach, the different inclinations will likely remain unaffected.

4. Dilwyn Knox on Copernicus's Sources

The editors acknowledge Dilwyn Knox's important studies on Copernicus's theory of the elements and elemental motion (see for example Knox 2007). Citing the principle of methodological prudence, however, they are at best non-committal with respect to Knox's hypothesis about Copernicus's reliance on a text from the *Suidae Lexicon* for details about his analysis of natural and violent motions. The editors (III:118-119) are correct to point out differences between Knox and me about Copernicus's reading of the passage, but I agree with Knox that Copernicus probably consulted the passage. There are some peculiar features in Copernicus's assertions that parallel the text on *kinesis* that we find nowhere else. Now, we do not know for a fact that this lexicon was in Varmia, but, unlike Albert of Brudzewo's *Commentariolum*, copies of which are rare, the lexicon was well known and widespread. In short, there are connections here that point uniquely to the *Suda* as Copernicus's source. The editors' doubt that Copernicus's Greek was good enough to use the dictionary does not strike me as altogether fair. After all, Knox did not claim that Copernicus read the entire dictionary, but merely one verifiable entry. I see no reason to doubt his ability to work his way through that entry and find comments that he thought were relevant to his critique of Aristotle's assumption about the simplicity of natural elemental motions. It seems to me that Knox has been very cautious in all of his publications, examining the sources meticulously, and correcting indeed my initial doubts about Copernicus's use of both editions of Pliny's *Natural History* that were in Varmia (Knox 2013, 77-86; Knox 2012, 111-148).

5. Some Final Brief Comments

I conclude this review essay with some reflections on the chronology of the writing of *De revolutionibus*, its title, the genesis of Copernicus's heliocentrism, the Galileo affair, and the major achievement that this edition represents.

The editors are rightly cautious about issuing any definitive resolution of disagreements about the dating of Copernicus's major text. Copernicus may have begun composing *De revolutionibus* between 1524 and 1529 (I:101; I:563-577). Based on the earliest paper that Copernicus used, Edward Rosen thought that he wrote some parts as early as 1515, but Jerzy Zathey moved that date up to 1520 and concludes that much of it was written in the 1530s. The editors also point out that the catalogue of stars, in particular, probably went through several stages before the version in the holograph (III:475-478). Furthermore, Copernicus entered values for the apogees of the planets in the margin of the catalogue, but he may have added them much later, leaving the dating of the

earliest stages of the catalogue perhaps earlier than 1520. The fact is that we do not know exactly when the earliest paper was available. For his part, Noel Swerdlow also concludes that Copernicus wrote most of the text in the 1530s, and the evidence seems to support his analysis. Jarosław Włodarczyk has made a useful distinction between a “long” and “short” chronology, which readers should consult (Włodarczyk 2015, 31-34). The editors provide a superb summary of all of the major evidence, explaining in meticulous detail the difficulties. On one detail there may be need for a slight revision, which, however, could have a consequence for the dating of the holograph. Copernicus evidently consulted George of Trebizond’s translation of Ptolemy’s *Almagest*, which was first published in 1528, for a comment in Book I and some other corrections. It has been generally assumed that Copernicus did not consult manuscripts; however, we should note that Grażyna Rosińska reports two manuscript versions of parts of Trebizond’s translation, one of which was copied by Martin Biem of Olkusz, dated to the second half of the 15th century, and the second is assigned to the first half of the 16th century.⁸

Likewise, we do not know with certainty the title that Copernicus preferred. The title of the 1543 edition is *De revolutionibus orbium coelestium*, but because Rheticus crossed out the words “orbium coelestium” in his copies, the editors suppose that Osiander added those words in addition to the unauthorized “letter to the reader”, and also because Rheticus believed them to refer to the “compensatory counter-turning” spheres of Eudoxos-Callippos-Aristotle (III:443-450). Although Copernicus probably preferred the title *Libri revolutionum*, it was not because he objected to celestial spheres. Almost all experts are in agreement that Copernicus adopted the existence of celestial spheres.

