The Copernican Question Revisited: A Reply to Noel Swerdlow and John Heilbron

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The Copernican Question *advances a radical reinterpretation of a classic episode in the history of science. Copernicus's turn to the heliocentric planetary arrangement occurred in the context of a late-fifteenth century political/religious controversy about the credibility of astrology triggered in 1496 by Giovanni Pico della Mirandola's attack on the science of the stars. This controversy about the principles of astrological prognostication continued to drive debates about the heavens from the late-fifteenth to the early seventeenth century. The reviewers conceal their defense of the historiographical status quo ante by focusing on matters of translation. The rebuttal demonstrates that the real disagreements are over method and interpretation.*

In separate reviews of *The Copernican Question* published in the Summer 2012 issue of this journal, Noel Swerdlow and John Heilbron find little that meets their approval while failing to provide readers with a full and accurate summary of the book's major claims and arguments.* The reviewers engage in an exercise in deconstructive surgery, essentially breaking down and reconstituting the work into separate studies. Swerdlow, who devotes most of his twenty-five page treatment to chapter 3 (with brief side-glances at the introduction, chapters 1, 8, and 11), leaves the impression that my book is almost entirely about Copernicus. Heilbron, who confines himself mostly to what I have to say about Galileo, identifies

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*The book review editor offered no opportunity for a simultaneous reply; to assist readers I frequently provide quotations from the reviews.

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a handful of translation mishaps in chapters 13, 14, 15, and 18 but fails to show that these errata have any major interpretive import. Neither reviewer mentions that in most cases of particularly significant quotations, especially from sources not easily accessible, I provide the original languages (Latin, Italian, German, French), inviting readers to check my translations and come to their own conclusions. Along the way, the reviewers leave their own trail of errors and distortions: omission of key references and major theses, misrepresentation of specific claims, empty or inflated complaints about mistranslation and allusions to phantom nonsequiturs—all designed to foster the pessimistic induction that none of the evidence preferred is to be trusted.

What Are the Book's Central Claims?

My central claims are twelve in number.

First, *classifications of knowledge* are bound to time and place. In Copernicus's lifetime (1473–1543) and well into the seventeenth century, astronomy and astrology constituted a *compound subject* called the "science of the stars." Each part of this disciplinary couple could be further subdivided into theoretical and practical parts. Authors who contributed to the literature of the heavens described themselves with various names that no longer carry current meanings, such as "mathematician" or "physician and astronomer"; we might best describe them, retrospectively, as "astronomer-astrologers." Most commonly, they located the topic of planetary order in the domains of theoretical astronomy and theoretical astrology.

Second, Copernicus's initial turn to the heliocentric planetary arrangement occurred in the context of a late-fifteenth century *political* controversy about the credibility of astrology triggered in 1496 by Giovanni Pico della Mirandola's attack on the science of the stars.

Third, the controversy about the principles of astrological prognostication persisted as a major motive that drove debates about the heavens from the late fifteenth- to the early seventeenth century. Those debates took place within a nexus of political-cultural arrangements defined by the churches, the universities and the royal, princely and imperial courts.

Fourth, in the face of Pico's critique there were different kinds of efforts to improve astrological prognostication during the sixteenth century and Copernicus's proposal to reform theoretical astronomy was but one of them.

Fifth, the appearance of *unforeseen, singular, celestial novelties* between 1572 and 1604 pushed some astronomer-astrologers to consider whether alternative planetary orderings, including Copernicus's, could better *explain* the unanticipated phenomena.

Sixth, this consideration of alternatives was the first major instance of *underdetermination* in the history of science, although the historical agents were unaware of the epistemological generality of that problem. It resulted in new *kinds* of controversies and raised unprecedented questions about weighting the criteria for adjudicating among different hypotheses, including ancient authority, scriptural compatibility, simplicity, explanatory breadth, predictive accuracy and physical coherence.

Seventh, the sixteenth-century followers of Copernicus did not constitute a socially and intellectually unified movement and the failure of Galileo and Kepler to forge a productive alliance around the Copernican theory is a particularly notable instance of this larger pattern.

Eighth, shared social context *underdetermined* the adoption of new theoretical claims. Many Copernicans, for example, were attracted to court settings because those spaces were more open to novelty than university settings. But while court *patronage* allowed for rhetorical and philosophical diversity, it fails to explain why particular figures, like Galileo, adopted specific theoretical claims, such as the Copernican hypothesis.

Ninth, Galileo's telescopic claims introduced *recurrent novelties* into the debate about alternative hypotheses. Unlike novas and comets, which seemed to appear only when God wanted to send a message, a human being could make phenomena like the moon's rough surface, never-before-seen distant stars or Jupiter's "planets" appear and disappear. Success in convincing others of the reality of these phenomena occurred largely through print rather than by live demonstrations with the instrument.

Tenth, the main locus of change of belief was not some twentieth-century-like "scientific community," but the *master-disciple relationship,* rooted in the all-male cultures of the universities and modeled on the paternalistic structures of the family.

Eleventh, *The Copernican Question* proposes a new periodization. Rather than "Copernicus and the reception of his theory," it argues for a "Long Sixteenth Century" which began with the late-fifteenth century conflict about the status of astrological prognostication; it ended in the early seventeenth century when the Catholic Church extended its skepticism (and its enforcement machinery) about naturalistic foreknowledge to the reality of the heliocentric planetary ordering.

Twelfth, Kepler's *Epitome of Copernican Astronomy* (1618–21) and Galileo's *Dialogue Concerning the Two Chief World Systems* (1632) consolidated a critical mass of claims, arguments and diagrams developed between the 1580s and the telescopic discoveries of 1610–1612 and made possible a multifaceted, robust public debate that involved a new breed of natural philosophers, the likes of Descartes, Gassendi, Mersenne, Hobbes and Wilkins.

How Do the Reviewers Summarize the Book's Major Claims?

The bulk of Swerdlow's synopsis is concerned with claim no. 2. He makes his first summary statement five pages into his review: "Professor Westman believes that Pico's work was of great importance to Copernicus, that Copernicus wished to rescue astrology from Pico's criticism, indeed, that Pico's criticism of the uncertainty of the order of the planets lies at the foundation of Copernicus's formulation of the heliocentric theory" (Swerdlow 2012a, p. 358). Twelve pages later, he asks, "Was Copernicus concerned with astrology?" (Swerdlow 2012a, p. 367) without observing that if Copernicus was concerned with Pico's critique, then he must have been "concerned with astrology." Seven pages beyond, he completes his summary with a backhanded compliment: "After finishing Pico and Copernicus, this history of what Professor Westman calls 'the long sixteenth century' *declines somewhat from its previous level* and seems to lose its way as it wanders around and around, roughly chronologically, through a variety of *lesser-known* figures . . . as well as *the most well-known,* Tycho, Kepler, and Galileo. . . . It is difficult to understand the purpose of the remaining fourteen chapters . . ." (Swerdlow 2012a, p. 374; my italics). Of course, it is difficult to understand how "lesser-known figures" could be of any possible interest if one considers the history of science to be the history of solitary geniuses.

John Heilbron's summary more or less concerns itself with claims no. 2 and 6: "The question of *The Copernican Question* is why Copernicus and his few early followers took the trouble to rework Ptolemy for a heliocentric world . . . why individuals accepted a Copernican theory (it came in various forms and in various degrees) at the expense of received learning" (Heilbron 2012, pp. 379–80). "A subsidiary question is how theories 'travel': how an important innovation tossed up at particular place and time becomes widely accepted" (Heilbron 2012, p. 379). And, in a final gesture: "The strength, timing, and possible fluctuations of Galileo's commitment to heliocentrism are standard problems in the literature" (Heilbron 2012, p. 380).

The reviewers' own interpretive investments guide their impoverished summaries and cherry-picking of my central claims. Other difficulties arise from confusions about Kuhn. Heilbron's summary of my engagement with *Structure* is so disjointed and distorted as to be barely intelligible (Heilbron 2012, pp. 387–88). Swerdlow mischaracterizes my work as an "attack" on Kuhn's *The Copernican Revolution* (Swerdlow 2012a, p. 377), which he regards with a reverence normally reserved for sacred scripture.[1]

1. Swerdlow 2012a, p. 377: "I believe [Kuhn's] *The Copernican Revolution* contains within its larger history, from antiquity to Newton, an account of the period from Coperni-

Much of my disagreement with Kuhn arises from his immunization of Copernicus from any astrological concerns, a perfectly understandable position that Kuhn held in the context of 1950s historiography of science. (Kuhn 1957, p. 94; Westman, 1994, pp. 89–90). But, as I will show below, Swerdlow seems unaware that his position on Copernicus's engagement with astrology is closer to mine than to Kuhn's and Heilbron's (Swerdlow 2012a, pp. 367–71). On the other hand, he appears not to have read Kuhn's more influential *Structure of Scientific Revolutions* (Kuhn 2012 [1962]). Inspired by Ludwik Fleck's *Genesis and Development of a Scientific Fact* (Fleck, 1979 [1935]), Kuhn called on readers to think of science as community-based practice—a challenge Kuhn's *Structure* famously posed in the heyday of internalism. Without attention to the concerns, practices, attitudes and debates of the "minor figures" who constituted those communities, it is difficult to understand from whence the motivating questions arose. That is a challenge my book directly addresses (CQ, p. xv).

Reconstructing Copernicus

What was the problem to which Copernicus's heliocentric arrangement was the answer?[2] Everyone who studies Copernicus knows that we lack the kind of direct evidence we would like to have in order to answer this question. Unless and until direct evidence comes to light, all we have are *plausible* reconstructions—although, it should be added, some are more plausible than others. This is a point that seems to be lost on Heilbron, for whom anything less than a direct statement from Copernicus merits the dismissive term "guess-thesis."[3] Swerdlow himself advanced a guess—actually, he referred to it quite candidly as "pure speculation"—in his 1973 commentary and translation of the "Commentariolus," Copernicus's earliest known, although unpublished, statement of his theory (ca. 1510).[4] He ar-

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cus to Galileo with a serious understanding of the astronomy and important issues, superior in comprehension and clarity to what I have here, a fraction of the length, for good reason read to this day, and I strongly recommend reading it."

2. I use the term "problematic" as a *noun,* by which I intend a "web of problems" or a "problem situation"—a term regularly employed in philosophical writing. Both Swerdlow and Heilbron are uncomfortable with this disciplinary crossover usage, a token of discomfort that they repeatedly mark with scare quotes.

3. "History is written from documents, not from guesses, and Westman has not exhibited the key evidence he needs to confirm his"; "the suppositious astrological motivation of Copernicus's work has no known documentary basis . . . [and] . . . rests on a shaky guess. . . ." (Heilbron 2012, pp. 380, 385).

