⌘ INTRODUCTION ⌘

Johannes Kepler's *Astronomia nova* (1609) has long been recognized as one of the canonical works of the Scientific Revolution. Between Copernicus's *De revolutionibus orbium coelestium* (1543) and Newton's *Philosophiae naturalis principia mathematica* (1687), it occupies a position of central importance in the development of astronomy during the sixteenth and seventeenth centuries. Its significance is twofold. In terms of astronomical theory, it signifies the beginning of the end for a millennia-old tradition of mathematical astronomy, in which the motions of the planets were represented using only compounds of uniform circular motion. Kepler's elliptical orbits and his area law (the first two of what later came to be called his three laws of planetary motion) subsequently became essential elements in the Newtonian theoretical synthesis that was the culmination of the Scientific Revolution. Kepler's achievement also presaged Newton's in a second, more fundamental way. In place of the ancient tradition of mathematical astronomy, Kepler substituted a physical approach to astronomy—"celestial physics," as he named it—in which theories of planetary motion were derived from the physical consideration of the cause of their motion. He was able to derive his first two laws of planetary motion from a flawed but self-consistent set of physical principles. The unification of physics and astronomy in which Kepler played a leading role represents the most important conceptual change in science during the period.

The conceptual importance of Kepler's methodology of physical astronomy has been described by historians of astronomy, beginning with Alexandre Koyré and more recently by Bruce Stephenson. Their task of analyzing the manifest role it played in his discoveries was made possible by another highly unusual feature of the *Astronomia nova*. Unlike the traditional literary models for astronomical treatises, such as Ptolemy's *Almagest* or Copernicus's *De revolutionibus*, in which the exposition of planetary theory proceeded deductively with few clues regarding the ways those theories came into being, the *Astronomia nova* was a narrative odyssey through Kepler's development of his astronomical theory. Kepler did not hesitate to discuss the series of false starts, blind alleys, and failures he encountered on his road to eventual success.

Recent research, especially that of William H. Donahue, has shown that the account Kepler offers his readers is not a true history of the course of his research—something Kepler never claimed—but is rather a didactic or rhetorical pseudohistory. But until now, the question of why Kepler chose this form of exposition has not been addressed. My work finds the

answer to this question in the context of the composition of the *Astronomia nova* and in Kepler's relation to the contemporary astronomical community. I argue that the unique conceptual and stylistic features of the *Astronomia nova* are intimately related: Kepler purposely chose this form of exposition precisely because of the response he knew to expect from the astronomical community to the revolutionary changes in astronomical methodology he was proposing.

This interpretation also resolves a broader tension in our view of Kepler's intellectual achievement. Throughout his life, Kepler's astronomical work was devoted to showing that the Copernican heliocentric system of the world was true. Yet some of his works are very different in character. His youthful *Mysterium cosmographicum* (1596) argued for heliocentrism on the basis of metaphysical, astronomical, astrological, numerological, and architectonic principles. By contrast, the *Astronomia nova* was far more tightly argued on the basis of only a few dynamical principles. The contrast in the works seems to embody a transition from Renaissance to early modern science; in Arthur Koestler's characterization, Kepler seems to have passed over a "watershed." However, Kepler did not subsequently abandon the broader approach of the *Mysterium cosmographicum*. Similar metaphysical arguments reappeared in his *Harmonice mundi* (1619), and he reissued the *Mysterium cosmographicum* in a second edition in 1621, in which he qualified only some of his youthful arguments. Given the persistence of these ideas in Kepler's work, it is clear that he himself did not experience some sort of conversion experience and become a modern scientist. We must ask instead how it was that the *Astronomia nova* in particular was written in the style it was. One of the conclusions of my work is that the *Astronomia nova* is only accidentally modern—that is, that the particular context in which the book was composed forced Kepler to rein in his broader arguments for heliocentrism, leaving only a subset of his physical reasoning that appears distinctly modern in retrospect.