On the origins of Copernicus’s heliocentrism, the editors refer to Copernicus’s own comments about his objections to the equant (I:328-329; III:355n2; II:342-343; III:361n6 on the annual motion of Earth as cause of apparent non-uniformities), but they seem to be partial to Bernard Goldstein’s account (I:373-381; see Goldstein 2002), and they cite a paper by me as essentially in agreement with Goldstein’s analysis (Goddu 2006, 37-53). Unfortunately, they apparently overlook my observation that Goldstein accounted for Copernicus’s *acceptance* of heliocentrism, not its *formulation*. In other words, Copernicus adopted a theory, according to Goldstein, that was already formulated. It is curious that the editors would overlook this distinction, for they argue at some length for Copernicus’s originality (I:386-390) and that none of the theories prior to Copernicus, not even those cited by Copernicus himself, was a precursor of his theory. As they themselves maintain, whatever ideas there were prior to Copernicus about the motions of Earth, the motions of the celestial spheres around a central fire, and the like, no one prior to Copernicus proposed all of these ideas, locating a static Sun at the center of the cosmos, and attempted to construct a complete astronomical system with all of the planets in motion around the Sun that could account for the observed celestial

⁸ Rosińska 1984, 307, referring to item no. 1563: MS, BJ 591 and BJ 592.

motions. According to the editors, once Copernicus understood that the relationship between distance and period of revolution could not be completely verified without putting Earth in motion, only then did he focus on the calculations. The starting-point, then, was a cosmological intuition, not a derivation based on the analysis of technical propositions. To put it in other words, in Copernicus's heliocentric theory the explication of a score of particular circumstances proper to planetary motions flow directly from the order, distances, and periods of the Earth and planets in ways that are either absent from or inexplicable in Ptolemy's geocentric system (I:273n1).

The conclusions that the editors have reached, accommodating as best as they could the most complete and persuasive accounts produced to date, probably represent the state of the question in a form that will satisfy those who focus on Copernicus's major works and their relationship to ancient cosmological tradition. That said, we must also acknowledge that many questions remain open, and that Copernicus's comments about his objections to the equant remain a problem that just does not go away, at least not for some of us.⁹

On the Galileo affair I have only some suggestions that touch on the relationship between Osiander's letter to the reader and the early stages of the affair between 1616 and 1620. The editors do not include Cardinal Bellarmine's letter to Foscarini among their texts, but they refer to it, and, in any case, the letter is well known in the field. For my purposes here, its most important features are Bellarmine's belief that Copernicus himself did not present his own theory absolutely but merely suppositionally, and his belief that Copernicus proposed his theory in order to eliminate eccentrics and epicycles. The first mistake is probably due to his ignorance of the authorship of the letter to the reader. The second probably derives from the figure that was published in *De revolutionibus* (I:10), which is highly misleading. I begin with Bellarmine's letter because it sets up the confusion over "hypotheses" that contaminated the entire affair down to Galileo's condemnation in 1633. While the heliocentric theory was literally condemned in 1616 as contrary to Sacred Scripture, Copernicus's book was not absolutely prohibited but rather suspended until it could be corrected. Why? The authorities recognized that there was much in the book that was useful, but we may also suppose that they too were unaware of Osiander's authorship of the letter. Had they known that Copernicus held his theory as absolutely true, then they might have prohibited the book altogether. The evidence for that conclusion appears in the censorship document of 1620, where

⁹ I have attempted to deal with that puzzle in the article on Birkenmajer and Wilson cited earlier, but I have no illusions about a final resolution of reasonable differences of opinion. One of the remaining problems concerns the complications in Copernicus's models for Venus and Mercury. Because Earth's annual motion could account for the maximum elongations of Venus and Mercury from the Sun, Copernicus's intention seems to have been to eliminate their epicycles altogether. Yet, in the Mercury model, for example, his own principles led him to restore an epicyclet with the planet moving not on its circumference but along its diameter (II:398-401; III:394-397). In II:417-419 (see also III:409n4), however, Copernicus did propose another solution without epicyclet.