4. Swerdlow, 1973, p. 478: "The final decision to let the earth move about the mean sun may have been determined by the intersection of the spheres of the mean sun and Mars

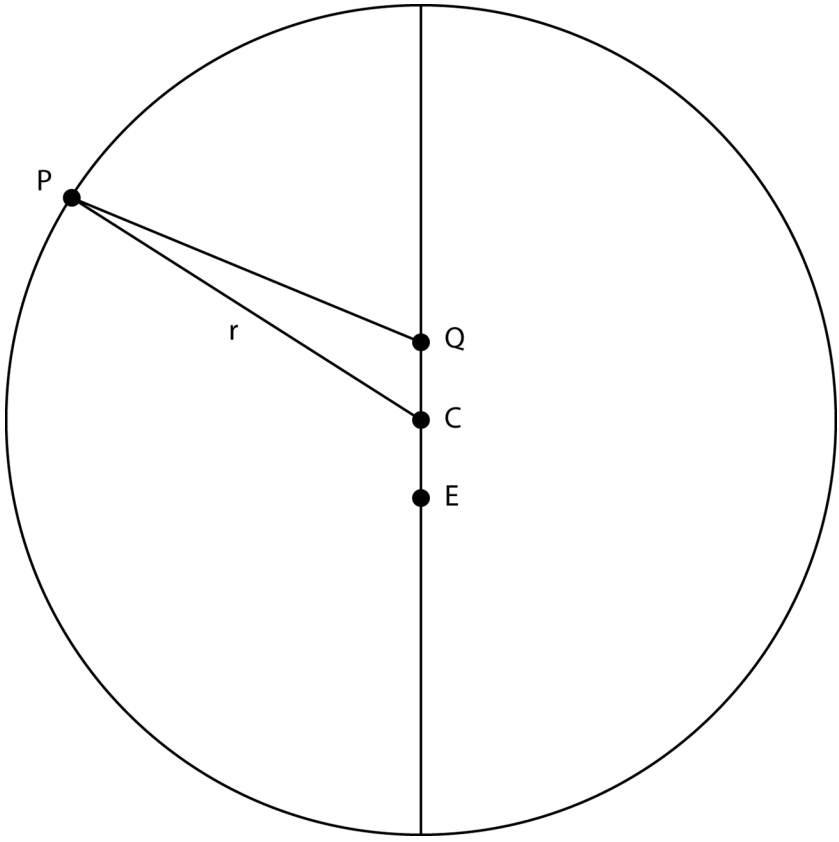

Figure 1. Equant Model. On a circle of radius *r,* point *P* moves with uniform angular motion around an off-center or eccentric point *Q,* but non-uniformly with respect to the circle's center *C* and earth *E.* QC = CE.

gued that Copernicus's problem situation originated in the choice of models appropriate for predicting the planets' motions. According to Swerdlow Copernicus believed that the planets were carried around by hard, impenetrable spheres and he believed that Ptolemy had violated his own axiom of uniform, circular motion by introducing the equant model that allowed spheres to rotate around a point other than their own center.

In a highly complex, technical analysis, Swerdlow then described Copernicus's move to a heliocentric ordering as a "derivation," a multi-step

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if the mean sun is allowed to revolve around the earth. This last point, since there is no written evidence, is pure speculation."

process in which Copernicus first arrived at a geoheliocentric or "Tychonic" phase where the Earth is at rest and the planets revolve around the Sun as the Sun revolves around the Earth (Swerdlow 1973, pp. 471–478). But in such a layout the sphere carrying Mars intersected that of the moving Sun. This physically unacceptable interpenetration would have faced Copernicus with a dilemma: either to give up the spheres, allow for intersecting paths and keep the earth at rest, as Tycho Brahe did much later, or to retain the spheres but remove the intersection by putting the Sun at rest and making the Earth a planet. Swerdlow believes that Copernicus did not follow this "Tychonic" path *avant la lettre* but instead paid the price of moving the earth in order to retain the solid spheres. "I can cite no evidence for this conjecture," he wrote, "but the reasoning does seem in keeping with Copernicus's meticulous insistence on constructing planetary motions out of the rotation of spheres. If correct, it is indeed ironic that the most admittedly medieval aspect of Copernicus's theory would be partially responsible for his most radical assertion that the earth moves about the sun" (Swerdlow 1973, p. 477).

As a reviewer of *The Copernican Question,* Swerdlow conceals his continuing investment in these youthful ironies and speculations which, by now, have hardened into dogmas. He has not come prepared to discuss where his own thinking has moved, how it relates to the book under review or how he would answer objections to his earlier speculations, such as the possibility that Copernicus, like Tycho Brahe, could have kept the Earth at rest and eliminated the problem of intersecting spheres by opting for permeable, *fluid* heavens (CQ, p. 84a; Grant 1994, p. 346). Or, if Copernicus did allow a sphere carrying the Earth, how would he explain how such an inalterable, perfect and solid body could contain and carry an earthy mudball? (Goddu 2010, pp. 353–354; 377). Indeed, the only documentary evidence to which Swerdlow could point in support of his Tychonic reconstruction was a single sheet of calculations in Copernicus's hand. Forty years later, his dating of that document—somewhere between 1500 and 1532—remains as uncertain as ever.[5]

Nonetheless, for some time, Swerdlow's proposal proved fertile. Some very good scholarship about the history of celestial spheres engaged with his work.[6] Moreover, at least until 2002, many specialists regarded Swerdlow's reconstruction to be the best available proposal. But, in that year, Bernard Goldstein advanced a short, simple, elegant reconstruction

5. Swerdlow, 1973, p. 428: "Since 15r was written after 1532, and 16v could have been written in 1500, it is certainly difficult to determine when these notes on 15v were written."

6. For example: Aiton 1981; Jardine 1982; Rosen 1985; Barker and Goldstein 1995; Lerner 1996–1997; Barker 2009; Goddu 2010, pp. 370–80.

wherein he proposed that Copernicus's originating question lay in the problem of planetary distances and order rather than in physical objections to the equant. For Goldstein, the problem of planetary distances and order crucially *preceded* the problem of planetary modeling and the associated principle of uniform, circular motion. In this regard, Goldstein made three powerful and salient points. First, "We now know that Muslim astronomers, beginning in the thirteenth century, were able to produce many models that resolved the problem [of the equant] while maintaining a geocentric framework. In other words, the equant was an astronomical problem whose solution did not impinge on cosmological issues" (Goldstein 2002, p. 220). Second, "To construct a heliocentric system in any detail, Copernicus needed to transform Ptolemy's geocentric models (modified to resolve the equant problem) to heliocentric models. But, in my view, this was done only *after* he made an initial commitment to a heliocentric system" (Goldstein 2002, p. 221). And third, "there is no evidence that Copernicus was concerned with this intersection of orbs, and I think it unnecessary to ascribe such a view to him. In any event, a Tychonic system would not satisfy the distance-period relationship, for the ordering of the planets (including the Sun) in it is not dependent on the order of their periods" (Goldstein 2002, p. 222).

Goldstein's singular contribution marks only the first in a string of major omissions by Swerdlow and Heilbron: reference to his article fails to appear once in their reviews. Yet, surely the reviewers are aware of Goldstein's reconstruction, not least because chapter 1 of my book devotes several pages to it (CQ, pp. 57–61).[7] Instead, Swerdlow concocts a fabulist historiography that conflates my stance with that of Edward Rosen, camouflages the genuine disagreement between himself and Goldstein and utterly fails to engage other recent reconstructions, including André Goddu's major study, *Copernicus and the Aristotelian Tradition* (Swerdlow 2012a, pp. 353–354; Goddu 2010, pp. 243–261) and important articles by Michael H. Shank and Martin Clutton-Brock (Shank 2009; Clutton-Brock 2005).

I favor Goldstein's account because it convincingly avoids the unnecessary technical complexity, the insecure evidentiary dating and the ignored physical difficulties of Swerdlow's treatment. It is also consistent with Copernicus's own emphasis on seeking a single, unifying principle resting on the relationship between periods of revolution and distances—not to men-

7. "[A]side from reproducing some figures from publications of *others* on the relation of geocentric and heliocentric models. . . ." (Swerdlow 2012a, p. 366; my italics). The unnamed "others" are, respectively, Curtis Wilson (CQ, fig. 17) and Bernard Goldstein (CQ, fig. 18).

tion the title Copernicus chose for his book: "On the Revolutions." If Copernicus's working out of the details of the individual planetary models involved some considerable mathematical ingenuity, his fundamental insight involved no calculations other than for the periods of revolution of Venus and Mercury.

But while Goldstein's account finally puts us on the right track, it does not, in my view, provide a strong enough explanation for Copernicus's willingness to overturn the entire physical foundation of astronomy. In fact, Copernicus had a safer route open to him: he could have avoided potential conflicts with theologians and Aristotelian natural philosophers had he presented his new account merely as a hypothesis, useful for planetary table-making and astrological prediction but with no correspondence to reality. That hypothetical view is, of course, precisely the position Andreas Osiander attributed to him in the famous anonymous "Letter to the Reader" he inserted without the author's permission into the introductory apparatus of *De revolutionibus.* It is even conceivable that at one point Copernicus entertained such a path, but it is not the position he took in any of his extant writings.

The Context of Copernicus's Early Motivation

Copernicus learned theoretical astronomy at Krakow where both John of Glogovia and Albert of Brudzewo were involved in making astrological forecasts and Brudzewo's commentary on Georg Peurbach's *Novae theoricae planetarum (New Theorics of the Planets)* was the primary text for teaching the elements of planetary theory. Although it is not known for certain that Brudzewo himself taught Copernicus, it is fair to assume that Copernicus was taught by one of Brudzewo's many students using the master's text.[8] Either way, if Copernicus knew anything about planetary theory in Krakow, he would have been introduced to Peurbach accompanied by Brudzewo's commentary, the first such gloss on Peurbach to appear since the original text in Nuremberg some twenty years earlier.