Two interrelated questions arise from the fact that Kepler's *Astronomia nova* does not provide an entirely faithful account of his research on the theory of Mars nor of his broader approach to the physical truth of heliocentrism. First, if it is not a true account of his Mars research, how did Kepler actually proceed? Second, what was Kepler's motivation for presenting his findings in the form of a narrative, and for obscuring his broader conception of physical reasoning? I argue in my work that answers to both these questions can be found in the development of Kepler's research and his interaction with the astronomical community.

This book covers the evolution of Kepler's thought through the publication of the *Astronomia nova*. My argument is twofold. First, I establish the breadth of Kepler's notion of physical reasoning and the continuity of

research from the *Mysterium cosmographicum* to the *Astronomia nova*. I describe how the conditions of his work under Tycho Brahe strictly limited Kepler's research, but that it nevertheless proceeded along lines that came forth from the *Mysterium cosmographicum*. Second, I address the composition of the *Astronomia nova*. I argue that Kepler intentionally obscured the continuity between the *Mysterium cosmographicum* and the *Astronomia nova* in the face of the negative response his physical reformation of astronomical theory faced from within the astronomical community. And I show how his rhetorical narrative was meant to convince his readers of the necessity of his approach and to lead them through difficult and contentious material.

Part 1 covers the period from Kepler's days as Michael Maestlin's student at the University of Tübingen up until he began his research with Tycho Brahe. In chapter 1, I introduce the prevailing attitude among astronomers to Copernicus's work and show how Kepler deviated from it. Although we regard Copernicus's work as significant for putting forward the idea that the earth travels around the sun, sixteenth-century astronomers largely ignored that claim, which violated Aristotelian physics and apparently contradicted the testimony of Holy Scripture. Instead, they were attracted by Copernicus's novel form of mathematical planetary theory, which eliminated Ptolemy's equant, a mechanism that caused the center of a planet's epicycle to travel nonuniformly around its eccentric and thus violated the precept that planetary theories should be composed of compounds of uniform circular motion.[1]

In Kepler's earliest writing on Copernicus, a fragment of a student disputation from 1593, he ignored conventional astronomers' interpretation of heliocentrism and disregarded Copernicus's detailed mathematical arguments. Instead, and apparently in the face of resistance from his audience, Kepler argued for the physical truth of heliocentrism on the basis of what he called "cosmographical" reasons. These were largely conventional metaphysical arguments for heliocentrism, taken from either Copernicus or Rheticus's *Narratio prima* (1540), but Kepler also introduced one highly significant innovation. He expanded a conventional claim that the sun was the source of all heat, light, and motion in the solar system to suggest that one might derive the planets' periods from their distances from the sun, the source of their motive power. Copernicus had noted the correlation but had never quantified it.

In chapter 2, I recount how Kepler, after having been forced to leave seminary to assume the position of mathematics teacher at the Protestant school in Graz, returned to the ideas of his student disputation and to his defense of Copernicus based on physical reasoning. He sought to redirect his religious aspirations into astronomy by arguing that the heliocentric

system of the world made plain the glory of God in His creation of the world. Thus he made the establishment of the physical truth of heliocentrism a religious vocation.

To the problem of accounting mathematically for the relationship between the planets' distances and their periods, Kepler now added the questions of accounting for the number of planets and their particular distances from the sun. He promptly hit upon an explanation for the latter problems. In his "polyhedral hypothesis," he reasoned that God had used the five perfect Platonic solids as archetypes when constructing the solar system. By interpolating the five solids between inscribed and circumscribed spheres, Kepler was able to derive values for their distances and to provide an explanation for the number of planets.

In addition, Kepler began to develop the notion he had expressed in his student disputation into a quantitative "motive force" hypothesis relating the planets' periods and distances. Reasoning that planets' periods increase with distance both because the planet-moving force is weaker and because the circumference of their orbits are longer, he combined the effects to come up with an expression for the relationship between the planets' distances and periods that was somewhat less accurate than the polyhedral hypothesis.