the committee says that Copernicus's book had been completely prohibited in 1616. If that had been the case, then surely the Congregation would not have suspended the book until corrected. Why correct or censor a book that has been completely prohibited? Now, the authors of the 1620 document address that question by emphasizing again the utility of the book, but it seems to me very likely that by 1620 the Congregation of the Index had become aware that Copernicus had really believed his theory, yet the committee completed the task which it had been assigned, the censorship of the book.

There is some circumstantial evidence to support my emphasis on the confusion caused by Bellarmine. One other problem requires a correction, however. In some comments on the affair, the editors characterize Osiander's position on astronomical hypotheses differently from their own edition. The edition (II:2, lines 13-15) reads: "Neque enim necesse est, eas hypotheses esse ueras, imo ne uerisimiles quidem, sed sufficit hoc unum, si calculum observationibus congruetem exhibeant, [. . .].", translated: "Il n'est en effet pas nécessaire que ces hypothèses soient vraies, ni même vraisemblables, mais il suffit qu'elles fournissent un calcul qui s'accorde avec les observations, [. . .]."¹⁰ In a comment on a report by Francesco Ingoli in 1618, in which he adopts a very pragmatic view of hypotheses, the editors say (III:183n5) in referring to the view of Osiander: "Ce dernier dit qu'une hypothèse astronomique n'a pas besoin d'être vraie, il suffit qu'elle soit vraisemblable, pourvu qu'elle donne un calcul s'accordant avec les apparences célestes" (*Ad lectorem*, p. 213-14). The relevant phrase is "it suffices that they be likely". Aside from the fact that the cited text says that the hypotheses need not be likely, the note even refers to the text cited above. This discrepancy is inexplicable.

Ingoli's comment, in fact, reflects the distinction made by Bellarmine. The second point to observe is that by 1618 Ingoli may have realized that Copernicus did not write the "letter", for he says (III:638-639) that Copernicus treats the movement of the Earth in a non-hypothetical manner, but if the passages are rendered hypothetically, they will not be contrary to the truth or to Sacred Scripture. The point is that sometime between 1616 and 1620, perhaps as early as 1618, members of the Congregation of the Index may have learned that Copernicus held his hypotheses absolutely, but they continued to repeat the explanation that Copernicus's book was useful for a number of reasons, and so should not be completely prohibited. One might conclude that the authorities were trying to find a compromise that would make clear their condemnation of the theory without completely banning the book. In the event, they seem to have sewn only confusion.

The confusion over hypotheses persisted until 1633. Galileo had used the distinction between an absolute and hypothetical adoption of heliocentrism as a pretext to offer what support he could to the theory. He succeeded in fooling no one. In the sentence of 1633, the inquisitors spelled out his error in unmistak-

¹⁰ "It is not necessary for the hypotheses to be true, nor even likely; it is sufficient that they furnish a calculation that agrees with the observations."

ably clear terms. His error was that he had held as “probable” a theory already declared contrary to Sacred Scripture, for there is no way that a theory contrary to Sacred Scripture could be possibly, let alone probably, true.