Although Peurbach's *Novae theoricae planetarum* of ca. 1472 contained nothing about the order of the planets (the planets were treated separately), Brudzewo's commentary on Peurbach did contain such a treatment and, most importantly, he explicitly connected that discussion to the ele-

8. Goddu 2010, pp. 31–38, 162–167; Birkenmajer 1972, pp. 488–91; Jardine 1982, pp. 189–190. Textbook commentaries were often crafted specifically for local university constituencies and Brudzewo's was the first such commentary on Peurbach. Hence, there were no immediate competitors and I consider it highly probable that Copernicus was exposed to it in Krakow. Moreover, after his arrival in Italy, already aware of Brudzewo's reputation, he could have encountered the published version in the book markets of Bologna, Milan or Venice.

mental qualities associated with the planets. I paraphrased this discussion in my book as follows: "The Earth was the locus where the primary physical qualities—heat, cold, moisture and dryness—were in a constant state of mingling and recombination. Saturn, the planetary sphere most distant from the Earth, was cold and dry, and the slowest of planets; it was associated with the least mixing of qualities. At the other end of the heavens, the Moon was closest to the sphere of mixing, so that it was reasonable that it shared in the Earth's moisture and returned the favor by causing the motion of the tides. Jupiter, after Saturn the next speediest, was hot and moist, but Brudzewo differentiated its moisture from that of the Earth by calling it a spiritual quality, the 'carrier of life virtue.' Jupiter's sphere could not initiate the mixing of matter, but it could influence matter that was already moved and mixed. Mars, next after Jupiter, was "moderately distant"; like the Sun, to which it was adjacent, it was hot and dry, but because of its distance it could not burn like the Sun. The Sun, on the other hand, had a type of heat and dryness that Mars did not possess. Located 'in the middle of the planets, like a heart,' the Sun's heat 'distributes and ripens the seed that gives life.' Next came Venus. Because it was the Sun's neighbor, it too could 'give life,' but it was also moist and hence capable of combining with the Sun. Finally, although all planets were capable of mixing, only Mercury had the virtue of mixing with both of its adjacent neighbors; it derived this capability from its place between the cold, moist Moon and hot, moist Venus."[9]

9. In mixtione autem est frigidum cum sicco et frigidum cum humido et calidum cum sicco et calidum cum humido. Est autem frigidum cum sicco sic, quod neutra qualitatum est vitae, sed utraque mortificativa; tamen in mixtura operatur potentia bene tenendi, sed male recipiendi et in hoc impedit mixturam. Oportuit ergo sphaeram per motum hoc operantem longissime poni a loco mixtionis et tamen oportuit ipsam esse propter potentiam bene retinendi. Sed quia hoc habet perficere sphaera Saturni, ideo elongata est maxime a loco mixtionis et suprema facta ac tardissimi motus, quia aliter mixtura omnis solveretur. Frigidum atque cum humido est duplex: est humidum simplex, quod est elementale, et est humidum complexionale quod est subiectum vitae et id quidem, quod est simplex, oportet habere fortem motum, miscibilibus ingeratur. Et ideo propter illud est sphaera Lunae vicinissima loco mixtionis, ut fortius moveat, propter quod etiam fluxus et refluxus maris sequitur motum Lunae. Humidum autem complexionale habet movere sphaera Veneris et ideo Solis coniuncta est, qui est dator vitae.

Calidum autem cum humido est, et calidum cum sicco. Sed humidum cum calido esse non potest, nisi sit humidum spirituale, ex quo fiunt spiritus, qui sunt vectores virtutum vitae, et ideo non potest esse excellens calidum, quia tale est nisi cum sicco. Et ideo calidum cum humido est complexionale calidum et spirituale humidum. Et hoc movet sphaera Jovis, propter quod altius post Saturnum est locata, quia ex temperamento sui non potest movere materiam mixtionis, sed motae iam et mixtae in se influere potest.

Calidum cum sicco dupliciter est. Aut enim est motivum totius materiae, aut digestivum et maturativum materiae iam motae. Et illud quidem, quod est motivum totius

Notice that in this account Brudzewo presents a *fixed* planetary order. In this regard, he was simply following Ptolemy's *Tetrabiblos* I.4, the authoritative account of astrology's principles and categories. He explains the planets' associated elemental qualities strictly in relation to that order. There are no announced uncertainties about the arrangement, no parallactic claims, no allusions to the differing opinions of earlier authorities on the order of Mercury and Venus such as Ptolemy presents in the *Almagest,* IX.1. In fact, the order *must* be fixed because as the planets rise and set and move around the zodiac, they *carry with them* the qualities that allow the planets to form combinations with one other and with different signs. In turn, these qualities serve as the basis for generating still further qualities. Furthermore, because no published version of Ptolemy's *Almagest* existed when Copernicus was at Krakow, it is even possible that he was unaware of any disagreement about the order of Venus and Mercury until after he arrived in Bologna in 1496 and became acquainted with Regiomontanus's *Epitome of the Almagest.*

Swerdlow makes not a single reference to Brudzewo, either in his 25-page review or his 23-page translation supplement. His screen goes blank. He refers vaguely to Peurbach's *Theoricae novae planetarum* as "a standard text that Copernicus must have known in one of several printings before the end of the fifteenth century" (Swerdlow 2012a, p. 366). Before the end of the fifteenth century? How could Copernicus fail to be acquainted with Peurbach at Krakow when Swerdlow matter-of-factly ascribes to Copernicus the highest competence in the major available texts of practical astronomy and theoretical astrology, both of which domains presupposed grounding in astronomical theory? "While a student at Cracow," Swerdlow reports, "he acquired copies of the *Alfonsine Tables,* for computation of the sun, moon, planets, and eclipses, Regiomontanus's *Tabulae directionum,* for spherical astronomy, both used nearly exclusively for astrology, and Ali ibn Abi r-Rijal's *In iudiciis astrorum,* the most comprehensive astrological treatise translated from Arabic. So it appears that he was interested and competent in these subjects and may also have attended lectures on astronomy and astrology, which were given regularly. Professor Westman takes note of this. . . ." (Swerdlow 2012a, p. 367).

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materiae, est frenans et vincens calidum; quod movet sphaera Martis, propter quod elongata est moderate ut attingere possit et non incendat: et ideo habet locum tertium. Siccum autem cum calido digestivo et maturativo seminum et conceptuum est movens sphaera Solis, propter quod in medio planetarum est posita, sicut cor, et sibi attribuitur dare vitam.

Cum isti sex planetae sic movere habeant principia mixtionis, unus solus est, qui est commiscibilem et applicabilem habet virtutem: qui est Mercurius. Et ideo habet motus involutos et ponitur inter duos planetas, qui movere habent frigidum et calidum, quod maxime est commiscibile: et isti sunt Luna et Venus (Brudzewo 1900, pp. 11–13).

For all the bluster and hand waving, Swerdlow concedes that Copernicus's astrological interests did indeed enjoy early traction at Krakow. But, again, he fails to include in this concession and the accompanying translation of a selection from Peurbach the obvious importance of the commentary that Copernicus used as a student. One reason for the omission is that he has used the 1474 and 1553 editions of Peurbach rather than Brudzewo's treatise, published in Venice (1494) and again at Milan (see Brudzewo 1495). Peurbach refers to certain motions that all the planets share with the sun—the Earth's motion, as we would now understand it, projected onto the planets' motions (a component of one solar year). Swerdlow pauses from his translation of this discussion in Peurbach only long enough to deliver a scolding: "If indeed Copernicus was concerned about the geocentric order of the planets, how would this lead him to the heliocentric theory? Professor Westman has little to say about this aside from quoting some snippets concerning the relation of the motions of the planets to the motion of the sun (CQ, p. 50), which he calls "shared motions . . . But he quotes the wrong snippets, missing the essential parts, and, aside from reproducing some figures from publications of others on the relation of geocentric and heliocentric models [CQ, pp. 59–60], never explains the relations to the motion of the sun, or the relation of geocentric and heliocentric models, or how Copernicus could find these relations let alone derive the heliocentric theory" (Swerdlow 2012a, pp. 365–366).

Swerdlow's frequent complaints about "snippets" and "missing essential parts" are but two of many gratuitous criticisms with no specific referents, repetitive tics that serve only to create unnecessary entanglements and confusion.[10] Thus, to disentangle. First, emphasis on the unexplained involvement of the sun in the motions of the planets did not "lead" Copernicus directly to the heliocentric theory—nor, *pace* Swerdlow, does *The Copernican Question* make any such claim. Second, the diagrams Swerdlow provides are simply more fine-grained—and, hence, more detailed—versions of the transformations of geocentric into heliocentric models already shown in *The Copernican Question,* the latter reproduced from the article by Bernard Goldstein whose existence Swerdlow and Heilbron do not acknowledge (CQ, figure 18; cf. Swerdlow 2012b, p. 12). My footnote also refers readers to Dennis Duke's brilliant computer animations which make Swerdlow's unsociable reconstructions friendly and accessible while clearly showing the annual solar component and its geo-/heliocentric transformations (CQ, p. 522n152, 523n189). Third, Swerdlow's diagrams show us

10. The term occurs at least thirteen times in his review. Another is the fantasy that I do not do my own translations and "have had" various passages translated (Swerdlow 2012a, pp. 371, 376).

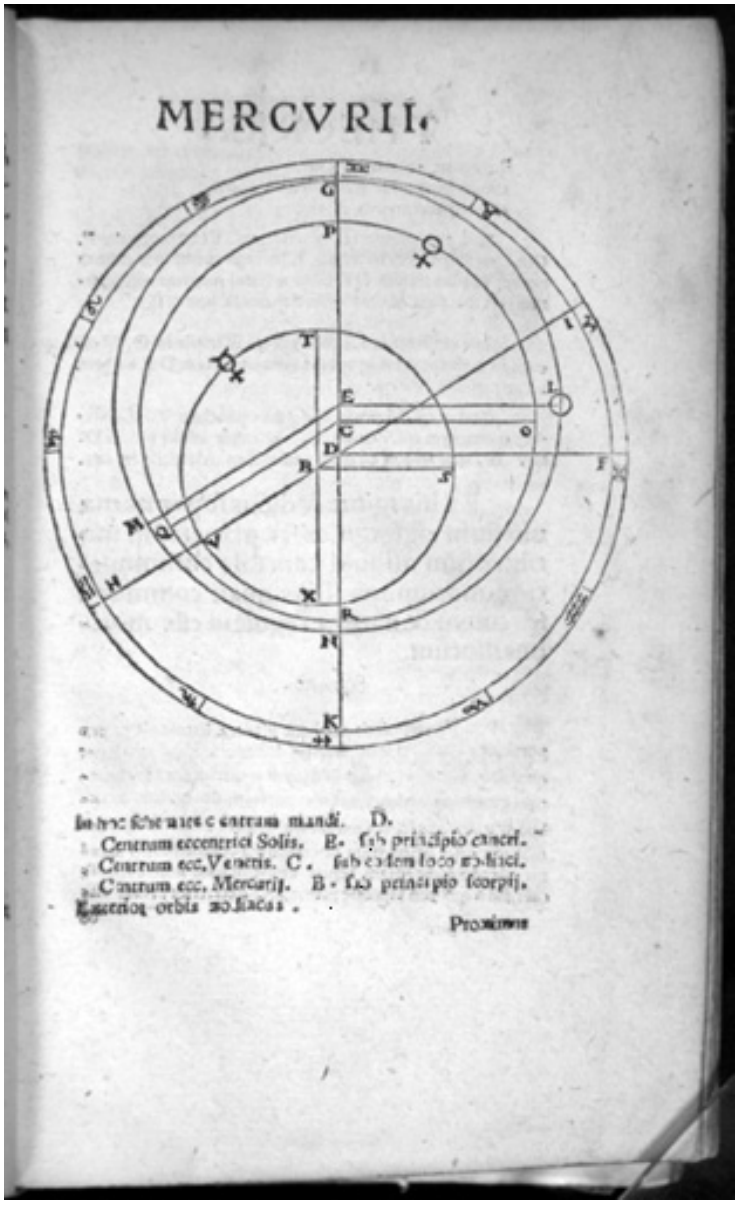

Figure 2. Erasmus Reinhold's interpretation of Peurbach's "shared motions" passage, showing ordering of eccentric circles with centers *B* (Mercury), *C* (Venus), *E* (Sun) and center of the world *D.* Mercury and Venus share the Sun's annual motion or solar year (Reinhold 1542). Image courtesy History of Science Collections, University of Oklahoma Libraries.

how *he* interprets Peurbach but they do not show us how *sixteenth-century contemporaries* made sense of the shared-motions passage (Swerdlow 2012b, p. 12). Only one such sixteenth-century commentator visually represented that passage for his readers—and that was the Wittenberg astronomer-astrologer, Erasmus Reinhold (1511–53). Reinhold's diagram clearly shows that Peurbach's discussion of the sun's mean motion did not automatically "lead to" a heliocentric framework because Mercury and Venus retain the customary Ptolemaic ordering and the image shows how a contemporary visually interpreted the passage's meaning. Swerdlow has no comment on Reinhold's diagram (see CQ, p. 53, fig. 13; Fay and Jardine 2012; Pantin 2012).