The polyhedral hypothesis became the centerpiece of Kepler's first book, the *Mysterium cosmographicum* (1596), which I discuss in chapter 3. The polyhedral hypothesis proved to be a very fertile source of ideas, and Kepler buttressed the argument with numerous auxiliary arguments based on the astrological, numerological, and metaphysical appropriateness of the arrangement he was proposing. Kepler considered all of these to be elements of his "physical" argument for Copernicus. In the preface, he refers to the arguments of his student disputation as "physical, or if you prefer, metaphysical." His conception of what constituted physical arguments corresponded roughly to Aristotelian causes, and especially to the formal cause of the world.

Although arguments of formal cause based on the polyhedral hypothesis had swelled to constitute the bulk of the *Mysterium cosmographicum*, Kepler did not lose sight of the significance of the sun as the source of motion in the solar system, and he included an additional argument based on this motive force hypothesis toward the end of the book. In a highly significant application of the idea of motive force, he considered what effect the change in a planet's distance from the sun would have on its motion around its own orbit. He came to the conclusion that the physical motion of a planet around an eccentric orbit would be the same as that described in classical mathematical astronomy by either Ptolemy's bisected equant or Copernicus's eccentric epicyclet arrangement. He thus

concluded that both these theories were merely mathematical models for the physical motion whose cause he had described.

Kepler conceived his physical "proof" of the reality of heliocentrism in the *Mysterium cosmographicum* as an affirmation of faith. However, this aim of the book was subverted by prepublication censorship. In chapter 4, I describe how the theologians at the University of Tübingen arranged to suppress a chapter of the book intended to address the reconciliation of the Copernican system with Holy Scripture. In doing so, they urged Kepler to "play the part of the pure mathematician" and desist from arguing for the physical truth of heliocentrism. Their view that mathematics had no claim to physical truth reflected a common fictionalist stance toward the status of astronomical hypotheses, which Kepler endured at that moment but ultimately could not accept.

The response of the astronomical community toward the *Mysterium cosmographicum*, which I also describe in chapter 4, was mixed. On the one hand, there were those who embraced Kepler's finding that the dimensions of the solar system could be found from the inscribed polyhedra. Georg Limnaeus, for instance, lavishly praised Kepler for reviving the *prisca philosophia* of the ancients. And Michael Maestlin even suggested that the polyhedral hypothesis could be used to derive better values for the planetary distances than could be found from observation. Even Tycho Brahe said that Kepler's scheme was ingenious, in spite of the fact that some expected Tycho to take the leading role in refuting Kepler's pro-Copernican argument.

However, there was one point to which astronomers reacted uniformly negatively: they all agreed that Kepler's attempt to account for the function of the equant on the basis of his planet-moving force was ill-conceived. They considered it inappropriate—even dangerous—to apply physical reasoning to mathematical planetary theory. I argue that the distinction between the fairly positive reaction to the book as a whole versus the critical reaction to Kepler's explanation of the equant was based on a rigid division within astronomy between cosmography and planetary theory. The former addressed broader questions about the form of the world and was closely allied to physics; thus Kepler's physical arguments were acceptable. The latter, however, was considered part of mathematics and did not admit physical reasoning. Thus to the mathematical astronomer Johannes Praetorius, Kepler's work was more aligned to physics, and "cannot be of use to the astronomer in almost any way."

The *Mysterium cosmographicum* had the fateful consequence of bringing Kepler into contact with Tycho Brahe. In part 2, I cover the period from Kepler's collaboration with Tycho Brahe until the publication of the *Astronomia nova*. During this time, Kepler's qualitative explanation of planetary motion based on his planet-moving force acquired a quantita-

tive exactness. With the help of Tycho's unprecedentedly accurate observations, his earlier physical insight led him to his first two laws of planetary motion. During the same period, he also became definitively aware of the resistance this new kind of physical astronomy would face.