Readers might understandably conclude that I find the new edition disappointing and unsatisfactory. On the contrary, it is by far, in my view, the most comprehensive and reliable study of Copernicus and his works ever published. Only use of the text over time will tell whether it is the definitive edition, but its many virtues make it an indispensable contribution, and one that marks a new turning-point in Copernican scholarship. By that I mean that anyone venturing to discuss the Copernican Revolution will have to turn to these volumes, and consider the editors' methodological principles, applications of those principles, evaluation of previous scholarship, and interpretations of the reception of the Copernican theory. On technical matters the editors have relied extensively on what everyone recognizes as the most exhaustive analysis ever produced, the study by Noel Swerdlow and Otto Neugebauer (1984). The editors have checked every calculation, but it will require more time to assess their work completely. On some matters they disagree with Swerdlow/Neugebauer, but here again they have examined the evidence and explained their own take. The editors appear to have taken into account the criticisms by Swerdlow and Gerald Toomer of earlier editions, and of Edward Rosen's English translation. There appear to be differences of opinion regarding the errata sheet for the 1543 edition and Owen Gingerich's extended errata list based on handwritten corrections in copies of the 1543 edition, one example of which is from Leipzig. Another discrepancy concerns whether there was a working copy containing the revisions that appear in the 1543 edition — a hypothesis rejected by the French — in addition to the fair copy that Rheticus took with him to Nuremberg. But consistency is difficult to maintain; even Swerdlow, who argued that the 1543 edition should be the basis of a critical edition, deplored the deletion of the introduction to Book I.

On the relation between the French edition and the 1984 German edition, the French edition differs from the 1984 German edition in that it preserves the Roman numerals of the 1543 edition, which helps in evaluating whether an error is computational or the result of a slip of the pen while writing a Roman numeral. The French also followed Swerdlow's advice in having checked the computations and set out the textual errors in their critical edition, but Swerdlow did not advocate a new edition. On the other hand, Swerdlow did not advocate a new edition even after the appearance of the Polish edition, flawed as it was. Swerdlow's advice, if followed, would require readers to create their own text, but surely readers capable of doing that could also use a critical edition as long as they read it critically. In other words, a critical edition frees us from having to reinvent the wheel while still requiring us to check its condition from time to time.

Here and there the editors have supplied in the notes (not the text) improved figures for Copernicus's, but readers will have to study them carefully to under-

stand the improvement. I suppose that some critics will deplore the decision not to print the edition in folio (the details of some figures in octo format are too small to read without a magnifying glass); on the other hand, the format is handy and attractive. For example, their figures illustrating Copernicus's explanation of retrograde motion (III:413) may confuse some readers. In Fig. 13a, if we extend the lines T₂-P₂, T₃-P₃, and T₄-P₄ beyond the page, we can see that they will eventually intersect against the background of the stars, meaning that from T we will see them in the following order, P₁, P₄, P₃, P₂, and P₅ (just as Fig. 13b depicts them). The commentary notes in Vol. III do not clutter the edition in Volume II, and having the French translation side-by-side with the Latin edition and variants at the bottom of the page is more than a convenience — it makes failing to consult the Latin text inexcusable. Remarkably, in over 2500 pages of text, of which I read about 1600 pages closely, I found only one typographical error — a period missing at the end of III:433n11!

Also on the subject of the Greek edition of the *Almagest*, Edward Rosen concluded that Copernicus received a copy of the Greek edition too late to take full advantage of it. The editors offer a remarkable reconstruction of one chapter from Book V (ch. 35) and thirty passages from Book VI, that contrary to Rosen, shows that Copernicus revised the text as it appears in the 1543 edition according to the Greek edition and, they suppose, a Latin translation probably supplied by Rheticus. In their reconstruction (III:512-568), which also examines Book V, 36, they designate the 1538 Greek edition as G, the 1515 Latin translation by Gerard of Cremona as L¹, and the translation by Trebizond as L². By comparison of these versions with the text of 1543, they demonstrate that the revised version did not rely on the older Latin translations but rather on the Greek text from 1538. This is, of course, an analysis that will require careful examination, but if it stands up to scrutiny, it will probably constitute their major contribution to the reconstruction of the 1543 edition.

There is a good deal more in these volumes, but my considerations surely confirm the importance, indeed indispensability, of the new edition with its introduction and complementary notes for further research on Copernicus's achievement.

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