Finally, once again Swerdlow avoids reference to Brudzewo's astrological commentary on this passage, although it is important: "The Master adds a corollary such that all the planets have some communication with the Sun's motion in their own motions. Now this is because they have a natural connection, as with a luminous body, just as Ptolemy says in Book One of the *Tetrabiblos;* and therefore they [the planets] participate in its motion, influence and operation" (CQ, p. 61).

What should be made of Brudzewo's gloss? Here is the explanation I give in *The Copernican Question:* "The Brudzewo corollary does not, *of course,* compel the Sun to be placed at rest at the center of the universe; but it adds an astrological inflection to Peurbach's otherwise-buried shared-motions proposition, namely, that in addition to the Sun's annual motion, the planets also share something of the Sun's power as a source of heat and astrological influence. And precisely here is a place where Copernicus *could have* made the 'Pythagorean move,' concluding that the source of heat, luminosity, and influence for all the planets should be at the center and at rest" (CQ, p. 61; my italics) This statement is perfectly unambiguous: Brudzewo's commentary *could have* focused Copernicus's attention on the Sun both as an unexplained astronomical presence *and* as an astrological force but without determining a unique solution to the planets' shared motions with the Sun. It is also a place where Copernicus *might* have begun to think about the Pythagorean opinion, as reported by Aristotle in *De caelo:* "They [the Pythagoreans] affirm[ed] that the center is occupied by fire, and that the earth is one of the stars, and creates night and day as it travels in a circle about the center" (CQ, p. 61).

So, What About the Pythagoreans?

Neither Swerdlow nor Heilbron have anything to say about the Pythagoreans or the fact that the heliocentric arrangement was customarily referred to in the sixteenth and seventeenth centuries as "the Pythagorean

opinion."[11] Important references to the Pythagoreans occur in all the major writings that constitute the Copernican corpus—the unpublished *Commentariolus* (ca. 1510–1514), Rheticus's *Narratio Prima* (1540, 1541) and *De revolutionibus* (1543) (CQ, pp. 1, 33, 56, 486, 523n180, 533n177). Copernicus, like any student who had been taught Aristotle's *De caelo,* already learned Aristotle's representation of the Pythagoreans at Krakow. In fact, Aristotle sets up the Pythagoreans in order to show that the consequence of their position is physically absurd: If heavy bodies fall to the center of the universe and that center is the Central Fire, then heavy bodies must fall "up" because it is the nature of Fire to go up; but heavy bodies fall "down" to the center; hence, to affirm the Pythagoreans' account was to affirm an absurdity. To master Aristotle's argument, therefore, meant rejecting the Pythagoreans, a routine step by which students learned to become Aristotelians in astronomy and physics. In the *Commentariolus,* Copernicus is also explicitly critical of the Pythagoreans—not because he rejects their position that the earth revolves but because he rejects their justification for it: "Lest anybody suppose that, with the Pythagoreans, we have asserted the Earth's motion rashly, he will find here strong evidence in [our] explanation of the circles. For the arguments by which natural philosophers try above all to establish the Earth's immobility rest for the most part on appearances. But all their arguments are the first to collapse here, since we overturn the Earth's immobility also by means of an appearance" (CQ, pp. 101; 531n151).

In his revealing 1973 commentary on this important passage, Swerdlow dismissed the relevance of Copernicus's allusion: "Copernicus's insulting reference to the Pythagoreans is amusing and informative. Far from being in any way influenced by these old sages, Copernicus, who arrived at the theory of the earth's motion through his own analysis, accuses them, justly I think, of not knowing what they are talking about . . . One has only to remember the technicalities of planetary theory to understand that the venerable old sages had nothing to do with Copernicus's work" (Swerdlow 1973, p. 439). Neither Swerdlow's thinking nor his rhetoric appear to have changed since 1973. But, of course, the fact remains that Copernicus presents *De revolutionibus* as an extended encounter with venerable old sages, like Ptolemy, Aristotle, Plato, Martianus Capella, Cicero, the Pythagoreans and others. Indeed, the experience of reading the an-

11. Indeed, Galileo makes Salviati say of Copernicus: "Finding that some of the Pythagoreans had in particular attributed the diurnal rotation to the earth, and others the annual revolution as well, he began to examine under these two new suppositions the appearances and peculiarities of the planetary motions . . . And seeing that the whole then corresponded to its parts with wonderful simplicity, he embraced this new arrangement. . . ." (Galilei 1967, p. 341.)

cients in the Renaissance, as Anthony Grafton has aptly reminded us, was "a conversation, not a monologue": "Ancient texts shaped modern writers' attitudes even as the moderns wrote new works that reinterpreted the ancients" (Grafton 1997, pp. 132–33; Goddu 2010, 254–55). Such an interactive image of Renaissance humanism well captures Copernicus's practice.

Unlike this engagement with the ancients, Copernicus was unforthcoming about the ideas and controversies that prevailed in his formative years. And this glaring lacuna is the source of much historiographic conjecture and disagreement. In my reconstruction, Copernicus grasped (perhaps suddenly) that a new version of the Pythagorean arrangement—*taking the Earth's diurnal and annual motions as mathematical assumptions, in the style of Ptolemy*—could become the central premise of a celestial order that would solve both the problem of the Sun's apparent presence in the motions of the other planets and the uncertain ordering of Mercury and Venus. He would have encountered the first of these problems at Krakow, the second in Bologna. When I say "solve," I mean here "explain" and also "unify". But not all lovely, unifying explanations are true and that could explain why Copernicus delayed publication for so long and at some point turned to probable/dialectical arguments rather than to a strict, apodictic demonstration that ruled out all alternatives.

The Political Context of Pico della Mirandola's Polemic against Astrology

To attack astrology and its practitioners was to attack the entire web of social and political arrangements of which they were a part, including the rulers who retained astrologers and the universities which supported the teaching of the science of the stars and made the issuance of annual astrological prognostications an obligation of the resident astronomer-astrologers (cf. Grafton 1997, p. 134). Just a few months before Copernicus's arrival in Bologna in 1496, Giovanni Pico della Mirandola's *Disputations against Divinatory Astrology* appeared from a major Bologna publisher and bookseller, Benedictus Hectoris (Ital., Benedetto Ettore Faelli). Pico came from a prominent aristocratic family, he was widely recognized for his extraordinary intellect and maintained important political ties with Bologna's ruling families, including its leading family, the Bentivoglios, as well as a key member of the city's oligarchy, Mino Rossi. Copernicus's landlord, the prognosticator Domenico Maria Novara (1454–1504), prominently mentioned Rossi in two of his annual prognostications (1501, 1502). Meanwhile, in Florence, Pico's close friend, the Dominican friar Girolamo Savonarola (1452–98) preached a powerful doctrine of Christian renewal while denouncing clerical corruption. When the French invaded Florence and expelled the ruling Medici in 1494, he became

leader of a popular republic, defied the pope (for which he was excommunicated) and led a campaign that included attacks on greedy astrologers while affirming the superiority of religious prophecy. Savonarola's denunciation of astrology, composed in the vernacular, was based on Giovanni Pico's massive and devastating treatise of the preceding year and included rejection of the zodiac as nothing but a human construction. In 1498, Savonarola and two of his followers were hung and then burned at the stake in the central piazza of Florence. In 1512, aided by the papacy, the Medici returned to power—with their astrologers.

Did Copernicus Read Pico and, if so, How?

Like Swerdlow, Heilbron ignores and thus depoliticizes the context of Pico's critique.[12] Throughout his review, Swerdlow makes fun of contextual considerations because they involve minor figures whose work did not rise to the level of memorable originality and whose names are worthy, at best, of parody. He treats all such matters as something like air conditioning—white background noise. But in one of his several surprising concessions, he admits that Copernicus was familiar with the *Disputations.*[13] What agitates him and Heilbron most is that there might be some kind of connection between Pico's attack on astrology and Copernicus's adoption of a heliocentric arrangement. Neither has considered the matter with sufficient care.

Could Copernicus have remained indifferent to Pico's critique while living with Novara and assisting him with his prognostications? Swerdlow registers his own impression of the *Disputations* in no uncertain terms: "from the parts I have read he [Pico] seems to regard the entire subject [of astrology] as perverse rubbish and is out to annihilate it in any and every way possible. Although much of the work is dry and scholastic, parts are lively and entertaining, even amusing because so caustic and astrology made to look so silly. Pico shows a good knowledge of earlier and contemporary astronomy and astrology, in Greek, Latin, and Hebrew, although one must be careful to distinguish between where he is writing an account of authentic astrol-

12. Cf. Heilbron's dessicated summary: "Because the Copernican corpus does not mention astrology, Westman has to argue from circumstantial evidence that a desire to reform the art drove heliocentrism. This evidence, as he has constructed it, consists primarily of the activities of astrologers whom Copernicus probably knew or knew of in Bologna around 1500" (Heilbron 2012, p. 380).

13. "Copernicus's own treatment of the geocentric order of the planets, in *De revolutionibus* I.10, *drawn from Pico and Regiomontanus,* is readily available for comparison with the texts translated here, from which his debt to both is obvious" (Swerdlow 2012a, p. 364; my italics).

ogy and where he is making up a parody of what he considers its stupidities and the ignorance of astrologers. He certainly read more of the subject, and knew it better, than anyone today" (Swerdlow 2012a, p. 358). Based on Swerdlow's own characterization, it seems reasonable to assume that Copernicus could hardly have remained indifferent to Pico while living with Novara. The very activity in which Copernicus was somehow involved as "not so much the pupil as the assistant and witness of the learned Dominicus Maria" (in the words of his disciple, Rheticus), was built on premises that Pico attacked.[14] And, here might be a good place to remind Swerdlow of a "snippet" of evidence that he has conveniently omitted—Rheticus' statement in the *Narratio Prima:* "If my teacher's account of the celestial phenomena had existed a little before our time, Pico would have had no opportunity, in his eighth and ninth books, of impugning not merely astrology but also astronomy."[15] In light of Rheticus's comment, written while living with Copernicus and in a book whose contents Swerdlow agrees Copernicus must have approved, we may sharpen our question even further[16]: *How might Copernicus have read those parts of Pico that "impugn{ed} not merely astrology but also astronomy"?*

As it happens, there is a chapter in Pico's *Disputations* (X.4) that manifestly assails both astrology and astronomy; and, furthermore, it can be said *with certainty* that Copernicus had read this chapter because in *De revolutionibus* he refers to evidence that could have come from nowhere else but this section of the *Disputations* (without naming Pico).[17] And further still, Copernicus's reference occurs in just that critical part of *De revolutionibus* that lays out his grand demonstration of the new order of planets (I.10) and, in particular, his proposed solution to the hitherto uncertain ordering of Venus and Mercury. Yet Swerdlow brushes aside the relevance of Pico's discussion either to the order of the planets or to the qualities of the elements associated with the planets or to the connection between the two.