The portentous encounter between Tycho Brahe, the aged observer, and Johannes Kepler, the young theorist, is so convenient that it can seem inevitable. In chapter 6, I try to take an unbiased view of their collaboration in the light of recent scholarship that has suggested that Kepler was more desirable to Tycho as a pawn in his legal struggle with Nicholas Reimers Ursus than as an assistant. From the terms of their agreement, Kepler does not seem to have occupied a particularly favored position in his first few months with Tycho, but was probably rather low in the hierarchy of assistants. Nor does Tycho appear to have overseen his work too closely.

Despite Kepler's express hope of receiving from Tycho improved values for the planetary distances with which to test and improve the polyhedral hypothesis, Tycho would not provide this information. Instead, he assigned Kepler to work on the theory of Mars and gave him observations for just that planet. Despite being barred from developing the primary argument from the *Mysterium cosmographicum*, Kepler could still pursue his motive force hypothesis. And during his first few months with Tycho, Kepler experienced some remarkable successes in his research with Mars. First, he discovered that the theory of Mars seemed to require being referred to the true sun—the source of its motion, to Kepler—rather than the center of the earth's orbit (the "mean sun"), as Copernicus and Tycho had done. Second, he discovered that the eccentricity in the theory of the earth needed to be bisected, just as Ptolemy had bisected the eccentricities in the theories of the planets. Ever since the time of Hipparchus, up to and including Tycho's successful solar theory, the earth had always been assigned a simple, unbisected eccentricity. But to Kepler, the earth's simple eccentricity had been an unsatisfying qualification in the motive force hypothesis in the *Mysterium cosmographicum*, for it had not been amenable to Kepler's explanation in terms of the planet-moving force. In addition to bringing the theory of the earth into line with the theories of all the other planets, the bisection of the earth's eccentricity also eliminated an annual variation in Mars's eccentricity that Tycho had raised as an objection to the planet-moving force hypothesis.

The bisection of the earth's eccentricity later became an important element of the argument of the *Astronomia nova*, where it was presented in part 3 and provided the justification for Kepler to change from a purely mathematical to a physical approach to finding Mars's orbit. But at the time Kepler found it, he had not yet completed the research presented in part 2. Moreover, the continuity between the *Mysterium cosmographi-*

cum and Kepler's Mars research makes it clear that he had pursued a physical approach to planetary theory from the beginning of his collaboration with Tycho.

A clue to Kepler's reorganization of the account of his research comes from Tycho's reaction to Kepler's resort to natural (physical) principles. The available evidence shows that Tycho objected "vehemently" to this kind of research. In chapter 7, I show how the direction of Kepler's research after Tycho's death, though always motivated by the physics of the planetary orbit, took an abruptly more physical turn, as he began for the first time to employ a version of his area law and to experiment with oval orbits.

Under the circumstances prevailing just after Tycho's death, the *Astronomia nova* would probably never have been published at all. In chapter 8, I explain how a struggle between Kepler and Tycho's heirs over the right to profit on Tycho's astronomical inheritance led to Kepler's losing responsibility for the *Rudolphine Tables*. At the same time, he was ordered to name what works he would produce to justify his recent appointment as imperial mathematician. Placed in this bind, Kepler named as one of the works he would produce his *Commentaries on Mars*—that is, the *Astronomia nova*. He was thus forced to conceive the book as a preliminary announcement of the fruits of his physical astronomy as applied to the orbit of Mars. It would contain his important finding regarding the bisection of the earth's eccentricity, which vindicated his physical account of the cause of the equant as well as clearing up certain problems in the orbit of Mars (and the orbits of Mercury and Venus as well). At that time, however, he had no clear idea of what the eventual solution to Mars's orbit would be. Although he was employing a form of the area law, the discovery of Mars's elliptical orbit was still two and a half years away.