A closer look reveals that my discussion in *The Copernican Question* and Swerdlow's translation of Pico's chapter are in no way at odds (CQ,

14. "Cum D. Doctor praeceptor meus Bononiae non tam discipulus quam adiutor et testis observationum doctissimi viri Dominici Mariae . . ." (Rheticus 1982, p. 43).

15. "Quod si talis paulo ante nostram aetatem rerum coelestium doctrina extitisset, nullam Picus in octavo et nono libro occasionem, non solum astrologiam, sed et astronomiam impugnandi habuisset" (Rheticus 1982, pp. 49–50; CQ, p. 103). That Rheticus wrote "eighth and ninth" rather than "ninth and tenth books" probably means that in 1540 neither he nor Copernicus had available to them a copy of Pico's *Disputations.*

16. "Since Rheticus wrote the *Narratio Prima* while visiting Copernicus, his 'teacher,' as he always refers to him, it is surely unkind to think he included subjects to which Copernicus was not favorable and withheld his account from his teacher's inspection and approval" (Swerdlow 2012a, p. 369).

17. For the full argument, see CQ, p. 104.

pp. 86–87, 99, 105; Swerdlow, 2012b, pp. 3–7). Two arguments are of special relevance to Copernicus' reading of Pico's chapter. The first is Pico's claim that astrologers' associations of elements (Earth [cold and dry], Water [cold and moist], Air [hot and moist], Fire [hot and dry]) with planets is *arbitrary.* Swerdlow thinks that Pico's objection is irrelevant to the question of planetary ordering and the associated elements because he regards Pico as *just* engaging in sarcastic parody.[18] But he does not seem to realize that, whatever the *rhetorical* tone, the *logic* of the criticism does not change. Pico asks, should not the order of the planets follow the order of the elements? In that case, if Fire is the first of the elements then it should be associated with Saturn, the first in the order of the planets. But the astrologers consider Saturn to be connected to the element Earth. And, Pico goes on to say that the situation would not be improved by making the Moon first because "the place and order of the intermediate planets is entirely uncertain" (CQ, p. 86). So, for Pico, there is neither necessity in these associations nor certainty about the order of the planets intermediate between the extremes, that is, between the Moon and Saturn.

At this point Albert of Brudzewo's commentary is apposite. It is obvious that Pico's argument *contradicted* the fixed ordering of the elemental qualities that Copernicus had learned from Brudzewo's commentary and that he and all other celestial practitioners knew, as well, from Ptolemy's *Tetrabiblos* (a copy of which Novara owned during the period in which Copernicus lived with him [CQ, pp. 96–97]). Both Brudzewo and Ptolemy *explain* why the individual planets have their specific causal attributes, based on their radial distance from the Sun, Moon and Earth or, in the case of the intermediate planets, on qualities *shared* with their neighbors. To have such a communal sharing of qualities, however, means that the intermediate planets (Mercury, Venus, Mars, Jupiter) must "know", as it were, who their neighbors are. But how can that be? By omitting Brudzewo and misrepresenting Ptolemy's discussion in *Tetrabiblos* I.4, Swerdlow claims erroneously that Ptolemy is using distances from the Sun, Earth and Moon as a single, consistent rule in the assignments of elemental qualities; but, to reiterate, he fails to appreciate that these explanations already *presuppose*

18. "Pico is writing a parody of an association of planets with elements, fire and water, to illustrate his point that 'the assumption is exceedingly worthless and senseless, that the first in one kind also correspond to the first of another kind by an affinity of nature' . . . Professor Westman . . . does not recognize Pico's sarcasm in the *Disputations,* does not recognize deliberate nonsense, and actually takes seriously the association of Saturn and Mars with fire and water . . . Never mind for the moment that *Pico says nothing of 'elemental qualities,'* only of elements, which are not the same thing, and that in a parody of what he considers a ridiculous assumption, is this so?" (Swerdlow 2012a, p. 359).

a fixed ordering.[19] How else could Copernicus prove that the linking of elemental qualities with planets was not arbitrary other than by a simple appeal to Ptolemy's authority—or, perhaps by something different and more radical?

A second argument in Pico's chapter must have disturbed Copernicus as well: the uncertain order of Venus and Mercury. If Copernicus had no familiarity with Ptolemy's *Almagest* while in Krakow (it was not yet published), then he would have been unaware of the inconsistency between that work and the *Tetrabiblos.* But once in Bologna, Copernicus surely would have recognized the *incompatibility* between Ptolemy's two works as soon as he was able to study Regiomontanus's *Epitome of the Almagest* where Regiomontanus describes the order of Mercury and Venus as a *controversia* (Swerdlow 2012b, pp. 9–11).[20] Pico emphatically aggravated that uncertainty in a manner that was unprecedented, playing up disagreements about the order of Venus and Mercury among ancient, Arabic and Jewish authorities in the service of undermining the foundations of astrology. The serious *implication* of Pico's critique in X.4 was that if the planetary order in the *Almagest* was uncertain, then the fixed planetary order in the *Tetrabiblos* would be weakened and eroded (CQ, p. 87). Hence, as I write: "Pico's questioning of Ptolemy's ordering of Mercury and Venus was itself not unprecedented—as we have seen, it was already in the *Almagest*—but the context was strikingly new. Now for the first time, an uncertainty about planetary order was situated in the context of the assignment of qualities and powers to the individual planets. As a consequence, an uncertainty about the order would put the whole scheme of astrological influences at risk—including what young Copernicus had learned just recently from Albert of Brudzewo's commentary on Peurbach's *New Theorics of the Planets*" (CQ, p. 57, figure 15).

To be clear: By "context" I mean not just the chapter in which Pico's discussion occurs but its positioning in a book that was "out to annihilate it [astrology] in any and every way possible" as well as the fraught and

19. "Planets closer to the sun heat and closer to the earth and moon moisten, planets farther from the sun cool and farther from the earth and moon dry, although somehow distant, beneficent Jupiter 'at the same time heats and moistens *(insimul calefacit et humectat).*' That is all a commonplace of elementary astrology. . . . Actually, the relation Ptolemy describes, planet by planet, is *not so much of order as of distance,* from the sun and from the earth and moon" (Swerdlow 2012a, pp. 359–60; my italics). The problem is with the intermediate planets. Compare my discussion (CQ, pp. 52b–53a).

20. Swerdlow translates "controversia" as "disagreement," which is slightly weaker than other alternatives offered by Robert Ainsworth's dictionary (controversy, dispute, debate, variance, quarrel; Ainsworth 1736).

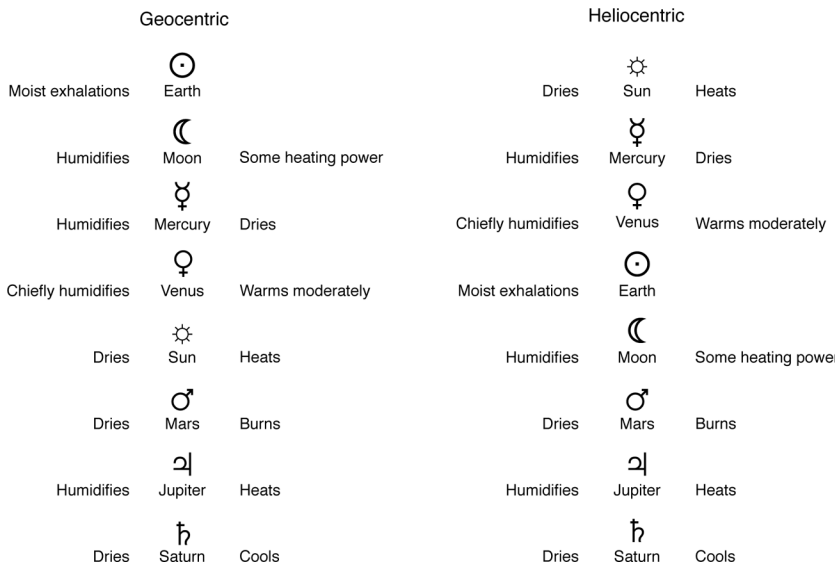

Figure 3. Ordering of Astral-Elemental Qualities according to Ptolemy's *Tetrabiblos,* book I, chapter 4 (left); reshuffled order of the same qualities and associated planets showing their positions in a Copernican arrangement (right).

anxious political setting of the besieged late fifteenth-century Italian principalities in which the book appeared. I also include Copernicus's Krakovian education in astrological and astronomical theory and his apprenticeship at Bologna in astrological practice. Thus, in response to Pico's attack, I submit that Copernicus engaged in commerce with the ancients (the Pythagoreans and Martianus Capella) as well as with the moderns (Regiomontanus) and thence proposed to resolve both the uncertainties of planetary order and the assignment of the planets' elemental qualities.

Swerdlow's misrepresentation of Ptolemy's *Tetrabiblos* (I.4), his ignoring of Brudzewo's commentary on Peurbach, his dismissal of the Pythagoreans and his mischaracterization of my claim concerning Pico's critique then all converge in a crashing, grand finale: "Pico himself says nothing, not one word, about 'the order of the planets and the assignment of elemental qualities,' so there goes the one and only reason, indeed, Professor Westman names no other, for Copernicus, or anyone, to know or care anything about it. And no one did. That is the end of 'Copernicus's Problematic,' it isn't even wrong, it does not exist. And with it goes the astrological origin of the heliocentric theory" (Swerdlow 2012a, p. 360). The orchestra can pack up; the audience can go home; the janitors can sweep up.

But no. There is an encore. For the next several pages, he informs us that in book X, chapters 13 and 15 Pico *does* dispute the order of the planets and its consequences for astrology, although not for the elemental qualities but for the planets' associations with hours of the day and the week and he wonders why I do not cover these sections. He also calls attention to a work by al-Qabisi "translated from Arabic in the twelfth century and still a standard guide to astrology in the fifteenth," in which the order of the planets is connected to the number of months the child spends in the womb (Swerdlow 2012a, p. 361). This is all very interesting but, far from detracting from my claim about the importance of Pico's focus on planetary order and its astrological entailments, it adds further support for my account. *Indeed, if Pico was explicit about some astrological implications of planetary order, how could he overlook the elemental qualities on which all the other categories directly depended?*

To unpack this point a bit further: Ptolemy says that Jupiter, Venus and the Moon are beneficent "because of their tempered nature and because they abound in the hot and the moist." Meanwhile, Saturn and Mars are maleficent, "one because of his excessive cold and the other for his excessive dryness" (Ptolemy 1940, I.5, p. 39). The moon and Venus are feminine, "because they share more largely in the moist" whereas the sun, Saturn, Jupiter, and Mars are masculine because they are associated with dryness and Mercury, "common to both genders, inasmuch as he produces the dry and the moist alike" (Ptolemy 1940, I.6, p. 41). And then we have diurnal and nocturnal planets, corresponding to the two major time intervals, and for which Ptolemy informs, "the day is more masculine because of its heat and active force, and night more feminine because of its moisture and its gift of rest." Hence, he reports that "tradition" hands down the association of moon and Venus as nocturnal, the sun and Jupiter as diurnal and bi-gendered Mercury "diurnal when it is a morning star and nocturnal as an evening star" (Ptolemy 1940, I.7, p. 43). And what of the fixed stars? They derive their natures and related powers from "kinship" or "affinity"—that is, by *analogy*—with the planets they resemble. For example, "Of the stars in Cancer, the two in the eyes produce the same effect as Mercury, and, to a less degree, as Mars: those in the claws, the same as Saturn and Mercury" (Ptolemy 1940, I.9, p. 49). Likewise, with the signs of the zodiac as well as the houses, exaltations and depressions (Ptolemy 1940, I.11–19, pp. 65–91). And on and on. All these assignments—and many more—follow from the association of planetary ordering with elemental qualities that Ptolemy had necessarily established right at the start, in *Tetrabiblos,* I.4 (cf. Swerdlow 2012a, pp. 370–371).

To sum up, Copernicus read Pico's *Disputations* X.4 and we know that it figured significantly in his discussion of the order of the planets in *De*

revolutionibus I.10.[21] Moreover, in view of the gravity of the polemic, he must have read a great deal more. It is hard to imagine he did not discuss the *Disputations* with Novara, although on that point there is no direct evidence. But, *De revolutionibus* was a work of theoretical astronomy, not theoretical astrology. We do not know what other entailments Copernicus may have considered for theoretical astrology or which of Pico's other arguments he explored and whether, like Ptolemy, he intended to compose his own *Tetrabiblos* as a companion work. In the conclusion to chapter 3, I explicitly acknowledge these issues: "Copernicus might have believed that if astronomy's foundations were reformed as he envisioned, then that change alone would be sufficient to sustain the traditional astrology found in the *Tetrabiblos.* But following Pico's critique of the arbitrary association of elemental qualities and planetary order, it seems far more likely that Copernicus would have recognized that a radical revision of the prevailing celestial arrangement would require a corresponding reform of astrology's principles. Indeed, besides the reassignment of physical qualities made necessary by the planetary reordering, Pico's other objections would need to be answered in a manner superior to that of [Lucio] Bellanti—for example, the house-division problem and the uncertainty of the instruments and tables. We may well wonder whether he would have turned to Regiomontanus's astrology, as he did with his planetary theory. In any case, just as a circularly moving Earth was incoherent with Aristotle's theory of the elements, so too the physics and meteorology underlying traditional astrology would need to be rethought. Reformulating Ptolemy's *Almagest* was evidently more than enough for one man. Perhaps Copernicus believed that he could leave to the young and astrologically-skilled Rheticus the reconfiguring of the *Tetrabiblos* just as he had explicitly left debate about the world's infinitude to the philosophers" (CQ, p. 105).

Swerdlow's Unannounced Departure from Kuhn

In *The Copernican Revolution,* Kuhn rightly states that "astrology . . . provided the principal motive for wrestling with the problem of the planets." But he then reasoned incorrectly (and ahistorically) that astrology would "lose much plausibility if the earth is a planet" and from that argument he derived two, undocumented historical conclusions: "It cannot be a coin-

21. Swerdlow acknowledges that, "Since he [Copernicus] had read at least parts of Pico's disputation against astrology, he too could have considered it perverse nonsense and ignored it entirely. But neither did he write a word against it. Still, since everyone of his age concerned with astronomy was also concerned with astrology—I know of no exceptions—these arguments from silence do not carry much weight" (Swerdlow 2012a, p. 367).

cidence that astrology's stranglehold upon the human mind finally relaxed during just the period in which the Copernican theory first gained acceptance. It may even be significant that Copernicus, the author of the theory that ultimately deprived the heavens of special power, belonged to the minority group of Renaissance astronomers who did not cast horoscopes" (Kuhn 1957, p. 94). The first conclusion is false. The second is also false if Kuhn is understood to be making the following argument: If Copernicus believed the earth to be a planet, then the heavens have no special powers; ergo, astrology loses plausibility and casting horoscopes makes no sense. (It is still not known whether or not Copernicus cast nativities). In light of my study, Kuhn's argument raises the following questions: If Copernicus was an opponent of astrology, why did he not publicly align himself with Pico's critique? Why did Copernicus allow Rheticus to write that his teacher's work would have prevented Pico from "impugning not merely astrology but astronomy?"

Midway through his review, Swerdlow stealthily tacks away from Kuhn's position, being careful not to call undue attention to his departure.[22] Copernicus "must have been concerned with it [astrology], but it [the evidence] comes from a much later period and is lost. Nevertheless, there can be no doubt of its significance" (Swerdlow 2012a, p. 367). The evidence consists of a single letter referring to a lost almanac by Copernicus, which certainly does add something interesting to the discussion and I am glad to learn about it, not least because it adds further support to the general argument of *The Copernican Question.*[23]

The letter was written by Bernard Wapowski, secretary to the King of Poland, to a councilor at the Hapsburg Court in Vienna; it refers to a new and improved almanac prepared by a Canon of Warmia, Nicolaus Copernicus, described as "a great mathematician." Dated 15 October 1535, Wapowski's description includes a tantalizing reference: "that for the correction *(verificatione)* of the motions of the planets, it is necessary to grant some motion to the earth, an opinion he has held for many years, and he maintains that the earth moves insensibly" (Swerdlow 2012b, pp. 16–17). Swerdlow believes that the almanac is "the most direct evidence for Copernicus's *interest in astrology*" and that Copernicus "intended his work to be *useful for astrology,* the purpose of ephemerides with aspects, which perhaps would encourage interest in his new theory, as Wapowski's remark

22. I shall not repeat here Swerdlow's remarks, indelicate to say the least, which conceal his movement away from Kuhn and towards my position (Swerdlow 2012a, p. 377).

23. Michel-Pierre Lerner also called this document to my attention in his review (Lerner 2012, pp. 233–38; 237).

about the necessity of the motion of the earth indicates, although the heliocentric theory and the motion of the earth would not be evident from an ephemeris by itself" (Swerdlow 2012a, p. 368; my italics). So, here at last we have evidence that meets Swerdlow's high standard of direct evidence, at least with respect to "interest in astrology"—more direct, apparently, than living with the Bologna astrological prognosticator Novara and assisting him with his observations, more direct than Copernicus's initiation into the science of the stars at Krakow with Brudzewo's astrological commentary on Peurbach, more direct than owning an Arabic astrological work in his student days, more direct than having read and responded to Pico's *Disputations.* But does the letter about Copernicus's lost almanac permit something stronger? For example, that Copernicus used the earth's motion as an assumption in constructing his improved almanac? If he did, then there are two possibilities: either he took the motion as real and the order of the astral-elemental qualities would have to be revised, possibly in a separate work on theoretical astrology; or, he assumed the earth's motion merely for purposes of calculation. A decade later, at Wittenberg, Erasmus Reinhold found a third way: use Copernicus's planetary models to calculate a new set of planetary tables while holding the Earth at rest. These tables were widely used by astrologer-astronomers after their publication in 1551. Swerdlow explores none of these implications.

Rheticus's World-Historical Prophecy

There is another piece of evidence that supports Copernicus's "interest in astrology"—and also a connection between the earth's motion and apocalyptic prophecy. In the *Narratio Prima,* Rheticus introduced chapter 5 simply: "I shall add a prophecy [*vaticinium*]: That the Kingdoms of the World Change with the Motion of the Center of the Eccentric." The center around which the earth revolves itself moves very slowly in a small circle. The period of this small circle, which Rheticus called the Wheel of Fortune, "does not differ much from the saying of Elijah, who prophesied [*vaticinatus est*] under divine inspiration that the world would endure only six thousand years." Even if this scheme was Rheticus's contribution alone, Swerdlow is willing to acknowledge that, "there is no reason to believe it contrary to Copernicus's own opinion of astrological history" (Swerdlow 2012a, p. 369). But Swerdlow's term "astrological history" obscures the Elijah prophecy and its apocalyptic meanings, ignores its powerful political and religious significance, especially for Lutherans at Wittenberg where Copernicus's *De revolutionibus* became widely known, fails to note that Rheticus provided no diagram for sixteenth-century

readers and complains, mistakenly, that I provide no discussion of Copernicus's model (Swerdlow 2012a, p. 369).[24]

Between Copernicus and Kepler: Differential Responses to Pico across the Sixteenth Century

Throughout the sixteenth century, there were counterattacks against Pico. It is one of the book's major themes but disappears in the reviewers' fragmentation of the text. The resistance took shape especially at Wittenberg and in the orbit of other German universities influenced by it (chaps. 4–5). A key figure was the Lutheran reformer Philipp Melanchthon, rector at Wittenberg, author of numerous textbooks, a defender of all sorts of naturalistic divination, including interpretation of the meanings of dreams, monstrous births, comets and, unlike his friend Martin Luther, astrological prediction. Astrologers' predictions of a much-publicized flood of Noachic proportions in February 1524 did not turn Melanchthon into a Piconian skeptic when the event failed to occur. Instead, he created academic positions for astronomer-astrologers, like Reinhold and Rheticus. (CQ, p. 110–113; cf. Swerdlow 2012a, p. 355). New forms of opposition to Pico also developed at Louvain, Paris and London (chap. 6) and, significantly, on Tycho Brahe's famous island of Hven (chap. 8). All these different kinds of efforts to answer Pico retained the motionless Earth. On the other side, Pico's polemic found significant support in Rome with enforcement from the Holy Index of Prohibited Books, the Church's recently-minted bureaucratic mechanism of control over what could and could not be read. Sixtus V's papal bull of 1586, indebted to Pico's *Disputations,* drew a distinct boundary between safe and dangerous divinatory arts. (CQ, chap. 7)

At least one reason few followers of Copernicus failed to reply to Pico is that Copernicus left them a meager road map: he had failed to name Pico or to draw out the implications of planetary order for astrology. *De revolutionibus* was modeled after the *Almagest,* and there was no obvious place in that genre of writing for such a reference. Ptolemy had devoted an entirely separate book to astrology. The pope to whom Copernicus's book was dedicated was known for his intimate courtship of astrologers, but Copernicus might have been uncertain where Rome stood with respect to Pico's polemic. The latter consideration may help to explain why he allowed Rheticus to include the reference to Pico in the *Narratio Prima* but omitted direct mention in *De revolutionibus.* Furthermore, later knowledge of

24. Swerdlow does not explain how his diagram improves the interpretation of the passage over that of the French commentators (Swerdlow 2012b, pp. 18–21; Rheticus 1982, pp. 153–55; CQ, p. 118).

the local context in which Copernicus had formulated his ideas remained fragmentary and, hence, Rheticus's published asides provided the only basis for a narrative. Only a few important details from Copernicus's formative period enjoyed circulation in the Wittenberg orbit and through Tycho Brahe's communication network, among which were the exposure of Osiander's identity, the original title of *De revolutionibus,* and the manuscript of the *Commentariolus.* The Copernicans neither constituted a unified movement (chap. 16) nor did they devote systematic attention to the new theory's astrological entailments. Kepler's teacher Maestlin was extremely cautious about astrology and his position was close to Pico's skepticism (chap. 9). His pupil Kepler was the *only* Copernican to make a sustained effort to revise the principles of theoretical astrology (chap. 14).