Kepler's struggle with Tycho's heirs also led to Tycho's son-in-law, Franz Tengnagel, gaining the right to censor any of Kepler's work based on Tycho's observations, and this outcome had serious consequences for the composition of the *Astronomia nova*. Tengnagel did indeed censor some of Kepler's work because it strayed too far from Tycho's intention. And when Kepler received letters of criticism from Christian Severin Longomontanus, Tycho's longtime chief assistant, imploring him to give up his ill-conceived program of physical astronomy, Kepler had reason to fear that a conspiracy among Tycho's legal and scientific heirs—whom he called "the Tychonics"—might threaten his philosophical freedom. In response, he justified the course of his research on the theory of Mars in a long letter to Longomontanus, whose rhetorical narrative is similar to the argument of the *Astronomia nova*. I argue that pressure from the Tychonics, including ridicule of the *Mysterium cosmographicum* from

Longomontanus, influenced Kepler to restrict the range of his physical arguments to only those dynamical arguments that were essential for the *Astronomia nova*.

At the same time as his problems with the Tychonics were developing, Kepler learned that even a sympathetic friend and correspondent could raise serious objections to his work. In chapter 9, I describe how Kepler's correspondence with David Fabricius, an East Frisian pastor and amateur astronomer, acted as a kind of peer review for the *Astronomia nova*. In a friendly and forthright manner, Fabricius demanded certain demonstrations in order for Kepler win his assent to the radical innovations he was proposing in the theories of the earth and Mars. I demonstrate how Fabricius's queries formed the framework for numerous specific chapters in the *Astronomia nova*.

As the complexity of Kepler's Mars work increased, Fabricius became more and more skeptical about the course of Kepler's research and began to offer him alternative theories of Mars based on compounds of uniform circular motion in the classical style. When the third of these reproduced an ellipse that seemed to differ insensibly from Kepler's own theory, Kepler viciously attacked it and broke off their correspondence. The threat to the argument of the *Astronomia nova* was clear. Kepler was arguing on the basis of the fact that only by the use of physical reasoning had he arrived at the correct solution of Mars's orbit. His solution of the problem of Mars's orbit would justify physical astronomy and, consequently, also the physical truth of the heliocentric system. He knew the argument could not succeed if an alternative in the classical instrumentalist form were available.

In the concluding chapter 10, I offer a reading of the *Astronomia nova* as rhetoric. I show how the argument of the book was a response to the various criticisms he had encountered during the course of his research. To the charge that his physical astronomy was an unjustified aberration, he responded by constructing his argument to make it appear as though he resorted to a physical approach to planetary theory only after a comprehensive failure of the most general kind of model in the classical form (which he presented in part 2, even though he actually completed the research only after parts of the research presented in part 3). He countered the charge that his radical innovations were themselves the source of the difficulties he had encountered by repeating many of the demonstrations in the book (as with the repeated demonstrations involving the true and the mean sun). And in order to justify his unprecedented innovation of bisecting the earth's eccentricity, he offered numerous redundant demonstrations.

Moreover, I argue that many of Kepler's failed attempts served a valuable didactic function. For instance, the faulty orbit of Mars called the

via buccosa was the result of Kepler's mistaken construction of Mars's position on the ellipse. His experience with Fabricius had shown that when he omitted the explanation as to why the construction did not work, Fabricius was suspicious of the complexity of the true construction. Thus, many features of the *Astronomia nova* become comprehensible only when they are viewed in the context of Kepler's experience in writing the book as elements of an elaborate and purposefully-constructed rhetorical argument.

This work analyzes Kepler's composition of the *Astronomia nova* in a detail never attempted before. By viewing the account of his research that Kepler offers in his published work with skepticism and by attempting to reconstruct the actual course of his research from contemporary sources, it adds a new depth to our appreciation of this canonical text. In particular, it establishes the meaning of this text within the context of late sixteenth- and early seventeenth-century astronomy and against the backdrop of Kepler's contemporaries' view of his work.

When viewed in context, the meaning of the *Astronomia nova* becomes clear. By observing the persistence with which Kepler pursued long-held convictions deep into the investigation of Mars's orbit and emerged triumphant, we understand what in the book was important to him. By examining how the situation in which he found himself after Tycho's death determined this announcement of his results, we understand why it was written. Most important, by knowing the suspicion with which Kepler's introduction of physics into astronomy was viewed and the incomprehension his work elicited, we can at last explain its curious structure.