In his astrological reform, Kepler followed Maestlin in accepting Pico's rejection of the reality of the zodiac as a human construction. But Kepler also found other uses for Pico. As a student at Tübingen, Kepler defended Copernicus before the physics candidates in 1593. In the surviving fragment of the disputation, we find Kepler proposing that the heliocentric sun is an active agent, an efficient cause that sends out a force that has the capacity to move the surrounding spheres. The presiding master at the disputation was Georg Liebler and in Liebler's textbook of natural philosophy he explicitly rejects Pico's idea that "the heavens have no particular force beyond the universal influence of motion and light." Liebler's denial of Pico's statement may have suggested to Kepler a solution to his quest for the cause(s) of the planets' motions in an affirmation of Liebler's denial: the sun is the source of both planetary motion and celestial effects in the terrestrial realm. (CQ, pp. 323–24) Swerdlow omits all mention of Liebler.[25]

Heilbron's Broodings

In light of this highly diverse, century-long pattern of response to Pico, worked out in detail over several chapters, it is puzzling to find Heilbron projecting into *The Copernican Question* a claim that he gratuitously dubs, "the thesis that Copernican practitioners were obsessed with astrology" (Heilbron 2012, p. 381). If there is such a "thesis," it is uniquely the product of Heilbron's imagination. His obsession with minor translation errors and illusory non-sequiturs, on the other hand, is real. The stringent standards that underlie that obsession are captured in a poignant memory: "The late Joseph Ben-David once told me that the day after he handed in his doctoral thesis he found his professor in the library checking the footnotes. We have fallen far from this level of oversight. We are often lax

25. But not without his customary reproaches (see Swerdlow 2012a, pp. 376–77).

about the extent and quality of the evidence we require to underpin historical arguments" (Heilbron 2012, p. 385).[26] Later, one learns that this regrettable decline in standards has spread like an epidemic, affecting the "laxer and less critical profession of recent years" (Heilbron 2012, p. 388). Alas, even the harshest idealization of the doctoral advisor must confront the messy reality of human imperfection. A limited sampling of translations in Heilbron's recent book, *Galileo,* quickly yields a small harvest of slip-ups (Heilbron 2010, pp. 110–114).

1. In Galileo's earliest known reference to Copernicus (May,1597) he refers to "the opinions of Pythagoras and Copernicus on the place and movement of the earth," which he tells his correspondent Jacopo Mazzoni, "I held to be *much* more probable than the opinion of Aristotle and Ptolemy." Leaving out the word "much," Heilbron weakens Galileo's declaration to read: "I held to be more probable" (Heilbron 2010, p. 110).[27]

2. Kepler's reply to Galileo's acknowledgement of receipt of the *Mysterium cosmographicum* (October 13, 1597): "I was very pleased to receive yours of 4 August [1597] . . . because of our agreement about Copernican cosmography." Heilbron replaces Kepler's word "cosmography" with "cosmology," a term that neither he nor Galileo ever used in their correspondence (Heilbron 2010, p. 113; Galilei 1890–1909, X, p. 69).[28]

3. Kepler's joy in the *Mysterium* at discovering the fit between the five regular polyhedra and the spaces between the planetary orbs. Using Alain Segonds' excellent French translation of Kepler's Latin, Heilbron has Kepler say, "I spent days and nights computing until I could see if my opinion . . . agreed with the *orbits* of Copernicus or if my joy would dissipate in the winds," but he replaces Segonds' *orbes* (Fr.) and Kepler's *orbibus* (Lat.) with "orbit," a word—and, more importantly, a concept—that Kepler had not yet conceived (Heilbron 2010, p. 114; Kepler 1984, p. 26; Kepler 1937–, I, p. 13; Goldstein and Hon 2005).[29]

The ghost of Ben-David's doctoral advisor must be frowning and shaking his finger. But do these blemishes support the inference that *all* of Heilbron's translations are suspect? Should they have been called to his attention by the two eminent scholars who provided the glowing blurbs for the dust jacket of *Galileo?* Should they have been listed by the *Isis* essay reviewer? Or, perhaps kindly pointed out beforehand by the people he thanks in his acknowledgements? Could they have been missed by the

26. Ben-David's advisor was the late Shmuel Noah Eisenstadt of the Hebrew University, Jerusalem.

27. "*assai* più probabile dell'altra di Aristotile e di Tolomeo" (Galilei 1890–1909, II, p. 198).

28. For discussion of the meaning and usages of "cosmology," see CQ, pp. 420–22.

29. No entry for Kepler's *Gesammelte Werke* appears in Heilbron's bibliography.

readers for Oxford University Press or did Heilbron not follow their advice? Regrettably, these are exactly the kinds of noisome questions Heilbron raises in his review of *The Copernican Question.*[30] But surely the important consideration is whether such peccadilloes as he identifies have any serious bearing on my central theses or middle-range interpretations. The answer is no.

Here are the main issues raised by Heilbron, with comments, as necessary, on matters of translation, alleged non-sequiturs and other matters.

(1) Based on Galileo's important letters of May and August 1597 to Mazzoni and Kepler, respectively, both Heilbron and I agree that Galileo held Copernican sympathies for some years before that date. In the second letter, rather than have Galileo say "I venerated *(venerim)* the opinion of Copernicus," I accept Heilbron's correction: "I adopted."[31] However, Heilbron fails to comment on my interpretation of the relationship between the two letters that follows the quite long quotation in which the mistranslation appears: "Galileo's position in this letter was stronger and more specific than in the previous one to Mazzoni. Rather than invoke the phrase 'much more probable,' he spoke of 'the causes of many natural effects' and 'many reasons as well as refutations of contrary arguments'" (CQ, p. 358). Contrast Heilbron's scant comparison between the two letters: "A few months after this private assistance to heliocentrism [Mazzoni's letter], Galileo received a challenge to declare himself publicly [Kepler's letter]" (Heilbron 2010, p. 112).

(2) A second point of contention is how early and under what circumstances Galileo learned the principles of theoretical and practical astrology. Filippo Fantoni was the lecturer in mathematics during Galileo's student days in Pisa (1580–85) and in 1589 Galileo succeeded him in that post. Fantoni explicitly defended astrological theory against Pico's arguments and, if Galileo was in attendance, his education in the *Tetrabiblos* would have been framed explicitly by the Piconian debate. But, in 1586,

30. Heilbron 2012, p. 385: "Did no one . . . point out the errors? Westman thanks twenty colleagues and students for careful reading of drafts of *The Copernican Question* and perceptive comments on them. He mentions further that he has tested his ideas in lectures and seminars all over Europe and the United States, some twenty-one times between 1993 and 2010 (CQ, pp. xvib–xviia). Did no one raise serious objections or did he not listen? Did the referees of the University of California Press not do their duty or were they ignored? The endorsements quoted at the outset of this review indicate that their authors would not have been likely to furnish the criticism that might have saved the book."

31. "I adopted [came to] Copernicus's opinion many years ago" (Heilbron 2012, pp. 380–1).

Pope Sixtus V issued a Piconian-tinged bull against divination, *Coeli et terrae.* As far as astrologers were concerned, it was mainly intended to prevent them from predicting the death of a pope while largely exempting them from worries in the domains of medicine, navigation and weather forecasting. Heilbron entirely neglects these developments but my inclusion of them explains why his Galileo could teach astrology to medical students at Pisa between 1589 and 1592 and then again during his time at Padua from 1592 onward (Heilbron 2010, pp. 48, 68).[32] Nowhere do I suggest that Galileo adopted Copernicus's view because it would "improve astrological forecasting."[33]

(3) Heilbron misses the distinction between the *kinds* of astrologies Galileo and Kepler practiced and lumps together theoretical astrology (categories and relations of possible causes, influences and effects, described in the *Tetrabiblos*) and practical astrology (the prediction of particular effects at specific times).[34] Galileo and Kepler were both cognizant of the Piconian arguments as early as their student days but Galileo composed nativities for private clients and for his daughters; Kepler issued public prognostications for an entire region and also wrote a treatise on theoretical astrology (CQ, pp. 486b–487a).

(4) Heilbron is also concerned with my treatment of the local circumstances of Galileo's run-in with the Holy Office in 1604. My discussion is framed in the larger context of the political atmosphere in Italy between 1597 and 1604. It goes as follows: Having acknowledged his Copernican sympathies to Kepler in 1597, Galileo then resisted Kepler's earnest entreaties to join him in a campaign to convert other mathematical practitioners to the Copernican cause and instead cut off all further correspondence with Kepler until 1610. Yet, contrary to Stillman Drake's influential reading, Galileo's Copernican commitments did not cease. I show

32. Had Heilbron paid attention to my discussion of Fantoni (CQ, p. 354), he would not have detected a "thought-stretching non-sequitur" concerning the early Galileo's involvement in astrological practice (CQ, p. 376b). Nonetheless, his translation corrections concerning Pagnoni's testimony before the inquisition are apt (Heilbron 2012, pp. 381–82).

33. Heilbron 2012, p. 382: "there is no reason to think that he venerated Copernicus' opinion because it might improve astrological forecasting. Nor does Westman assert such a connection." If there is no assertion, what needs to be corrected?

34. Heilbron 2012, p. 382: "He [Westman] rightly says that Galileo's astrological practice was old-fashioned. Indeed, the prognostications that Galileo made about the characters of his infant daughters came straight from Ptolemy's *Tetrabiblos.* Galileo therefore would seem to lie outside Westman's orbit. The mistakes made by not leaving him there are quite unnecessary self-inflicted injuries."

that Galileo and Kepler continued to track one another at least until 1603 through the covert mediation of an otherwise obscure Englishman, Edmund Bruce (CQ, pp. 362b–366; Bucciantini 2003, pp. 93–116). However, the execution of Giordano Bruno in 1600 and, most importantly, the placement of all his writings in the most severe category of prohibition on the 1603 Index decisively froze the intellectual atmosphere. The 1603 decree surely explains why Galileo was very careful not to mention Copernicus or Bruno and why it would have reinforced his silence with regard to Kepler (CQ, pp. 366b–368a, 375b).[35] Thus, when Galileo was denounced to the Holy Office in April 1604 by the amanuensis who had once lived in his house, the charges concerned astrological fatalism and rumors that he was visiting his mistress rather than attending mass. The main point of my discussion, unacknowledged by Heilbron, is that Galileo's 1604 encounter with the Holy Office had nothing to do with his ongoing Copernican sympathies, his relationship with Kepler, his studies of motion or any reference to Bruno (CQ, p. 376).[36]

(5) Patronage is a major theme in *The Copernican Question.* It is an important meeting point of the history and sociology of scientific knowledge. I engage the theme of patronage partly through Richard S. Westfall and Mario Biagioli's quite different studies of Galileo, considering such questions as: "What weight ought to be assigned to the demands and opportunities of patronage in shaping Galileo's judgments, his beliefs about the heavens, and the ways he should frame his ideas, as well as which he should push forward and which not?" (CQ, p. 436b) Did great patrons, like King James I, remain noncommittal in philosophical controversies? Could prospective clients expect patrons to read their books? (CQ, chap. 15)

Unlike Swerdlow, who entirely disallows questions of this sort, Heilbron allows the questions but strips out Biagioli's socio-historical generalizations about court power dynamics.[37] In turn, Heilbron's *Galileo* retains

35. Compare Heilbron's glib summary: "There were many good reasons that Galileo did not take up the role of Copernican agent that Kepler assigned him. He was an untenured foreigner (a Tuscan in Venice) obliged to teach Ptolemaic astronomy in Catholic Italy. Why should he risk compromising himself by making common cause with an unknown crackpot Protestant schoolteacher? Westman traces Galileo's silence to the continuing threat of the Roman censorship. No doubt that became palpable later, after Bruno's horrible death, which warned prudent astronomers to keep away from heretical celestial practitioners" (Heilbron 2012, p. 381).

36. Heilbron's corrections neither mention nor affect this claim. They bear only on an aside, that the amanuensis' testimony "also sheds light on the ways family tensions could become a basis for inquisitorial mischief" (Heilbron 2012, p. 382).

37. In a rare show of agreement, Heilbron concedes that "Biagioli's main thesis, which

a simplified version of Galileo-courtier, sanitized of all sociological theorizing and perhaps made more suitable for combating the tedium of long airplane flights: "From a lowly professor he had risen to a high-class jester, expected to help relieve the dull and punctilious court routine by producing an occasional wonder" (Heilbron 2012, p. 164).[38] Having watered down Biagioli's generalizations, Heilbron cannot leave well-enough alone. He struggles to defend one of Biagioli's most important claims about Galileo's patronage tactics by criticizing my approving citation of Michael Shank's critique of Biagioli's thesis: "It is one thing to challenge an interpretation," writes Heilbron, "and another to accuse the interpreter of having 'overlooked or misread historical sources both crucial and inconvenient to his argument'" (Heilbron 2012, p. 386; CQ, p. 596, n26)."[39]

Commendably, Biagioli aspired to bring art history into serious engagement with an understanding of the tactics of court patronage. His thesis concerned an association of Jupiter with the Medici *dynasty,* dating to the reign of the dynasty's founder Cosimo I (1519–74) and still prevailing when his grandson Cosimo II (1590–1621) took the throne in 1609, the year before Galileo published his *Sidereus nuncius.* These dynastic meanings were supposed to be "fully apparent to a Florentine audience" in the court palace's exact alignment of mythological decorations in the upstairs rooms and specific Medici rulers in the downstairs rooms: "The correspondence between the room of Jupiter and that of Cosimo I is the pivot for the mythological narratives developed throughout the paintings of the two apartments" (Biagioli 1993, p. 110). Galileo was "somehow midwife to this astrologico-dynastic encounter" (Biagioli 1993, p. 110). Thus, Biagioli argued: "While Galileo could have dedicated the newly discovered planets to any patron, the Medici were in the position to fully appreciate (and reward) the mythological significance of Galileo's discoveries" (Biagioli 1993, p. 106).[40]

But Shank showed convincingly that there was no evidence for an association of Jupiter with the Medici dynasty nor any evidence that any such association was generally known and appreciated in Florence (Shank

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attributes much of Galileo's science after 1609 to choices he made, or had to make, to maintain himself as a favorite of Cosimo II, conflicts with the wider range of sociabilities Westman assigns to heavenly practitioners. He is right that the restriction of motives to considerations of court dynamics not only leaves too little room for historical explanation but also demotes 'heavenly practitioners to the subsidiary role of socially anxious performers and entertainers (CQ, p. 438a)" (Heilbron 2012, p. 386).

38. The note to this passage references Biagioli's account of court dynamics (Biagioli 1993, pp. 120–38).

39. Presumably, it is acceptable to challenge interpretations on the basis of evidence.

40. Shank cites the full passage (Shank 1996, p. 116).

1996). Moreover, Saturn and Capricorn were Grand Duke Cosimo I's important astrological referents, as was readily established in one of Biagioli's major secondary sources (Cox-Rearick 1984, p. 3ff.). Thus, invoking a Jupiter-dynastic connection could not have been one of Galileo's patronage tactics. To counter Shank's arguments, Heilbron again strongly attenuates one of Biagioli's most interesting theses: "What counted was whether, when Galileo dedicated *Sidereus nuncius* to Cosimo II, anyone remembered or cared about the iconography of the old palace. (In any case, comparing a ruling prince to Jupiter scarcely needed . . . elaborate justification. . . .)" (Heilbron 2012, p. 386).

In chapter 15, I take up a related claim advanced by Biagioli: that a patron's honor could be defiled if, as in a duel or a joust, his knight lost the contest—hence, the patron had to maintain a distantiated, noncommittal stance. The case of Kepler and King James I does not lend support. When Kepler sent James a copy of his *De stella nova* (1606), his accompanying letter directly involved the king, communicating his respect for His Majesty's learning by recommending specific chapters for study or perusal. They included chapters 2–6, where Kepler endorsed elements of Pico's critique of astrology (with which the king would likely agree), but also other chapters and a diagram in which Kepler produced arguments in support of an astrology based on his own archetypal harmonies (with which the king probably did not agree). The ever-optimistic Kepler never received a direct response from the king just as an earlier attempt to win Galileo's support for a Copernican crusade met with silence.[41] Heilbron does not challenge my discussion of Kepler's letter and the *Stella Nova* or its implications for Biagioli's thesis but chooses to foreground some minor difficulties with my translation of the poem that Kepler inscribed to the king.

(6) A final point of contention, worthy of comment, concerns Galileo's efforts to defend the existence of Jupiter's four moons (then described as "planets") at a public event in Bologna in April 1610. Martin Horky, secretary to G. A. Magini, the astronomer-astrologer at the University of Bologna, informed Kepler that if Galileo's claim was true, then the standard seven-planet ephemerides would require the addition of four more planets. Magini was eager for a collaboration with Kepler on a new ephemeris;

41. Heilbron misses the parallel between Kepler's two failed attempts: "He [Westman] asks in one of his arresting non-sequiturs, 'having failed abysmally, thus far, with Galileo, how could Kepler hope for the king's public endorsement?' . . . Whether [King] James was Diogenes or the Devil has nothing to do with the astrological origins of heliocentrism" (Heilbron 2012, p. 383). My discussion of Kepler and James I draws no connections with "the astrological origins of heliocentrism."

now, Galileo's four new planets might undermine the whole enterprise. On April 24, Galileo himself arrived in Bologna with one of his good telescopes. As he slept, however, Horky recounted to Kepler that he engaged in a deception: "I never slept, but I tested Galileo's instrument in innumerable ways as much on what is below as on what is above. On things below, it rendered marvels; in the heavens, it failed because what appeared to be stars were fixed stars doubly enlarged." Horky also confessed that he had secretly made for himself a wax impression of the lenses, bragging that he could make an even better instrument. "The representation of the scene," I write, "nicely evokes Tycho [Brahe]'s recounting of [the visiting servant Reimarus] Ursus's nocturnal sniffings around the diagrams in his library—except, in this instance, the report came from the offender himself" (CQ, p. 470a). The next evening, as related by Horky, Galileo's performance was deemed a failure. Horky reported to Kepler: "All confessed that the instrument deceived." But he went further, seeking to undermine Galileo's credibility by characterizing him as a "fable-telling celestial merchant" (CQ, 471) and, vividly representing his body as diseased: "His hair falling out . . . his skin covered with the pimples of the French disease; his skull attacked, his ravings finding lodging in his brain . . . his guts producing an unnatural tumor . . . ," etc. (CQ, pp. 471a–472b).

Heilbron has suggested some minor improvements to the full translation of Horky's description, which I accept, but, substantive disagreement still remains on what to make of the passage. Heilbron takes Horky's testimony at face value: "Horky reported what he saw. Galileo did suffer from most if not all the ills mentioned, and others besides." (Heilbron 2012, p. 384) In *Galileo,* Heilbron is prepared to embellish further: "Horky noticed delirium, arising from the French disease. That was probably a good hit; the Galileo-Sagredo life style almost guaranteed a dose of syphilis, whose symptoms can mimic those of other ailments" (Heilbron 2010, p. 162).[42] Perhaps Galileo did feel ill on that occasion, but, at a minimum, there is more than enough reason to discount Horky's description as exaggerated and even better reason to regard the whole description as a further attempt to undermine Galileo's credibility by deploying a trope of bodily disorder. Giovanni Antonio Roffeni, who was actually in attendance at the Bologna performance, later "criticized Horky for his inflated and bumptious language as a basis for mistrusting his objections. Indeed, Roffeni pointed out that some of Horky's language was not even original, as it came 'word for word' from Ursus's 1597 attack on Tycho!" (CQ, 476) Heilbron overlooks Roffeni's public rebuke of Horky in his review and fails to cite it in *Galileo.* But these omissions do not deter him from using

42. A "dose of syphilis"?

the Horky-Galileo episode to conclude that "Westman's book, big as it is, does not contain the sort of information he needs to elucidate Galileo's relationships with his contemporaries" (Heilbron 2012, p. 383).

Conclusion

The Copernican Question is not the book the reviewers would like me to have written. They would have preferred a conventional biography of Copernicus or perhaps a history of Renaissance astronomy and astrology. They have inflated minor issues of translation, omitted numerous vital references, and downplayed or overlooked actual points of mutual agreement. Their grossly imbalanced reviews amount to a futile attempt to undermine trust in the book's evidentiary credibility and thereby to divert attention from the scope of the argument and its real objectives.

"All looks yellow to the jaundiced eye." (Alexander Pope)

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